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Helmholtz's Treatise

on

PHYSIOLOGICAL OPTICS

Translated from the Third German Edition

Edited by

James P. C. Southall

Formerly Professor of Physics in Columbia University

complete in three volumes bound as two

Volume III

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Volume III

Handbuch
der
Physiologischen Optik

von

H. von Helmholtz.

Dritte Auflage

ergänzt und herausgegeben in Gemeinschaft mit

Prof. Dr. A. Gullstrand und **Prof. Dr. J. von Kries**
Upsala Freiburg

von

Professor Dr. W. Nagel
Rostock

Dritter Band

Mit 81 Abbildungen im Text, 6 Tafeln
und einem Porträt von H. von Helmholtz

Die Lehre von den Gesichtswahrnehmungen
herausgegeben von Prof. Dr. **J. von Kries**

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EDITOR'S NOTE

This last volume comprising the third division of the Treatise on Physiological Optics is much larger than either of the other two volumes, although except for two brief Notes by Professor v. KRIES, which have been inserted at the end of Chapters 30 and 32, the text of the English Translation contains no new additions. While the Bibliography of recent literature which has been appended at the end of the volume does not pretend to be complete by any means, at least it will afford some idea of the vast amount of contemporary research and speculation on the subject of light and vision in all its manifold aspects and practical bearings; and taken in conjunction with similar lists given in Volumes I and II and the occasional references to new literature in the added footnotes in all three volumes, it should be useful in enabling the student to obtain some clues to the particular developments of those questions in which he happens to be mainly interested. The Index to all three volumes which is included also at the end of this volume is another addition that will increase the usefulness of the treatise as a book of reference. For many of the corrections given in the list of "Corrigenda in Volume II" I am especially indebted to Professor FRANK ALLEN and Professor E. J. WALL.

When one considers the comparatively limited means by which the sense of sight enables us to form more or less accurate conclusions about the nature and configuration of the various objects that are exposed to view in the visual field, and the amount of information that is conveyed to us in this way, the clearness and precision of the so-called Perceptions of Vision, which is the subject treated in this third volume, is indeed little short of marvellous. Here we are concerned not so much with physiological as with psychological optics; and here also fundamental philosophical speculations are bound to arise, which have been, and perhaps always will be, the subject of much controversy. Owing to the metaphysical nature of many of the questions that come up here for discussion, and doubtless owing also to my own limitations, the translation of this volume has been far more difficult than that of either of the other two volumes. I have sometimes doubted whether I had succeeded in expressing the precise shade of meaning that the writer wished to convey, conscientiously as I have striven to do so. However, any faults of this kind certainly cannot be attributed to lack of competent editorial assistance, as the enumeration of the following list of collaborators will suffice to show:

Dean R. P. ANGIER, Yale University (Chapters 6, 7 and 8 of v. KRIES's Appendix I); Dr. H. S. GRADLE, Chicago, Ill., (§§27 and 28); Prof. WILLIAM

Kunerth, Iowa State College (§§26 and 29); Prof. Jakob Kunz (and also Prof. Elmer Culler), University of Illinois (§§32 and 33, together with v. Kries's Notes on §31, and Chapters 1, 2, 3, 4 and 5 of v. Kries's Appendix I); Adolph Lomb, Esq., and H. C. Lomb, Esq., New York City, (two new Notes by v. Kries at the end of §§30 and 32); Dr. G. W. Moffitt, Frankford Arsenal, (v. Kries's Appendix II); Prof. L. D. Weld, Coe College (§31); and Prof. W. Weniger, Oregon State Agricultural College (§30).

I am particularly grateful also to Professor E. J. Wall for helping me with the proof-reading and to Miss Ruth Townsend for transcribing the entire manuscript with the most painstaking fidelity. Professor Wall and Mr. C. A. Peerenboom have both assisted me in compiling the Index of Authors.

I wish I could pay an adequate tribute to Mr. Adolph Lomb for his unfailing support and encouragement throughout. His noble and single-minded devotion to the advancement of science for the welfare of mankind is so genuine and unstinted that I know he will consider himself amply rewarded if the usefulness of Helmholtz's work is continued and extended by this English edition of the Treatise on Physiological Optics.

James P. C. Southall

Department of Physics,
Columbia University,
New York City, N. Y.
May 1, 1925.

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PART THIRD

The Theory of the Perceptions of Vision

§26. Concerning the Perceptions in General

The sensations aroused by light in the nervous mechanism of vision enable us to form conceptions as to the existence, form and position of external objects. These ideas are called *visual perceptions*. In this third subdivision of Physiological Optics we must try to analyze the scientific results which we have obtained concerning the conditions which give rise to visual perceptions.

Perceptions of external objects being therefore of the nature of ideas, and ideas themselves being invariably activities of our psychic energy, perceptions also can only be the result of psychic energy. Accordingly, strictly speaking, the theory of perceptions belongs properly in the domain of psychology. This is particularly true with respect to the mode of the mental activities in the case of the perceptions and with respect to the determination of their laws. Yet even here there is a wide field of investigation in both physics and physiology, inasmuch as we have to determine, scientifically as far as possible, what special properties of the physical stimulus and of the physiological stimulation are responsible for the formation of this or that particular idea as to the nature of the external objects perceived. In this part of the subject, therefore, we shall have to investigate the special properties of the retinal images, muscular sensations, etc., that are concerned in the perception of a definite position of the observed object, not only as to its direction but as to its distance; how the perception of the form of a body of three dimensions depends on certain peculiarities of the images; and under what circumstances it will appear single or double as seen by both eyes, etc. Thus, our main purpose will be simply to investigate the material of sensation whereby we are enabled to form ideas, in those relations that are important for the perceptions obtained from them. This problem can be solved entirely by scientific methods. At the same time, we cannot avoid referring to psychic activities and the laws that govern them, as far as they are concerned with the perception of the senses. But the discovery and description of these psychic activities will not be regarded as an essential part of our present task, because then we might run the risk of losing our hold of established facts and of not adhering steadily to a method founded on clear, well-recognized principals. Thus, for the present at least, I[1] think the psychological domain of the physiology

[1] ¶In this volume (contrary to the usage adopted in the two previous volumes of the English translation), the editor has deemed it best to retain the more intimate language of the original text, and let the author speak throughout in the first person. (J.P.C.S.)

of the senses should be kept separate from pure psychology, whose province really is to establish as far as possible the laws and nature of the processes of the mind.

Still we cannot altogether avoid speaking of the mental processes that are active in the sense-perceptions, if we wish to see clearly the connection between the phenomena and to arrange the facts in their proper relation to one another. And hence, to prevent any misconception of the plan I have in mind, I intend to devote the latter part of this chapter to a discussion of the conclusions which I think can be inferred with respect to these mental processes. And yet we know by experience that people very seldom come to any agreement as to abstract questions of this nature. The keenest thinkers, philosophers like KANT for instance, have long ago analyzed these relations correctly and demonstrated them, and yet there is no permanent and general agreement about them among educated people. And, therefore, in the subsequent chapters devoted specially to the theory of the visual perceptions, I shall endeavour to avoid all reference to opinions as to mental activity, as involving questions that always have been, and perhaps always will be, subjects of debate between the various metaphysical schools; so as not to distract the reader's attention from those facts about which an agreement may possibly be reached, by wrangling over abstract propositions that are not necessarily involved in the problem before us.

Here I shall merely indicate at the outset certain general characteristics of the mental processes that are active in the sense-perceptions, because they will be constantly encountered in connection with the various subjects to be considered. Without some previous explanation of their general significance and wide range of activity, the reader might be apt in some special case to regard them as paradoxical and incredible.

The general rule determining the ideas of vision that are formed whenever an impression is made on the eye, with or without the aid of optical instruments, is that *such objects are always imagined as being present in the field of vision as would have to be there in order to produce the same impression on the nervous mechanism, the eyes being used under ordinary normal conditions.* To employ an illustration which has been mentioned before, suppose that the eyeball is mechanically stimulated at the outer corner of the eye. Then we imagine that we see an appearance of light in front of us somewhere in the direction of the bridge of the nose. Under ordinary conditions of vision, when our eyes are stimulated by light coming from outside, if the region of the retina in the outer corner of the eye is to be stimulated, the light actually has

to enter the eye from the direction of the bridge of the nose. Thus, in accordance with the above rule, in a case of this kind we substitute a luminous object at the place mentioned in the field of view, although as a matter of fact the mechanical stimulus does not act on the eye from in front of the field of view nor from the nasal side of the eye, but, on the contrary, is exerted on the outer surface of the eyeball and more from behind. The general validity of the above rule will be shown by many other instances that will appear in the following pages.

In the statement of this rule mention is made of the ordinary conditions of vision, when the visual organ is stimulated by light from outside; this outside light, coming from the opaque objects in its path that were the last to be encountered, and having reached the eye along rectilinear paths through an uninterrupted layer of air. This is what is meant here by the normal use of the organ of vision, and the justification for using this term is that this mode of stimulation occurs in such an enormous majority of cases that all other instances where the paths of the rays of light are altered by reflections or refractions, or in which the stimulations are not produced by external light, may be regarded as rare exceptions. This is because the retina in the fundus of the firm eyeball is almost completely protected from the actions of all other stimuli and is not easily accessible to anything but external light. When a person is in the habit of using an optical instrument and has become accustomed to it, for example, if he is used to wearing spectacles, to a certain extent he learns to interpret the visual images under these changed conditions.

Incidentally, the rule given above corresponds to a general characteristic of all sense-perceptions, and not simply to the sense of sight alone. For example, the stimulation of the tactile nerves in the enormous majority of cases is the result of influences that affect the terminal extensions of these nerves in the surface of the skin. It is only under exceptional circumstances that the nerve-stems can be stimulated by more powerful agencies. In accordance with the above rule, therefore, all stimulations of cutaneous nerves, even when they affect the stem or the nerve-centre itself, are perceived as occurring in the corresponding peripheral surface of the skin. The most remarkable and astonishing cases of illusions of this sort are those in which the peripheral area of this particular portion of the skin is actually no longer in existence, as, for example, in case of a person whose leg has been amputated. For a long time after the operation the patient frequently imagines he has vivid sensations in the foot that has been severed. He feels exactly the places that ache on one toe or the other. Of course, in a case of this sort the stimulation can affect only what is left of the stem of the nerve

whose fibres formerly terminated in the amputated toes. Usually, it is the end of the nerve in the scar that is stimulated by external pressure or by contraction of the scar tissue. Sometimes at night the sensations in the missing extremity get to be so vivid that the patient has to feel the place to be sure that his limb is actually gone.

Thus it happens, that when the modes of stimulation of the organs of sense are unusual, incorrect ideas of objects are apt to be formed; which used to be described, therefore, as *illusions of the senses*. Obviously, in these cases there is nothing wrong with the activity of the organ of sense and its corresponding nervous mechanism which produces the illusion. Both of them have to act according to the laws that govern their activity once for all. It is rather simply an illusion in the judgment of the material presented to the senses, resulting in a false idea of it.

The psychic activities that lead us to infer that there in front of us at a certain place there is a certain object of a certain character, are generally not conscious activities, but unconscious ones. In their result they are equivalent to a *conclusion*, to the extent that the observed action on our senses enables us to form an idea as to the possible cause of this action; although, as a matter of fact, it is invariably simply the nervous stimulations that are perceived directly, that is, the actions, but never the external objects themselves. But what seems to differentiate them from a conclusion, in the ordinary sense of that word, is that a conclusion is an act of conscious thought. An astronomer, for example, comes to real conscious conclusions of this sort, when he computes the positions of the stars in space, their distances, etc., from the perspective images he has had of them at various times and as they are seen from different parts of the orbit of the earth. His conclusions are based on a conscious knowledge of the laws of optics. In the ordinary acts of vision this knowledge of optics is lacking. Still it may be permissible to speak of the psychic acts of ordinary perception as *unconscious conclusions*, thereby making a distinction of some sort between them and the common so-called conscious conclusions. And while it is true that there has been, and probably always will be, a measure of doubt as to the similarity of the psychic activity in the two cases, there can be no doubt as to the similarity between the results of such unconscious conclusions and those of conscious conclusions.

These unconscious conclusions derived from sensation are equivalent in their consequences to the so-called *conclusions from analogy*. Inasmuch as in an overwhelming majority of cases, whenever the parts of the retina in the outer corner of the eye are stimulated, it has been

found to be due to external light coming into the eye from the direction of the bridge of the nose, the inference we make is that it is so in every new case whenever this part of the retina is stimulated; just as we assert that every single individual now living will die, because all previous experience has shown that all men who were formerly alive have died.

But, moreover, just because they are not free acts of conscious thought, these unconscious conclusions from analogy are irresistible, and the effect of them cannot be overcome by a better understanding of the real relations. It may be ever so clear how we get an idea of a luminous phenomenon in the field of vision when pressure is exerted on the eye; and yet we cannot get rid of the conviction that this appearance of light is actually there at the given place in the visual field; and we cannot seem to comprehend that there is a luminous phenomenon at the place where the retina is stimulated. It is the same way in case of all the images that we see in optical instruments.

On the other hand, there are numerous illustrations of fixed and inevitable associations of ideas due to frequent repetition, even when they have no natural connection, but are dependent merely on some conventional arrangement, as, for example, the connection between the written letters of a word and its sound and meaning. Still to many physiologists and psychologists the connection between the sensation and the conception of the object usually appears to be so rigid and obligatory that they are not much disposed to admit that, to a considerable extent at least, it depends on acquired experience, that is, on psychic activity. On the contrary, they have endeavoured to find some mechanical mode of origin for this connection through the agency of imaginary organic structures. With regard to this question, all those experiences are of much significance which show how the judgment of the senses may be modified by experience and by training derived under various circumstances, and may be adapted to the new conditions. Thus, persons may learn in some measure to utilize details of the sensation which otherwise would escape notice and not contribute to obtaining any idea of the object. On the other hand, too, this new habit may acquire such a hold that when the individual in question is back again in the old original normal state, he may be liable to illusions of the senses.

Facts like these show the widespread influence that experience, training and habit have on our perceptions. But how far their influence really does extend, it would perhaps be impossible to say precisely at present. Little enough is definitely known about infants and very young animals, and the interpretation of such observations as have been made on them is extremely doubtful. Besides, no one can say that

infants are entirely without experience and practice in tactile sensations and bodily movements. Accordingly, the rule given above has been stated in a form which does not anticipate the decision of this question. It merely expresses what the result is. And so it can be accepted even by those who have entirely different opinions as to the way ideas originate concerning objects in the external world.

Another general characteristic property of our sense-perceptions is, that *we are not in the habit of observing our sensations accurately, except as they are useful in enabling us to recognize external objects. On the contrary, we are wont to disregard all those parts of the sensations that are of no importance so far as external objects are concerned.* Thus in most cases some special assistance and training are needed in order to observe these latter subjective sensations. It might seem that nothing could be easier than to be conscious of one's own sensations; and yet experience shows that for the discovery of subjective sensations some special talent is needed, such as PURKINJE manifested in the highest degree; or else it is the result of accident or of theoretical speculation. For instance, the phenomena of the blind spot were discovered by MARIOTTE from theoretical considerations. Similarly, in the domain of hearing, I discovered the existence of those combination tones which I have called summation tones. In the great majority of cases, doubtless it was accident that revealed this or that subjective phenomenon to observers who happened to be particularly interested in such matters. It is only when subjective phenomena are so prominent as to interfere with the perception of things, that they attract everybody's attention. Once the phenomena have been discovered, it is generally easier for others to perceive them also, provided the proper precautions are taken for observing them, and the attention is concentrated on them. In many cases, however—for example, in the phenomena of the blind spot, or in the separation of the overtones and combination tones from the fundamental tones of musical sounds, etc.—such an intense concentration of attention is required that, even with the help of convenient external appliances, many persons are unable to perform the experiments. Even the after-images of bright objects are not perceived by most persons at first except under particularly favourable external conditions. It takes much more practice to see the fainter kinds of after-images. A common experience, illustrative of this sort of thing, is for a person who has some ocular trouble that impairs his vision to become suddenly aware of the so-called *mouches volantes* in his visual field, although the causes of this phenomenon have been there in the vitreous humor all his life. Yet now he will be firmly persuaded that these corpuscles have developed as the result of his

ocular ailment, although the truth simply is that, owing to his ailment, the patient has been paying more attention to visual phenomena. No doubt, also, there are cases where one eye has gradually become blind, and yet the patient has continued to go about for an indefinite time without noticing it, until he happened one day to close the good eye without closing the other, and so noticed the blindness of that eye.[1]

When a person's attention is directed for the first time to the double images in binocular vision, he is usually greatly astonished to think that he had never noticed them before, especially when he reflects that the only objects he has ever seen single were those few that happened at the moment to be about as far from his eyes as the point of fixation. The great majority of objects, comprising all those that were farther or nearer than this point, were all seen double.

Accordingly, the first thing we have to learn is to pay heed to our individual sensations. Ordinarily we do so merely in case of those sensations that enable us to find out about the world around us. In the ordinary affairs of life the sensations have no other importance for us. Subjective sensations are of interest chiefly for scientific investigations only. If they happen to be noticed in the ordinary activity of the senses, they merely distract the attention. Thus while we may attain an extraordinary degree of delicacy and precision in objective observation, we not only fail to do so in subjective observations, but indeed we acquire the faculty in large measure of overlooking them and of forming our opinions of objects independently of them, even when they are so pronounced that they might easily be noticed.

The most universal sign by which subjective visual phenomena can be identified appears to be by the way they accompany the movement of the eye over the field of view. Thus, the after-images, the *mouches volantes*, the blind spot, and the "luminous dust" of the dark field all participate in the motions of the eye, and coincide successively with the various stationary objects in the visual field. On the other hand, if the same phenomena recur again invariably at the same places in the visual field, they may be regarded as being objective and as being connected with external bodies. This is the case with contrast phenomena produced by after-images.

The same difficulty that we have in observing subjective sensations, that is, sensations aroused by internal causes, occurs also in trying to analyze the compound sensations, invariably excited in the same connection by any simple object, and to resolve them into their separate

[1] ¶Nearly everybody has a dominant eye, which governs the other eye; and in which the vision is superior to that in the other eye. But not many persons are aware of the fact (J.P.C.S.)

components. In such cases experience shows us how to recognize a compound aggregate of sensations as being the sign of a simple object. Accustomed to consider the sensation-complex as a connected whole, generally we are not able to perceive the separate parts of it without external help and support. Many illustrations of this kind will be seen in the following pages. For instance the perception of the apparent direction of an object from the eye depends on the combination of those sensations by which we estimate the adjustment of the eye, and on being able to distinguish those parts of the retina where light falls from those parts where it does not fall. The perception of the solid form of an object of three dimensions is the result of the combination of two different perspective views in the two eyes. The gloss of a surface, which is apparently a simple effect, is due to differences of colouring or brightness in the images of it in the two eyes. These facts were ascertained by theory and may be verified by suitable experiments. But usually it is very difficult, if not impossible, to discover them by direct observation and analysis of the sensations alone. Even with sensations that are much more involved and always associated with frequently recurring complex objects, the oftener the same combination recurs, and the more used we have become to regarding the sensation as the normal sign of the real nature of the object, the more difficult it will be to analyze the sensation by observation alone. By way of illustration, it is a familiar experience that the colours of a landscape come out much more brilliantly and definitely by looking at them with the head on one side or upside down than they do when the head is in the ordinary upright position. In the usual mode of observation all we try to do is to judge correctly the objects as such. We know that at a certain distance green surfaces appear a little different in hue. We get in the habit of overlooking this difference, and learn to identify the altered green of distant meadows and trees with the corresponding colour of nearer objects. In the case of very distant objects like distant ranges of mountains, little of the colour of the body is left to be seen, because it is mainly shrouded in the colour of the illuminated air. This vague blue-grey colour, bordered above by the clear blue of the sky or the red-yellow of the sunset glow, and below by the vivid green of meadows and forests, is very subject to variations by contrast. To us it is the vague and variable colour of distance. The difference in it may, perhaps, be more noticeable sometimes and with some illuminations than at other times. But we do not determine its true nature, because it is not ascribed to any definite object. We are simply aware of its variable nature. But the instant we take an unusual position, and look at the landscape with the head under one arm, let us say, or

between the legs, it all appears like a flat picture; partly on account of the strange position of the image in the eye, and partly because, as we shall see presently, the binocular judgment of distance becomes less accurate. It may even happen that with the head upside down the clouds have the correct perspective, whereas the objects on the earth appear like a painting on a vertical surface, as the clouds in the sky usually do. At the same time the colours lose their associations also with near or far objects, and confront us now purely in their own peculiar differences.[1] Then we have no difficulty in recognizing that the vague blue-grey of the far distance may indeed be a fairly saturated violet, and that the green of the vegetation blends imperceptibly through blue-green and blue into this violet, etc. This whole difference seems to me to be due to the fact that the colours have ceased to be distinctive signs of objects for us, and are considered merely as being different sensations. Consequently, we take in better their peculiar distinctions without being distracted by other considerations.

The connection between the sensations and external objects may interfere very much with the perception of their simplest relations. A good illustration of this is the difficulty about perceiving the double images of binocular vision when they can be regarded as being images of one and the same external object.

In the same way we may have similar experiences with other kinds of sensations. The sensation of the *timbre* of a sound, as I have shown elsewhere,[2] consists of a series of sensations of its partial tones (fundamental and harmonics); but it is exceedingly difficult to analyze the compound sensation of the sound into these elementary components. The tactile sensation of wetness is composed of that of coldness and that of smoothness of surface. Consequently, on inadvertently touching a cold piece of smooth metal, we often get the impression of having touched something wet. Many other illustrations of this sort might be adduced. They all indicate that we are exceedingly well trained in finding out by our sensations the objective nature of the objects around us, but that we are completely unskilled in observing the sensations *per se*; and that the practice of associating them with things outside of us actually prevents us from being distinctly conscious of the pure sensations.

This is true also not merely with respect to qualitative differences

[1] This explanation is given also by O. N. ROOD, SILLIMAN'S *Journ.*, (2) xxxii. 1861. pp. 184, 185.

[2] HELMHOLTZ, *Die Lehre von den Tonempfindungen.* Braunschweig 1862. (¶See English translation by A. J. ELLIS, entitled *On the sensations of tone as a physiological basis for the theory of music.* 3rd ed. London and New York, 1895.—J.P.C.S.)

of sensation, but it is likewise true with respect to the perception of space-relations. For example, the spectacle of a person in the act of walking is a familiar sight. We think of this motion as a connected whole, possibly taking note of some of its most conspicuous singularities. But it requires minute attention and a special choice of the point of view to distinguish the upward and lateral movements of the body in a person's gait. We have to pick out points or lines of reference in the background with which we can compare the position of his head. But look through an astronomical telescope at a crowd of people in motion far away. Their images are upside down, but what a curious jerking and swaying of the body is produced by those who are walking about! Then there is no trouble whatever in noticing the peculiar motions of the body and many other singularities of gait; and especially differences between individuals and the reasons for them, simply because this is not the everyday sight to which we are accustomed. On the other hand, when the image is inverted in this way, it is not so easy to tell whether the gait is light or awkward, dignified or graceful, as it was when the image was erect.

Consequently, it may often be rather hard to say how much of our apperceptions (*Anschauungen*) as derived by the sense of sight is due directly to sensation, and how much of them, on the other hand, is due to experience and training. The main point of controversy between various investigators in this territory is connected also with this difficulty. Some are disposed to concede to the influence of experience as much scope as possible, and to derive from it especially all notion of space. This view may be called the *empirical theory* (*empiristische Theorie*). Others, of course, are obliged to admit the influence of experience in the case of certain classes of perceptions; still with respect to certain elementary apperceptions that occur uniformly in the case of all observers, they believe it is necessary to assume a system of innate apperceptions that are not based on experience, especially with respect to space-relations. In contradistinction to the former view, this may perhaps be called the *intuition theory* (*nativistische Theorie*) of the sense-perceptions.

In my opinion the following fundamental principles should be kept in mind in this discussion.

Let us restrict the word *idea* (*Vorstellung*) to mean the image of visual objects as retained in the memory, without being accompanied by any present sense-impressions; and use the term *apperception* (*Anschauung*) to mean a perception (*Wahrnehmung*) when it is accompanied by the sense-impressions in question. The term *immediate perception* (*Perzeption*) may then be employed to denote an appercep-

tion of this nature in which there is no element whatever that is not the result of direct sensations, that is, an apperception such as might be derived without any recollection of previous experience. Obviously, therefore, one and the same apperception may be accompanied by the corresponding sensations in very different measure. Thus idea and immediate perception may be combined in the apperception in the most different proportions.[1]

A person in a familiar room which is brightly lighted by the sun gets an apperception that is abundantly accompanied by very vivid sensations. In the same room in the evening twilight he will not be able to recognize any objects except the brighter ones, especially the windows. But whatever he does actually recognize will be so intermingled with his recollections of the furniture that he can still move about in the room with safety and locate articles he is trying to find, even when they are only dimly visible. These images would be utterly insufficient to enable him to recognize the objects without some previous acquaintance with them. Finally, he may be in the same room in complete darkness, and still be able to find his way about in it without making mistakes, by virtue of the visual impressions formerly obtained. Thus, by continually reducing the material that appeals to the senses, the perceptual-image (*Anschauungsbild*) can ultimately be traced back to the pure memory-image (*Vorstellungsbild*) and may gradually pass into it. In proportion as there is less and less material appeal to the senses, a person's movements will, of course, become more and more uncertain, and his apperception less and less accurate. Still there will be no peculiar abrupt transition, but sensation and memory will continually supplement each other, only in varying degrees.

But even when we look around a room of this sort flooded with sunshine, a little reflection shows us that under these conditions too a large part of our perceptual-image may be due to factors of memory and experience. The fact that we are accustomed to the perspective distortions of pictures of parallelopipeds and to the form of the shadows they cast has much to do with the estimation of the shape and dimensions of the room, as will be seen hereafter. Looking at the room with one eye shut, we think we see it just as distinctly and definitely as with both eyes. And yet we should get exactly the same view in case every

[1] ¶It is very difficult to find the precise English equivalents for these metaphysical terms, which will prove satisfactory to everybody. And it may not be quite possible to restrict the English word "idea," for example, to the definition here given. It is doubtful whether the author himself is scrupulously careful throughout the remainder of this work to distinguish these shades of meaning always exactly. (J.P.C.S.)

point in the room were shifted arbitrarily to a different distance from the eye, provided they all remained on the same lines of sight.

Thus in a case like this we are really considering an extremely multiplex phenomenon of sense; but still we ascribe a perfectly definite explanation to it, and it is by no means easy to realize that the monocular image of such a familiar object necessarily means a much more meagre perception than would be obtained with both eyes. Thus too it is often hard to tell whether or not untrained observers inspecting stereoscopic views really notice the peculiar illusion produced by the instrument.

We see, therefore, how in a case of this kind reminiscences of previous experiences act in conjunction with present sensations to produce a perceptual image (*Anschauungsbild*) which imposes itself on our faculty of perception with overwhelming power, without our being conscious of how much of it is due to memory and how much to present perception.

Still more remarkable is the influence of the comprehension of the sensations in certain cases, especially with dim illumination, in which a visual impression may be misunderstood at first, by not knowing how to attribute the correct depth-dimensions; as when a distant light, for example, is taken for a near one, or *vice versa*. Suddenly it dawns on us what it is, and immediately, under the influence of the correct comprehension, the correct perceptual image also is developed in its full intensity. Then we are unable to revert to the previous imperfect apperception.

This is very common especially with complicated stereoscopic drawings of forms of crystals and other objects which come out in perfect clearness of perception the moment we once succeed in getting the correct impression.

Similar experiences have happened to everybody, proving that the elements in the sense-perceptions that are derived from experience are just as powerful as those that are derived from present sensations. All observers who have thoroughly investigated the theory of the sense-perceptions, even those who were disposed to allow experience as little scope as possible, have always admitted this.

Hence, at all events it must be conceded that, even in what appears to the adult as being direct apperception of the senses, possibly a number of single factors may be involved which are really the product of experience; although at the time it is difficult to draw the line between them.

Now in my opinion we are justified by our previous experiences in stating that no indubitable present sensation can be abolished and

overcome by an act of the intellect; and no matter how clearly we recognize that it has been produced in some anomalous way, still the illusion does not disappear by comprehending the process. The attention may be diverted from sensations, particularly if they are feeble and habitual; but in noting those relations in the external world, that are associated with these sensations, we are obliged to observe the sensations themselves. Thus we may be unmindful of the temperature-sensation of our skin when it is not very keen, or of the contact-sensations produced by our clothing, as long as we are occupied with entirely different matters. But just as soon as we stop to think whether it is warm or cold, we are not in the position to convert the feeling of warmth into that of coldness; maybe because we know that it is due to strenuous exertion and not to the temperature of the surrounding air. In the same way the apparition of light when pressure is exerted on the eyeball cannot be made to vanish simply by comprehending better the nature of the process, supposing the attention is directed to the field of vision and not, say, to the ear or the skin.

On the other hand, it may also be that we are not in the position to isolate an impression of sensation, because it involves the composite sense-symbol of an external object. However, in this case the correct comprehension of the object shows that the sensation in question has been percieved and used by the consciousness.

My conclusion is, that *nothing in our sense-perceptions can be recognized as sensation which can be overcome in the perceptual image and converted into its opposite by factors that are demonstrably due to experience.*

Whatever, therefore, can be overcome by factors of experience, we must consider as being itself the product of experience and training. By observing this rule, we shall find that it is merely the qualities of the sensation that are to be considered as real, pure sensation; the great majority of space-apperceptions, however, being the product of experience and training.

Still it does not follow that apperceptions, which persist in spite of our better conscious insight and continue as illusions, might not be due to experience and training. Our knowledge of the changes of colour produced in distant objects by the haziness of the atmosphere, of perspective distortions, and of shadow is undoubtedly a matter of experience. And yet in a good landscape picture we shall get the perfect visual impression of the distance and the solid form of the buildings in it, in spite of knowing that it is all depicted on canvas.

Similarly, our knowledge of the composite sound of the vowels is certainly obtained from experience; and yet we get the auditory-

impression of the vowel sound by combining the individual tones of tuning forks (as I have demonstrated) and grasp the sound in its entirety, although in this instance we know that it is really compound.

Here we still have to explain how experience counteracts experience, and how illusion can be produced by factors derived from experience, when it might seem as if experience could not teach anything except what was true. In this matter we must remember, as was intimated above, that the sensations are interpreted just as they arise when they are stimulated in the normal way, and when the organ of sense is used normally.

We are not simply passive to the impressions that are urged on us, but we *observe*, that is, we adjust our organs in those conditions that enable them to distinguish the impressions most accurately. Thus, in considering an involved object, we accommodate both eyes as well as we can, and turn them so as to focus steadily the precise point on which our attention is fixed, that is, so as to get an image of it in the fovea of each eye; and then we let our eyes traverse all the noteworthy points of the object one after another. If we are interested in the general shape of the object and are trying to get as good an idea as we can of its relative dimensions, we assume a position such that, without having to turn the head, we can survey the whole surface, enabling us at the same time to view as symmetrically as possible those dimensions we wish to compare. Thus, in looking at an object, as, for example, a building with prominent horizontal and vertical lines, we like to stand opposite to it with the centres of rotation of the two eyes in a horizontal line. This position of the eyes can be controlled at any moment by separating the double images; which in the case mentioned here are in the same horizontal plane.

Unquestionably, our reason for choosing this definite mode of seeing is because in this way we can observe and compare most accurately; and, consequently, in this so-called *normal* use of the eyes we learn best how to compare our sensations with the reality. And so we obtain also the most correct and most accurate perceptions by this method.

But if, from necessity or on purpose, we employ a different mode of looking at objects, that is, if we view them merely indriectly or without focusing both eyes on them, or without surveying them all over, or if we hold the head in some unusual position, then we shall not be able to have as accurate apperceptions as when the eyes are used in the normal fashion. Nor are we so well trained in interpreting what we see under such circumstances as in the other case. Hence there is more scope for interpretation, although, as a rule, we are not clearly aware

of this uncertainty in the explanation of our sense-perceptions. When we see an object in front of us, we are obliged to assign it to some definite place in space. We cannot think of it as having some dubious intermediate position between two different places in space. Without any recollections coming to our aid, we are wont to interpret the phenomenon as it would have to be interpreted if we had received the same impression in the normal and most accurate mode of observation. Thus certain illusions enter into the perception, unless we concentrate our eyes on the objects under observation, or when the objects are in the peripheral part of the visual field, or if the head is held to one side, or if we do not focus the object with both eyes at once. Moreover, the agreement between the images on the two retinas is most constant and regular in looking at distant objects. The fact that the horizontal floor usually happens to be in the lower part of the visual field, apparently influences the comparison of the fields of the two eyes in a peculiar manner. Thus, our judgment as to the position of near objects is not entirely correct when we observe them with the look tilted decidedly up or down. The retinal images presented in this way are interpreted just as if they had been obtained by looking straight ahead. We run across many illustrations of this sort. Our training in interpreting immediate perceptions is not equally good in all directions of the eyes, but simply for those directions which enable us to have the most accurate and most consistent perceptions. We transfer the latter to all cases, as in the instances just cited.

Now it is quite possible that the similarity between a visual impression of this kind and one of the possible impressions obtained by normal observation may not be so overwhelming and striking as to preclude many other comparisons and corresponding interpretations of that impression. In such cases the explanation of the impression varies. Without any change of the retinal images, the same observer may see in front of him various perceptual images in succession, in which case the variation is easy to recognize. Or else one observer may incline more toward one comparison and interpretation, and another toward another. This has been a source of much controversy in physiological optics, because each observer has been disposed to consider the apperception which he obtained by the most careful observation he could make as being the only valid one. But supposing that we have such confidence in the observers as to assume that their observations were careful and unprejudiced, and that they knew how to make them, it would not be proper in such cases to adopt one of the conflicting interpretations of the visual phenomenon as being the only correct one. And yet that is what they are disposed to do who try to derive the

origin of perceptual images mainly from innate factors. The truth rather is, that in a case of this sort various perceptual images may be developed; and we should seek rather to discover what circumstances are responsible for the decision one way or the other.

It is true we meet with a difficulty here that does not exist in the other parts of the natural sciences. In many instances we have simply the assertions of individual observers, without being in the position to verify them by our own observation. Many idiosyncrasies are manifested in this region, some of which are doubtless due to the structure of the eyes, others to the habitual way of using the eyes, and others still perhaps to previous impressions and apperceptions. Of course, nobody save the person who has peculiarities of this nature can observe their effects, and nobody else can give an opinion about them. On the other hand, observation in this region is by no means so easy as might be supposed at first. Steady fixation of a point for a long time while observations are being made in indirect vision; controlling the attention; taking the mind away from the ordinary objective interpretation of sense-impression; estimation of difference of colour and of difference of space in the visual field—all these things take much practice. And hence a number of facts in this region cannot be observed at all without having had previous long training in making observations in physiological optics. It cannot be done even by persons who are skilled in making other kinds of observations. Thus, with respect to many matters we have to depend on the observations of a very limited number of individuals, and hence when the results found by somebody else are different, it is much harder in this subject than anywhere else to judge rightly whether secondary influences have not contributed in an observation of this sort. Accordingly, I must apprise the reader in advance that much of the material that is perhaps new in the following chapters may possibly be due to individual peculiarities of my own eyes. Under such circumstances, there was no alternative for me except to observe as carefully as possible the facts as they appeared to my own eyes, and to try to ascertain their connection. Discrepancies that have been found by other observers have been noted. But how widespread this or the other mode of vision may be, is something that has to be left to the future to determine.

Incidentally, the more the visual impressions are unlike the normal ones, the greater will be the variety of interpretation as a rule. This is a natural consequence of the view which I hold, and is an essential characteristic of the activity of psychic influences.

Heretofore practically nothing has been ascertained as to the nature of psychic processes. We have simply an array of facts. Therefore, it is

not strange that no real explanation can be given of the origin of sense-perceptions. The *empirical theory* attempts to prove that at least no other forces are necessary for their origin beyond the known faculties of the mind, although these forces themselves may remain entirely unexplained. Now generally it is a useful rule in scientific investigation not to make any new hypothesis so long as known facts seem adequate for the explanation, and the necessity of new assumptions has not been demonstrated. That is why I have thought it incumbent to prefer the empirical view essentially. Still less does the *intuition theory* attempt to give any explanation of the origin of our perceptual images; for it simply plunges right into the midst of the matter by assuming that certain perceptual images of space would be produced directly by an innate mechanism, provided certain nerve fibres were stimulated. The earlier forms of this theory implied some sort of self-observation of the retina; inasmuch as we were supposed to know by intuition about the form of this membrane and the positions of the separate nerve terminals in it. In its more recent development, especially as formulated by E. HERING, there is an hypothetical subjective visual space, wherein the sensations of the separate nerve fibres are supposed to be registered according to certain intuitive laws. Thus in this theory not only is KANT's assertion adopted, that the general apperception of space is an original form of our imagination, but certain special apperceptions of space are assumed to be intuitive.

The naturalistic view has been called also a special *theory of identity*, because in it the perfect fusion of the impressions on the corresponding places of the two retinas has to be postulated. On the other hand, the *empirical* theory is spoken of as a *theory of projection*,[1] because according to it the perceptual images of objects are projected in space by means of psychic processes. I should like to avoid this term, because both supporters and opponents of this view have often attached undue importance to the idea that this projection must take place parallel to the lines of direction; which was certainly not the correct description of the psychic process. And, even if this construction were admitted as being valid simply with respect to the physiological description of the process, the idea would be incorrect in very many instances.

I am aware that in the present state of knowledge it is impossible to refute the intuition theory. The reasons why I prefer the opposite view are because in my opinion:

1. The intuition theory is an unnecessary hypothesis.

[1] See remarks in Appendix I as to misunderstandings connected with the term "projection theory."—K.

2. Its consequences thus far invariably apply to perceptual images of space which only in the fewest cases are in accordance with reality and with the correct visual images that are undoubtedly present; as will be shown in detail later. The adherents of this theory are, therefore, obliged to make the very questionable assumption, that the *space sensations*, which according to them are present originally, are continually being improved and overruled by knowledge which we have accumulated by experience. By analogy with all other experiences, however, we should have to expect that the sensations which have been overruled continued to be present in the apperception as a conscious illusion, if nothing else. But this is not the case.

3. It is not clear how the assumption of these original "*space sensations*" can help the explanation of our visual perceptions, when the adherents of this theory ultimately have to assume in by far the great majority of cases that these sensations must be overruled by the better understanding which we get by experience. In that case it would seem to me much easier and simpler to grasp, that all apperceptions of space were obtained simply by experience, instead of supposing that the latter have to contend against intuitive perceptual images that are generally false.

This is by way of justifying my point of view. A choice had to be made simply for the sake of getting at least some sort of superficial order amid the chaos of phenomena; and so I believed I had to adopt the view I have chosen. However, I trust it has not affected the correct observation and description of the facts.

To prevent misunderstandings as to my meaning, and to make it clearer to the natural intelligence of those readers who have never thought much about their sense-perceptions, the following explanations will be added.

Thus far the sensations have been described as being simply *symbols* for the relations in the external world. They have been denied every kind of similarity or equivalence to the things they denote. Here we touch on the much disputed point as to how far our ideas agree in the main with their objects; that is, whether they are true or false, as one might say. Some have asserted that there is such an agreement, and others have denied it. In favour of it, a *pre-established harmony* between nature and mind was assumed. Or it was maintained that there was an *identity* of nature and mind, by regarding nature as the product of the activity of a general mind; the human mind being supposed to be an emanation from it. The *intuition theory* of space-apperceptions is connected with these views to the extent that, by some innate mechanism and a certain pre-established harmony, it

admits of the origin of perceptual images that are supposed to correspond with reality, although in a rather imperfect fashion.

Or else the agreement between ideas and their objects was denied, the ideas being explained therefore as illusions. Consequently, it was necessary to deny also the possibility of all knowledge of any objects whatsoever. This was the attitude of certain so-called "sensational" philosophers in England in the eighteenth century. However, it is not my purpose here to undertake an analysis of the opinions of the various philosophical schools on this question. That would be much too extensive a task in this place. I shall confine myself therefore merely to inquiring what I think should be the attitude of an investigator toward these controversies.

Our apperceptions and ideas are *effects* wrought on our nervous system and our consciousness by the objects that are thus apprehended and conceived. Each effect, as to its nature, quite necessarily depends both on the nature of what causes the effect and on that of the person on whom the effect is produced. To expect to obtain an idea which would reproduce the nature of the thing conceived, that is, which would be true in an absolute sense, would mean to expect an effect which would be perfectly independent of the nature of the thing on which the effect was produced; which would be an obvious absurdity. Our human ideas, therefore, and all ideas of any conceivable intelligent creature, must be images of objects whose mode is essentially co-dependent on the nature of the consciousness which has the idea, and is conditioned also by its idiosyncrasies.

In my opinion, therefore, there can be no possible sense in speaking of any other truth of our ideas except of a *practical* truth. Our ideas of things *cannot* be anything but symbols, natural signs for things which we learn how to use in order to regulate our movements and actions. Having learned correctly how to read those symbols, we are enabled by their help to adjust our actions so as to bring about the desired result; that is, so that the expected new sensations will arise. Not only is there *in reality* no other comparison at all between ideas and things — all the schools are agreed about this — but any other mode of comparison is entirely *unthinkable* and has no sense whatever. This latter consideration is the conclusive thing, and must be grasped in order to escape from the labyrinth of conflicting opinions. To ask whether the idea I have of a table, its form, strength, colour, weight, etc., is true *per se*, apart from any practical use I can make of this idea, and whether it corresponds with the real thing, or is false and due to an illusion, has just as much sense as to ask whether a certain musical note is red, yellow, or blue. Idea and the thing conceived evidently

belong to two entirely different worlds, which no more admit of being compared with each other than colours and musical tones or than the letters of a book and the sound of the word they denote.

Were there any sort of similarity of correspondence between the idea in the head of a person A and the thing to which the idea belongs, another intelligent person B, conceiving both the thing itself and A's idea of it, according to the same laws, might be able to find some similarity between them or at least to suppose so; because the same sort of thing represented (conceived) in the same way would have to give the same kinds of images (ideas). Now I ask, what similarity can be imagined between the process in the brain that is concomitant with the idea of a table and the table itself? Is the form of the table to be supposed to be outlined by electric currents? And when the person with the idea has the idea that he is walking around the table, must the person then be outlined by electric currents? Perspective projections of the external world in the hemispheres of the brain (as they are supposed to be) are evidently not sufficient for representing the idea of a bodily object. And granted that a keen imagination is not frightened away by these and similar hypotheses, such an electrical reproduction of the table in the brain would be simply another bodily object to be perceived, but no idea of the table. However, it is not simply persons with materialistic opinions who try to refute the proposed statement, but also persons with idealistic views. And for the latter I should think the argument would be still more forcible. What possible similarity can there be between the idea, some modification of the incorporeal mind that has no extension in space, and the body of the table that occupies space? As far as I am aware, the idealistic philosophers have never once investigated even a single hypothesis or imagination in order to show this connection. And by the very nature of this view it is something that cannot be investigated at all.

In the next place as to the *properties* of objects in the external world, a little reflection reveals that all properties attributable to them may be said to be simply *effects* exerted by them either on our senses or on other natural objects. Colour, sound, taste, smell, temperature, smoothness, and firmness are properties of the first sort, and denote effects on our organs of sense. Smoothness and firmness denote the degree of resistance either to the gliding contact or pressure of the hand. But other natural bodies may be employed instead of the hand. And the same thing is true in testing other mechanical properties such as elasticity and weight. Chemical properties are described by certain reactions, that is, by effects exerted by one natural body

on others. It is the same way with any other physical property of a body, optical, electrical, or magnetic. In every case we have to do with the mutual relations between various bodies and with the effects depending on the forces that different bodies exert on each other. For all natural forces are such as are exerted by one body on others. When we try to think of mere matter without force, it is void of properties likewise, except as to its different distribution in space and as to its motion. All properties of bodies in nature are manifested therefore simply by being so situated as to interact with other bodies of nature or with our organs of sense. But as such interaction may occur at any time, particularly too as it may be produced by us voluntarily at any moment, and as then we see invariably the peculiar sort of interaction occurring, we attribute to the objects a permanent capacity for such effects which is always ready to become effective. This permanent capacity is a so-called characteristic *property*.

The result is that in point of fact the characteristic *properties* of natural objects, in spite of this name, do not denote something that is peculiar to the individual object by itself, but invariably imply some relation to a second object (including our organs of sense). The kind of effect must, of course, depend always on the peculiarities both of the body producing it and of the body on which it is produced. As to this there is never any doubt even for an instant, provided we have in mind those properties of bodies that are manifested when two bodies belonging to the external world react on each other, as in the case of chemical reactions. But in the case of properties depending on the mutual relations between things and our organs of sense, people have always been disposed to forget that here too we are concerned with the reaction toward a special reagent, namely, our own nervous system; and that colour, smell, and taste, and feeling of warmth or cold are also effects quite essentially depending on the nature of the organ that is affected. Doubtless, the reactions of natural objects to our senses are those that are most frequently and most generally perceived. For both our welfare and convenience they are of the most powerful importance. The reagent by which we have to test them is something we are endowed with by nature, but that does not make any difference in the connection.

Hence there is no sense in asking whether vermilion as we see it, is really red, or whether this is simply an illusion of the senses. The sensation of red is the normal reaction of normally formed eyes to light reflected from vermilion. A person who is red-blind will see vermilion as black or as a dark grey-yellow. This too is the correct reaction for an eye formed in the special way his is. All he has to know is that his eye is

simply formed differently from that of other persons. In itself the one sensation is not more correct and not more false than the other, although those who call this substance red are in the large majority. In general, the red colour of vermilion exists merely in so far as there are eyes which are constructed like those of most people. Persons who are red-blind have just as much right to consider that a characteristic property of vermilion is that of being black. As a matter of fact, we should not speak of the light reflected from vermilion as being red, because it is not red except for certain types of eyes. When we speak of the properties of bodies with reference to other bodies in the external world, we do not neglect to name also the body with respect to which the property exists. Thus we say that lead is soluble in nitric acid, but not in sulphuric acid. Were we to say simply that lead is soluble, we should notice at once that the statement is incomplete, and the question would have to be asked immediately, Soluble in what? But when we say that vermilion is red, it is implicitly understood that it is red for our eyes and for other people's eyes supposed to be made like ours. We think this does not need to be mentioned, and so we neglect to do so, and can be misled into thinking that red is a property belonging to vermilion or to the light reflected from it, entirely independently of our organs of sense. The statement that the waves of light reflected from vermilion have a certain length is something different. That is true entirely without reference to the special nature of our eye. Then we are thinking simply of relations that exist between the substance and the various systems of waves in the aether.

The only respect in which there can be a real agreement between our perceptions and the reality is the time-sequence of the events with their various peculiarities. Simultaneity, sequence, the regular recurrence of simultaneity or sequence, may occur likewise in the sensations as well as in the events. The external events, like their perceptions, proceed in time; and so the temporal relations of the latter may be the faithful reproduction of the temporal relations of the former. The sensation of thunder in the ear succeeds the sensation of lightning in the eye, just in the same way as the sound vibrations in the air due to the electrical discharge reach the place where the observer is later than the vibrations of the luminiferous aether. Yet here it certainly should be noted that the time-sequence of the sensations is not quite a faithful reproduction of the time-sequence of the external events, inasmuch as the transmission from the organs of sense to the brain takes time, and in fact a different time for different organs. Moreover, in case of the eye and the ear, the time has to be added that it takes light and sound to reach the organ. Thus at present we see the fixed stars as they were various long periods of years ago.

As to the representation of space-relations, there certainly is something of this sort in the peripheral nerve terminals in the eye and to a certain extent in the tactile skin, but still only in a limited way; for the eye gives only perspective surface-images, and the hand reproduces the objective area on the surface of a body by shaping itself to it as congruently as possible. A direct image of a portion of space of three dimensions is not afforded either by the eye or by the hand. It is only by comparing the images in the two eyes, or by moving the body with respect to the hand, that the idea of solid bodies is obtained. Now since the brain itself has three dimensions, of course, there is still another conceivable possibility, and that is to fancy by what mechanism in the brain itself images of three dimensions can arise from external objects in space. But I cannot see any necessity for such an assumption nor even any probability for it. The idea of a body in space, of a table, for instance, involves a quantity of separate observations. It comprises the whole series of images which this table would present to me in looking at it from different sides and at different distances; besides the whole series of tactile impressions that would be obtained by touching the surface at various places in succession. Such an idea of a single individual body is, therefore, in fact a *conception* (*Begriff*) which grasps and includes an infinite number of single, successive apperceptions, that can all be deduced from it; just as the species "table" includes all individual tables and expresses their common peculiarities. The idea of a single individual table which I carry in my mind is correct and exact, provided I can deduce from it correctly the precise sensations I shall have when my eye and my hand are brought into this or that definite relation with respect to the table. Any other sort of similarity between such an idea and the body about which the idea exists, I do not know how to conceive. One is the mental symbol of the other. The kind of symbol was not chosen by me arbitrarily, but was forced on me by the nature of my organ of sense and of my mind. This is what distinguishes this sign-language of our ideas from the arbitrary phonetic signs and alphabetical characters that we use in speaking and writing. A writing is correct when he who knows how to read it forms correct ideas by it. And so the idea of a thing is correct for him who knows how to determine correctly from it in advance what sense-impressions he will get from the thing when he places himself in definite external relations to it. Incidentally, it does not matter at all what sort of mental symbols we employ, provided they constitute a sufficiently varied and ordered system. Nor does it matter either how the words of a language sound, provided there are enough of them, with sufficient means of denoting their grammatical relations to one another.

On this view of the matter, we must be on our guard against saying that all our ideas of things are consequently *false*, because they are not *equal* to the things themselves, and that hence we are not able to know anything as to the *true nature* of things. That they cannot be equal to things, is in the nature of knowledge. Ideas are merely pictures of things. Every image is the image of a thing merely for him who knows how to read it, and who is enabled by the aid of the image to form an idea of the thing. Every image is similar to its object in one respect, and dissimilar in all others, whether it be a painting, a statue, the musical or dramatic representation of a mental mood, etc. Thus the ideas of the external world are images of the regular sequence of natural events, and if they are formed correctly according to the laws of our thinking, and we are able by our actions to translate them back into reality again, the ideas we have are also the *only true* ones for our mental capacity. All others would be false.

In my opinion, it is a mistake, therefore, to try to find preestablished harmony between the laws of thought and those of nature, an identity between nature and mind, or whatever we may call it. A system of signs may be more or less perfect and convenient. Accordingly, it will be more or less easy to employ, more exact in denoting or more inexact, just as is the case with different languages. But otherwise each system can be adapted to the case more or less well. If there were not a number of similar natural objects in the world, our faculty of forming shades of conception would indeed not be of any use to us. Were there no solid bodies, our geometrical faculties would necessarily remain undeveloped and unused, just as the physical eye would not be of any service to us in a world where there was no light. If in this sense anybody wishes to speak of an adaptation of our laws of mind to the laws of nature, there is no objection to it. Evidently, however, such adaptation does not have to be either perfect or exact. The eye is an extremely useful organ practically, although it cannot see distinctly at all distances, or perceive all sorts of aether vibrations, or concentrate exactly in one point all the rays that issue from a point. Our intellectual faculties are connected with the activities of a material organ, namely the brain, just as the faculty of vision is connected with the eye. Human intelligence is wonderfully effective in the world, and brings it under a strict law of causation. Whether it necessarily must be able to control whatever is in the world or can happen—I can see no guarantee for that.

We must speak now of the manner in which our ideas and perceptions are formed by inductive conclusions. The best analysis of the nature of our conclusions I find in J. S. Mill's Logic. As long as the

premise of the conclusion is not an injunction imposed by outside authority for our conduct and belief, but a statement related to reality, which can therefore be only the result of experience, the conclusion, as a matter of fact, does not tell us anything new or something that we did not know already before we made the statement. Thus, for example:

Major: All men are mortal.
Minor: Caius is a man.
Conclusion: Caius is mortal.

The major premise, that all men are mortal, which is a statement of experience, we should scarcely venture to assert without knowing beforehand whether the conclusion is correct, namely, that Caius, who is a man, either is dead or will die. Thus we must be sure of the conclusion before we can state the major premise by which we intend to prove it. That seems to be proceeding in a circle. The real relation evidently is, that, in common with other folks, we have observed heretofore without exception that no person has ever survived beyond a certain age. Observers have learned by experience that Lucius, Flavius and other individuals of their acquaintance, no matter what their names are, have all died; and they have embraced this experience in the general statement, that *all* men die. Inasmuch as this final result occurred regularly in all the instances they observed, they have felt justified in explaining this general law as being valid also for all those cases which might come up for observation hereafter. Thus we preserve in our memory the store of experiences heretofore accumulated on this subject by ourselves and others in the form of the general statement which constitutes the major premise of the above conclusion.

However, the conviction that Caius would die might obviously have been reached directly also without formulating the general statement in our consciousness, by having compared his case with all those which we knew previously. Indeed, this is the more usual and original method of reasoning by induction. Conclusions of this sort are reached without conscious reflection, because in our memory the same sort of thing in cases previously observed unites and reinforces them; as is shown especially in those cases of inductive reasoning where we cannot succeed in deducing from previous experiences a rule with precisely defined limits to its validity and without any exceptions. This is the case in all complicated processes. For instance, from analogy with previous similar cases, we can sometimes predict with tolerable certainty what one of our acquaintances will do, if under certain circumstances he decides to go into business; because we know his character and that he is, let us say, ambitious or timid. We may not be

able to say exactly how we have estimated the extent of his ambition or timidity, or why this ambition or timidity of his will be enough to decide that his business will turn out as we expect.

In the case of conclusions properly so-called, which are reached consciously, supposing they are not based on injunctions but on facts of experience, what we do, therefore, is really nothing more than deliberately and carefully to retrace those steps in the inductive generalizations of our experiences which were previously traversed more rapidly and without conscious reflection, either by ourselves or by other observers in whom we have confidence. But although nothing essentially new is added to our previous knowledge by formulating a general principle from our previous experiences, still it is useful in many respects. A definitely stated general principle is much easier to preserve in the memory and to be imparted to others than to have to do this same thing with every individual case as it arises. In formulating it we are led to test accurately every new case that occurs, with reference to the correctness of the generalization. In this way every exception will be impressed on us twice as forcibly. The limits of its validity will be recalled much sooner when we have the principle before us in its general form, instead of having to go over each separate case. By this sort of conscious formulation of inductive reasoning, there is much gain in the convenience and certainty of the process; but nothing essentially new is added that did not exist already in the conclusions which were reached by analogy without reflection. It is by means of these latter that we judge the character of a person from his countenance and movements, or predict what he will do in a given situation from a knowledge of his character.

Now we have exactly the same case in our sense-perceptions. When those nervous mechanisms whose terminals lie on the right-hand portions of the retinas of the two eyes have been stimulated, our usual experience, repeated a million times all through life, has been that a luminous object was over there in front of us on our left. We had to lift the hand toward the left to hide the light or to grasp the luminous object; or we had to move toward the left to get closer to it. Thus while in these cases no particular conscious conclusion may be present, yet the essential and original office of such a conclusion has been performed, and the result of it has been attained; simply, of course, by the unconscious processes of association of ideas going on in the dark background of our memory. Thus too its results are urged on our consciousness, so to speak, as if an external power had constrained us, over which our will has no control.

These inductive conclusions leading to the formation of our sense-

perceptions certainly do lack the purifying and scrutinizing work of conscious thinking. Nevertheless, in my opinion, by their peculiar nature they may be classed as *conclusions*, inductive conclusions unconsciously formed.

There is one circumstance quite characteristic of these conclusions which operates against their being admitted in the realm of conscious thinking and against their being formulated in the normal form of logical conclusions. This is that we are not able to specify more closely what has taken place in us when we have experienced a sensation in a definite nerve fibre, and how it differs from corresponding sensations in other nerve fibres. Thus, suppose we have had a sensation of light in certain fibres of the nervous mechanism of vision. All we know is that we have had a sensation of a peculiar sort which is different from all other sensations, and also from all other visual sensations, and that whenever it occurred, we invariably noticed a luminous object on the left. Naturally, without ever having studied physiology, this is all we can say about the sensation, and even for our own imagination we cannot localize or grasp the sensation except by specifying it in terms of the conditions of its occurrence. I have to say, "I see something bright there on my left." That is the only way I can describe the sensation. After we have pursued scientific studies, we begin to learn that we have nerves, that these nerves have been stimulated, and that their terminals in fact lie on the right-hand side of the retina. Then for the first time we are in a position to define this mode of sensation independently of the mode in which it is ordinarily produced.

It is the same way with most sensations. The sensations of taste and smell usually cannot be described even as to their quality except in terms of the bodies responsible for them; although we do have a few rather vague and more general expressions like "sweet," "sour," "bitter" and "sharp."

These judgments, in which our sensations in our ordinary state of consciousness are connected with the existence of an external cause, can never once be elevated to the plane of conscious judgments. The inference that there is a luminous object on my left, because the nerve terminals on the right-hand side of my retina are in a state of stimulation, can only be expressed by one who knows nothing about the inner structure of the eye by saying, "There is something bright over there on my left, because I see it there." And accordingly from the standpoint of everyday experience, the only way of expressing the experience I have when the nerve terminals on the right-hand side of my eyeball are stimulated by exerting pressure there, is by saying, "When I press my eye on the right-hand side, I see a bright glow on the left."

There is no other way of describing the sensation and of identifying it with other previous sensations except by designating the place where the corresponding external object appears to be. Hence, therefore, these cases of experience have the peculiarity that the connection between the sensation and an external object can never be expressed without anticipating it already in the designation of the sensation, and without presupposing the very thing we are trying to describe.

Even when we have learned to understand the physiological origin and connection of the illusions of the senses, it is impossible to get rid of the illusion in spite of our better knowledge. This is because inductive reasoning is the result of an unconscious and involuntary activity of the memory; and for this very reason it strikes our consciousness as a foreign and overpowering force of nature. Incidentally, manifold analogies for it are to be found in all other possible modes of *apparition*. We might say that all apparition originates in premature, unmeditated inductions, where from previous cases conclusions are deduced as to new ones, and where the tendency to abide by the false conclusions persists in spite of the better insight into the matter based on conscious deliberation. Every evening apparently before our eyes the sun goes down behind the stationary horizon, although we are well aware that the sun is fixed and the horizon moves. An actor who cleverly portrays an old man is for us an old man there on the stage, so long as we let the immediate impression sway us, and do not forcibly recall that the programme states that the person moving about there is the young actor with whom we are acquainted. We consider him as being angry or in pain according as he shows us one or the other mode of countenance and demeanour. He arouses fright or sympathy in us, we tremble for the moment, which we see approaching, when he will perform or suffer something dreadful; and the deep-seated conviction that all this is only show and play does not hinder our emotions at all, provided the actor does not cease to play his part. On the contrary, a fictitious tale of this sort, which we seem to enter into ourselves, grips and tortures us more than a similar true story would do when we read it in a dry documentary report.

The experiences we have that certain aspects, demeanours and modes of speech are indicative of fierce anger, are generally experiences concerning the external signs of certain emotions and peculiarities of character which the actor can portray for us. But they are not nearly so numerous and regular in recurrence as those experiences by which we have ascertained that certain sensations correspond with certain external objects. And so we need not be surprised if the idea of an object which is ordinarily associated with a sensation does not vanish,

even when we know that in this particular instance there is no such object.

Finally, the tests we employ by voluntary movements of the body are of the greatest importance in strengthening our conviction of the correctness of the perceptions of our senses. And thus, as contrasted with purely passive observations, the same sort of firmer conviction arises as is derived by the process of experiment in scientific investigations. The peculiar ultimate basis, which gives convincing power to all our conscious inductions, is the law of causation. If two natural phenomena have frequently been observed to occur together, such as thunder and lightning, they seem to be regularly connected together, and we infer that there must be a common basis for both of them. And if this causal connection has invariably acted heretofore, so that thunder and lightning accompany each other, then in the future too like causes must produce like effects, and the result must be the same in the future. However, so long as we are limited to mere observations of such phenomena as occur by themselves without our help, and without our being able to make experiments so as to vary the complexity of causes, it is difficult to be sure that we have really ascertained all the factors that may have some influence on the result. There must be an enormous variety of cases where the law is obeyed, and the law must define the result with great precision, if we are to be satisfied with a case of mere observation. This is the case with the motions of the planetary system. Of course, we cannot experiment with the planets, but the theory of universal gravitation as propounded by Newton gives such a complete and exact explanation of the comparatively complicated apparent motions of the heavenly bodies, that we no longer hesitate about considering it as being sufficiently proved. And yet there are Reich's experiments on the gravitational attraction of lead balls, Foucault's experiment on the deviation of the plane of vibration of a pendulum in consequence of the earth's rotation, and the experimental determinations of the velocity of light in traversing terrestrial distances as made by Foucault and Fizeau, that are of the utmost value in strengthening our conviction experimentally also.

Probably there is no event of pure observation that has been found to be so unexceptionally correct as the general statement previously used by way of illustration, namely, that all human beings die before they have passed a certain age. In many millions of human beings not a single exception has been found. If one had occurred, we might assume that we should have heard of it. Among those who have died there are individuals who have lived in the most varied climates and on the most various kinds of nourishment, besides having been engaged

in the most diverse occupations. Nevertheless, the statement that all men are bound to die, cannot be said to have the same degree of certainty as any law of physics whose consequences have been precisely compared experimentally with experience in manifold modifications. I do not know the causal connection for the death of human beings. I cannot state the causes that inevitably entail old age, in case life has not been terminated sooner by some rougher external injury. I have not been able to verify by experiments that when I allow those causes to operate, old age inevitably occurs, and that it does not occur when I remove those causes of its occurrence. Anyone who tells me that the life of man can be indefinitely prolonged by employing certain means may be treated, of course, with the utmost incredulity, but he cannot be positively contradicted without knowing certainly that individuals have actually lived in the circumstances he describes, and yet have ultimately perished. On the other hand, when I assert that all liquid mercury will expand when it is heated, if it is free to do so, I know that whenever I have observed the two together, not only higher temperature and expansion of mercury were due to the action of an unknown common third cause, as I might have supposed from pure observation alone, but I know by experiment that the heat by itself was enough to cause the expansion of the mercury. At various times I have often heated mercury. I have deliberately selected the moment when I wished the experiment to begin. If therefore the mercury expanded under these circumstances, the expansion must have been dependent on those conditions that I produced in the experiment. Consequently, I know that the heating by itself was a sufficient cause for the expansion, and that no other latent influences were needed to bring about this result. By comparatively few carefully executed experiments we are enabled to establish the causal conditions of an event with more certainty than can be done by a million observations where we have not been able to vary the conditions as we please. For instance, if I had merely seen mercury expand in a thermometer which was inaccessible to me, and in a place where the air was saturated with moisture at all temperatures, I should have to inquire whether mercury expands on account of heat or on account of the moisture. The only way to determine this would be by experiment, and by finding out whether the volume of mercury changes with change of humidity, when the temperature is kept constant, or with change of temperature, when the humidity is kept constant.

The same great importance which experiment has for the certainty of our scientific convictions, it has also for the unconscious inductions of the perceptions of our senses. It is only by voluntarily bringing our

organs of sense in various relations to the objects that we learn to be sure as to our judgments of the causes of our sensations. This kind of experimentation begins in earliest youth and continues all through life without interruption.

If the objects had simply been passed in review before our eyes by some foreign force without our being able to do anything about them, probably we should never have found our way about amid such an optical phantasmagoria; any more than mankind could interpret the apparent motions of the planets in the firmament before the laws of perspective vision could be applied to them. But when we notice that we can get various images of a table in front of us simply by changing our position; and that we can sometimes have one view and sometimes another, just as we like at any time, by a suitable change of position; and that the table may vanish from sight, and then be there again at any moment we like, simply by turning the eyes toward it; we get the conviction based on experiment, that our movements are responsible for the different views of the table, and that whether we see it just at this moment or do not see it, still we can see it whenever we like. Thus by our movements we find out that it is the stationary form of the table in space which is the cause of the changing image in our eyes. We explain the table as having existence independent of our observation, because at *any moment we like*, simply by assuming the proper position with respect to it, we can observe it.

The essential thing in this process is just this principle of experimentation. Spontaneously and by our own power, we vary some of the conditions under which the object has been perceived. We know that the changes thus produced in the way that objects look depend solely on the movements we have executed. Thus we obtain a different series of apperceptions of the same object, by which we can be convinced with experimental certainty that they are simply apperceptions, and that it is the common cause of them all. In fact we see children also experimenting with objects in this way. They turn them constantly round and round, and touch them with the hands and the mouth, doing the same things over and over again day after day with the same objects, until their forms are impressed on them; in other words, until they get the various visual and tactile impressions made by observing and feeling the same object on various sides.

In this sort of experimentation with objects some of the changes in the sense-impressions are found to be due to our own will; whereas others, that is, all that depend on the nature of the object directly before us, are urged upon us by a necessity which we cannot alter as we like, and which we feel most when it arouses disagreeable sensations

or pain. Thus we come to recognize something independent of our will and imagination, that is, an external cause of our sensations. This is shown by its persisting independently of our instantaneous perception; because at any moment we like, by suitable manipulations and movements, we can cause to recur each one of the series of sensations that can be produced in us by this external cause. Thus this latter is recognized as an object existing independently of our perception.

The idea and the cause here combine, and it is a question whether we have a right to assume this cause in the original perception of the senses. Here again the difficulty is that we are not able to describe the processes except in the language of metaphysics, whereas the reflection of the consciousness in itself is not yet distinctly contained in the original form of the conscious perception.

Natural consciousness, which is entirely absorbed in the interest of observing the external world, and has little inducement to direct its attention to the Ego that appears always the same amid the multicoloured variations of outside objects, is not in the habit of noticing that the *properties* of the objects that are seen and touched are their effects, partly on other natural bodies, but mainly on our senses. Now as our nervous system and our sensation-faculty, as being the constant reagent on which the effect is exerted, is thus left out of account entirely, and as the difference of the effect is regarded as being simply a difference in the object from which it proceeds, the effect can no longer be recognized as an effect (for every effect must be the effect on something else), and so comes to be considered objectively as being a property of the body and merely as belonging to it. And then as soon as we recall that we perceive these properties, our impression, consequently, seems to us to be a pure image of the external state of affairs reflecting only that external condition and depending solely on it.

But if we ponder over the basis of this process, it is obvious that we can never emerge from the world of our sensations to the apperception of an external world, except by inferring from the changing sensation that external objects are the causes of this change. Once the idea of external objects has been formed, we may not be concerned any more as to how we got this idea, especially because the inference appears to be so self-evident that we are not conscious of its being a new result.

Accordingly, the law of causation, by virtue of which we infer the cause from the effect, has to be considered also as being a law of our thinking which is prior to all experience. Generally, we can get no experience from natural objects unless the law of causation is already active in us. Therefore, it cannot be deduced first from experiences which we have had with natural objects.

This statement has been made in many ways. The law of causation was supposed to be a law of nature arrived at by induction. Recently it has been again interpreted in that way by J. S. Mill. He has even suggested the possibility of its not being valid in other parts of the universe. As opposed to that view, I shall merely say, for what it is worth, that there is good reason to think that the empirical proof of the law is extremely doubtful. For the number of cases in which we think we can trace perfectly the causal connection between natural processes is small as compared with the number of those in which we are absolutely unable to do so at present. The former cases belong almost exclusively to inorganic nature. The cases that are not understood include the larger part of the phenomena of organic nature. In fact, by the evidence of our own consciousness, we positively assume both in beasts and in man a principle of free will, for which we claim most decidedly complete independence of the force of the law of causation. And in spite of all theoretical speculations as to possible mistakes about this conviction, I am of the opinion that our natural consciousness will hardly ever be free from it. Thus the case of conduct itself, which we know best and most accurately, we consider as being an exception to that law. Were therefore the law of causation a law of experience, its inductive proof would seem to be in a very bad shape. The best we could say is that it was not any more valid than rules of meteorology like the law of rotation of the wind, etc. Perhaps, we could not positively controvert the vitalistic physiologists who maintain that the law of causation is valid in inorganic nature; although in the organic world they relegate it to a lower sphere of action.

Finally, the law of causation bears on its face the character of a purely logical law, chiefly because the conclusions derived from it do not concern actual experience, but its interpretation. Hence it cannot be refuted by any possible experience.[1] For if we founder anywhere in applying the law of causation, we do not conclude that it is false, but simply that we do not yet completely understand the complex of causes mutually interacting in the given phenomenon. And when at length we have succeeded in explaining certain natural processes by the law of causation, the conclusions we derive from it are that certain masses of matter exist and move in space and act on each other with certain motive forces. But the conception of both matter and force are entirely abstract in nature, as is shown by their attributes. Matter

[1] Helmholtz, *Über das Sehen des Menschen, ein populär wissenschaftlicher Vortrag.* Leipzig 1855.

without force[1] is assumed to exist only in space, but not to act or to have any properties. Thus it would be of no importance whatever for all other affairs in the world or for our perceptions. It would be practically non-existent. Force without matter is indeed said to act; but it cannot exist independently, for whatever exists is matter. Thus the two conceptions are inseparable; they are merely abstract modes of regarding the same objects of nature in various aspects. For that very reason neither matter nor force can be direct objects of observation, but are always merely the revealed causes of the facts of experience. Hence, if we conclude by proposing certain abstractions, which can never be objects of experience, as the final and sufficient bases of natural phenomena, how can we say that experience proves that the phenomena have sufficient bases?

The law of sufficient basis amounts simply to the requirement of wishing to understand everything. The process of our comprehension with respect to natural phenomena is that we try to find *generic notions* and *laws of nature*. Laws of nature are merely generic notions for the changes in nature. But since we have to assume the laws of nature as being valid and as acting independently of our observation and thinking, whereas as generic notions they would concern at first only the method of our thinking, we call them *causes* and *forces*. Hence, when we cannot trace natural phenomena to a law, and therefore cannot make the law objectively responsible as being the cause of the phenomena, the very possibility of comprehending such phenomena ceases.

However, we must try to comprehend them. There is no other method of bringing them under the control of the intellect. And so in investigating them we must proceed on the supposition that they are comprehensible. Accordingly, the law of sufficient reason is really nothing more than the *urge* of our intellect to bring all our perceptions under its own control. It is not a law of nature. Our intellect is the faculty of forming general conceptions. It has nothing to do with our sense-perceptions and experiences, unless it is able to form general conceptions or laws. These laws are then objectified and designated as causes. But if it is found that the natural phenomena are to be subsumed under a definite causal connection, this is certainly an objectively valid fact, and corresponds to special objective relations between natural phenomena, which we express in our thinking as being their causal connection, simply because we do not know how else to express it.

[1] ¶The word *force* (*Kraft*) appears to be used here in the sense of *energy*; and in the same sense as it was used in the author's famous paper *Über die Erhaltung der Kraft*, read before the Physical Society of Berlin in 1847. (J.P.C.S.)

Just as it is the characteristic function of the eye to have light-sensations, so that we can *see* the world only as a *luminous phenomenon*, so likewise it is the characteristic function of the intellect to form general conceptions, that is, to search for causes; and hence it can *conceive* (*begreifen*) of the world only as being *causal* connection. We have other organs besides the eye for comprehending the external world, and thus we can feel or smell many things that we cannot see. Besides our intellect there is no other equally systematized faculty, at any rate for comprehending the external world. Thus if we are unable to *conceive* a thing, we cannot imagine it as existing.

The earlier history of the theory of the sense-perceptions is practically the same as the history of philosophy, as given at the end of §17. The investigations of the physiologists of the seventeenth and eighteenth centuries generally did not go beyond the image on the retina, for they supposed that when it was formed, everything was settled. Hence they were little troubled by the questions as to why we see objects erect and why we see them single, in spite of two inverted retinal images.

Among philosophers DESCARTES was the first to take any deep interest in visual perceptions as related to the knowledge of his time. He considered the qualities of sensation as being essentially subjective, but he regarded the ideas of the quantitative relations of size, form, motion, position, duration, number of objects, etc., as something that could be correctly perceived objectively. However, in order to explain the correctness of these ideas, he assumes, as the idealistic philosophers did who came after him, a system of *innate ideas* which are in harmony with the things. This theory was subsequently developed in its most logical and purest form by LEIBNITZ.

BERKELEY made a profound study of the influence of memory on the visual perceptions and their concomitant inductive conclusions. He says concerning them that they take place so quickly that we are not aware of them unless we are deliberately on the watch for them. It is true, this empirical basis led him to assert that not only the qualities of sensation but the perceptions also were mainly merely internal processes having no correspondence with anything outside. What led him into making this false conclusion was the error contained in the proposition that the cause (the object perceived) must be of the same kind as its effect (the idea), that is, must be a mental entity also, and not a real object.

In his theory of the human understanding, LOCKE denied the existence of innate ideas and attempted to establish an empirical basis for all understanding; but this attempt ended in HUME's denying all possibility of objective knowledge.

The most essential step for putting the problem in its true light was taken by KANT in his *Critique of Pure Reason*, in which he derived all real content of knowledge from experience. But he made a distinction between this and whatever in the form of our apperceptions and ideas was conditioned by the peculiar ability of our mind. Pure thinking *a priori* can yield only formally correct propositions, which, while they may certainly appear to be absolutely binding as necessary laws of thought and imagination, are, however, of no real significance for actuality; and hence they can never enable us to form any conclusion about facts of possible experience.

According to this view perception is recognized as an effect produced on our sensitive faculty by the object perceived; this effect, in its minuter deter-

minations, being just as dependent on what causes the effect as on the nature of that on which the effect is produced. This point of view was applied to the empirical relations especially by JOH. MÜLLER in his theory of the Specific Energy of the Senses.[1]

The subsequent idealistic systems of philosophy associated with the names of J. G. FICHTE, SCHELLING and HEGEL all emphasized the theory that idea is essentially dependent on the nature of the mind; thus neglecting the influence which the thing causing the effect has on the effect. Consequently, their views have had slight influence on the theory of the sense-perceptions.

KANT had briefly represented space and time as given forms of all apperception, without going farther and investigating how much might be derived from experience in the more minute formation of individual apperceptions of space and time. This investigation was outside of his special work. Thus, for example, he regarded the geometrical axioms as being propositions in space-apperception which were given to start with;—a view which is not at all settled yet.[2] His lead was followed by JOH. MÜLLER and the group of physiologists who tried to develop the *intuition theory* of space-apperception. JOH. MÜLLER himself assumed that the retina might "sense" itself in its space-extension by virtue of an innate ability for it, and that the sensations of the two retinas are fused together in this case. The one who has recently tried to carry out this view in its most logical form and to adapt it to newer discoveries is E. HERING.

Prior to MÜLLER, STEINBUCH had tried to explain individual apperceptions of space by means of the movements of the eye and of the body. Among the philosophers, HERBART, LOTZE, WAITZ and CORNELIUS attacked the same problem. From the empirical side, it was WHEATSTONE especially who, by inventing the stereoscope, gave a powerful incentive to the investigation of the influence of experience on our visual apperceptions. In addition to minor contributions which I myself have made to the solution of this problem in various works, attempts to give an *empirical* view may be found in the writings of NAGEL, WUNDT and CLASSEN. In the succeeding chapters, more will be said with reference to these investigations and the points of controversy.

1637. CARTESIUS, *Dioptrice.* See Tome V. of V. COUSIN's edition of his Works.
1644. CARTESIUS, *Principia Philosophiae*, T. III.
1703. LEIBNITZ, *Nouveaux essais sur l'entendement humain.* See Vol. I, p. 194 of his *Opera philos.* edited by ERDMANN.
1709. BERKELEY, *Theory of vision.* London.
1720. LOCKE, *Essay on the Human Understanding.*
HUME, *Untersuchungen über den menschlichen Verstand.*
1787. J. KANT, *Kritik der reinen Vernunft.* 2. Aufl. Riga 1787.
1811. STEINBUCH, *Beiträge zur Physiologie der Sinne.* Nürnberg.
1816. J. F. HERBART, *Lehrbuch zur Psychologie.* See Vol. V of his Works published by HARTENSTEIN, Leipzig 1850.
1825. HERBART, Psychologie als Wissenschaft. *Sämtliche Werke.* Bd. VI.
1826. JOH. MÜLLER, *Zur vergleichenden Physiologie des Gesichtssinns.* Leipzig.
1849. TH. WAITZ, *Lehrbuch der Psychologie als Naturwissenschaft.* Braunschweig.
1852. H. LOTZE, *Medizinische Psychologie.* Leipzig.

[1] ¶See E. MINKOWSKI, Zur MÜLLERschen Lehre von spezifischen Sinnesenergien. *Zft. f. Sinnesphysiol.*, 45 (1911), 129-152. (J. P. C. S.)

[2] As is well known, HELMHOLTZ subsequently defended the empirical value of the axioms of geometry with very much greater determination and in opposition to KANT. In another place we shall discuss more in detail the relation between apriority, as KANT intended, and HELMHOLTZ's empiricism.—K.

1856. H. Lotze, *Mikrokosmus.* Leipzig.
1861. Cornelius, *Die Theorie des Sehens und räumlichen Vorstellens.* Halle.
M. J. Schleiden, *Zur Theorie des Erkennens durch den Gesichtssinn.* Leipzig.
A. Nagel, *Das Sehen mit zwei Augen und die Lehre von den identischen Netzhautstellen.* Leipzig u. Heidelberg.
1861-64. E. Hering, *Beiträge zur Physiologie.* Leipzig.
1862. W. Wundt, *Beiträge zur Theorie der Sinneswahrnehmung.* Leipzig u. Heidelberg. Reprinted from the *Zeitschrift für rationelle Medizin* 1858-1862.
1863. A. Classen, *Das Schlussverfahren des Sehaktes.* Rostock.
E. Hering über Dr. A. Classens Beitrag zur physiologischen Optik. *Archiv für pathol. Anatomie und Physiologie.* VIII. 2. p. 179.
1864. C. S. Cornelius, *Zur Theorie des Sehens.* Halle.
J. Dastich, *Über die neueren physiologisch-psychologischen Forschungen im Gebiete der menschlichen Sinne.* Prag.
1866. H. Ulrici, *Gott und der Mensch. I. Leib und Seele, Grundzüge einer Psychologie des Menschen.* Leipzig.

§27. Movements of the Eyes

The movements of the eyes have much to do with the formation of apperceptions of space by the sense of sight; and so it will be necessary now to learn more about them.[1]

The eyeball, indeed, has no regular firm socket made of bone like the joints in the limbs of the body. The socket of the eye as a whole is rather as shown in Fig. 22, Volume I, that is, a recess shaped like a tetrahedral pyramid with its apex toward the rear, which is not conformable in any way to the almost spherically moulded eyeball. The intervals contained between the latter and the bony walls of the orbit are filled up with very fatty, loose connective tissue, where the muscles, nerves, vessels of the eye and tear glands are found. There is only quite a small space left, especially above on the outside and inside, between the eyeball and bone; as may be easily felt by trying to shove the tip of the finger in between. It cannot be done without producing pressure images at the same time. Downwards on the outside toward the cheek-bone the space is somewhat bigger. The consequence is that the soft mass of fat, muscles, nerves, vessels and glands lying behind the eyeball are all comprised in a cavity, which is almost completely surrounded by solid walls, and where only a few small wedges of more yielding substance are to be found. On the sides and behind, this cavity is formed by the bony walls of the eye-socket, while it is completed above by the eyeball itself. The conglomeration of fat, muscles, nerves, etc., mentioned above being almost entirely incompressible, like the water that constitutes the bulk of its weight,

[1] ¶A valuable book to be consulted in connection with this chapter is Dr. Ernest E. Maddox's *Tests and studies of the ocular muscles.* (Third edition, specially revised and enlarged by the author and published in Philadelphia, U.S.A. 1907). (J.P.C.S.)

cannot shrink or increase appreciably in volume. Thus, in the first place all the movements of the eyeball are subject to the condition that the volume of the portions behind the eyeball cannot be altered.

Under normal conditions, therefore, the eyeball cannot go deeper in the socket or protrude from it, at least not as a result of the rapidly alternating contractions of its own muscles. Of course, when blood pours into the vessels of the socket or is discharged from them more than normally, as, for instance, in wasting diseases and in death, the result is a change of volume of the soft parts lying behind the eyeball, causing the latter to protrude or recede. But such changes cannot be produced by voluntary movements of the eye. Any attempt to force the eyeball back in its socket by digital pressure will be met with a considerable resistance even before any appreciable displacement of the eye has occurred; and the subjective phenomena produced by pressure on the eye will be noticed at once. In this case the soft parts, especially below the eyeball, will be seen to protrude; but on releasing the pressure, they recede again by virtue of their elastic tension.

It is just as impossible to move the eyeball as a whole to one side or the other, or up or down, because the adjacent parts of the anterior bony edge of the socket are in the way.

Accordingly, any displacement of the eyeball as a whole, that is, any displacement in which every point of the eyeball is moved in the same direction, is rendered impossible. Consequently, the only movements that can possibly be executed are *rotations*, or movements by which one side of the eyeball enters the ocular cavity while another side emerges from it. On the whole, therefore, the way in which the eyeball is embedded, so far as its movements are concerned, is mechanically the same as if it were a spherical ball-and-socket joint, like the joint of the upper thigh.[1]

The eyeball, therefore, being capable only of rotatory movements, the first question is with respect to the *centre* of these rotations.

Professor Junge, of St. Petersburg, working in my laboratory, has endeavoured to determine the centre of rotation of the eye, by observing how much the luminous reflexes in the corneas of the two eyes approached each other when the visual axes were converged from parallelism to a definite angle of convergence. But it developed that the ellipticities of the corneal surfaces had an appreciable effect on the calculation of the results; and as it is very troublesome to determine this ellipticity for a large number of eyes, the method was not capable

[1] Concerning motions of another kind, see Note 1 at the end of this chapter.—K.

of being used extensively; although as far as it went, the results obtained were very accurate.

DONDERS and DOIJER[1] used, therefore, a simpler method, which proved to be accurate enough. First, the horizontal diameter of the cornea was measured with the ophthalmometer, and the position of the visual axis found with respect to the corneal axis. Then a fine vertical thread was stretched directly in front of the eye, and the observation consisted in ascertaining how far the eye had to look to the right and left in order for first one edge and then the other edge of the cornea to coincide with the thread. From this angle and the known amplitude of the rotations, the position of the centre of rotation could then be computed. The method will be explained more fully presently.

The result they found was that for 19 emmetropic eyes the centre of rotation was between 10.42 and 11.77 mm beyond the plane passed through the edge of the cornea, the average being 10.957 mm; that is, 13.557 mm beyond the vertex of the cornea and about 10 mm in front of the posterior surface of the sclera; accordingly, somewhat nearer the latter than the base of the cornea. The position of the centre of rotation depends mainly on the form of the posterior half of the eyeball, just because it is only this half that comes in contact with the soft resisting cushion that occupies the base of the socket of the eye. In normal eyes this posterior half of the eyeball seems to be part of a more flattened ellipsoid than the anterior half. The centre of rotation has to coincide about with the centre of this ellipsoid.

Near-sighted eyes are elongated posteriorly. In them, therefore, the centre of rotation is farther back than it is in emmetropic eyes. The greatest value, as found by DONDERS, was 13.26 mm beyond the base of the cornea, that is, 15.86 mm beyond its vertex. Hypermetropic eyes, on the other hand, are flatter behind, and hence also the centre of rotation is a little more to the front. The least value found for its distance from the base of the cornea was 9.71 mm or 12.32 mm beyond the vertex of the cornea.

Whether the position of the centre of rotation is absolutely the same for every direction and magnitude of rotation, has not yet been investigated by DONDERS.[2]

[1] Derde Jaarlijksch Verslag betr. het Nederlandsch Gasthuis voor Ooglijders. Utrecht 1862, p. 209-229.

[2] Concerning other determinations of the centre of rotation of the eye, see Note 2 at the end of this chapter.—K.

¶It may be noted here that as early as 1842 L. J. SCHLEIERMACHER had insisted that the eye turned around a fixed centre which he inferred was between 12.14 mm and 12.61 mm from the vertex of the cornea (see H. BOEGEHOLD, *Zft. f. ophthalm. Optik*, VIII. 1920. 1-10. While the most careful and accurate investigations (as described by Professor v. KRIES at

Another thing that was shown by these experiments was that in the case of normal eyes (with one single exception), there was no difficulty in executing the rotations, amounting to 28° on both sides, that were necessary for making the measurements. However, the power of movement of myopic eyes was apt to be less, although this was seldom the case with hypermetropic eyes. In fact, most eyes can execute even still greater rotations. By making considerable effort I can turn my eyes about 50° horizontally either way, and about 45° up or down. Thus, I have a range of about a right angle vertically and rather more than that horizontally. However, the extreme excursions require much effort and cannot be long maintained.

Let us see now what rotations the eyeball is capable of. There is nothing in the mode of fastening of the eyeball to prevent every sort of rotation of moderate amplitude. There are muscles also by which the eye can be turned about any axis. However, careful investigation has shown that, under the ordinary conditions of normal vision, the human eye does not actually perform all the movements by any means that it is capable of executing, so far as its mechanical contrivances are concerned. Accordingly, the next question for us to investigate is, What motions are actually made by the human eye?

In the determinations of the positions of the eyes and of the objects seen by them, the problem usually consists in finding how they are oriented with reference to the head, the position of the latter and its attitude in space being supposed to be known. For these relations the following nomenclature, as employed by HENLE for anatomical descriptions, will be found to be most convenient.

The human head consists of two symmetrical halves. Its middle plane of symmetry may be termed the *median plane*. Lines joining corresponding points of the right and left halves of the head will be called *transversal lines* (*quere Linien*). They are perpendicular to the median plane. Planes parallel to the median plane are called *sagittal sections*.

The natural position of the head may be regarded as that which is assumed when the body is erect, and the look is directed toward the horizon. When I assume this position, the *glabella* of the frontal bone in the forehead (the part just over the root of the nose) is vertically above the upper teeth. This position as thus defined is, of course, not

the end of this chapter) indicate that the average eye does not turn about an absolutely fixed centre, still the axes of rotation are found to pass very close to some such point lying about 13 mm beyond the vertex of the cornea, which for practical purposes may be regarded as being the centre of rotation of the eye. (J.P.C.S.)

indicated perfectly exactly, but only approximately. It will be shown presently how we can make a more exact determination of the ocular movements. The horizontal planes passed through the head when it is in this position are called *horizontal sections* or *transverse sections*; whereas the vertical planes at right angles to the median plane are called *frontal sections*. The frontal and transverse sections intersect each other in *transversal* lines. The lines in which the median plane and the sagittal sections parallel to it are cut by the transverse (horizontal) sections are called *sagittal* (*pfeilrechte*) *lines*; and those in which the median plane and the sagittal sections are cut by the frontal sections are called *vertical* (*senkrechte*) *lines*. Hence, the *transversal* lines run from right to left, the *sagittal* lines from in front to behind, and the *vertical* lines from above downwards.

In this way we obtain a rectangular system of coördinates, which is fixed in the head itself and is considered as being movable with it. The two sides of the median plane are to be distinguished as *right* and *left*. The two sides of a sagittal plane are to be distinguished as *inner* and *outer*; unless this should result in confusion with respect to the inside of hollow organs, in which case the two sides will be denoted, as HENLE suggested, as *lateral* (looking to the outer side) and as *medial* (looking towards the median plane). The two sides of the transverse section may be denoted as *upper* and *lower*; or in case this might be ambiguous when the head is tilted, we can say, turned *brow-wards* and *chin-wards*. The two sides of the frontal section are to be called simply *front* and *back*.

So far as the ocular movements are concerned, the centre of rotation is the fixed point. In normal vision, both eyes are always adjusted so as to fixate one and the same external point. Vision with the mobile eye is called *looking* (*Blicken*), and hence this external point is called the *Blickpunkt* or the *point of fixation*. A straight line drawn from the *point of fixation* to the *centre of rotation* of the eye is called the *line of fixation*. The line of fixation is not quite the same as the *visual axis* corresponding to the unrefracted ray of light,[1] but must be a little on its inner (medial) side, since the centre of rotation is presumably on the optical axis and therefore toward the median plane from the visual axis. Still in most cases the distinction between these two lines is negligible. A ray of light starting along the line of fixation, like all rays coming from the point of fixation, must ultimately go through the *fovea centralis*, and hence it cannot continue along the prolongation of the line of fixation.

[1] ¶That is, the line drawn through the point of fixation and the nodal point of the eye. (J.P.C.S.)

A plane passed through the lines of fixation of both eyes is called the *plane of fixation* (*Blickebene*). (The name *visual plane* [*Visierebene*], has also been used for this plane, but it is better to reserve this term to denote the plane containing the *lines of sight* [*Visierlinien*] of the two eyes. Incidentally, the difference between *Blickebene* and *Visierebene* is usually negligible.) The line joining the centres of rotation of the two eyes is to be regarded as the base of the triangle of which the lines of fixation are the other two sides—and is called, therefore, the *base-line*. The median plane of the head bisects the base-line, and intersects the plane of fixation in the *median line* of the latter plane.

The point of fixation may be raised or lowered, that is, displaced upward toward the brow or downward toward the chin. The field over which it may be moved is called the *field of fixation*; the extent of this field, however, being less than that of the field of view.[1] The field of fixation is regarded as being part of a spherical surface whose centre is at the centre of rotation. If a certain position of the plane of fixation, chosen arbitrarily at first, although presently it may be defined more precisely, is assumed to be its initial position, every new position of this plane will be given by the angle it makes with the initial position. This angle will be called the *angle of elevation* of the look. It is reckoned as positive when the plane of fixation is raised toward the brow, and negative when it is lowered toward the chin.

The line of fixation of each eye may be turned in the plane of fixation "laterally" or "medially." Such movements are called *lateral displacements*, and are measured in terms of the *azimuth angle*, that is, by the angle made by the direction of the line of fixation with the median line of the plane of fixation. Deviations to the right being reckoned as positive, those to the left will be counted, therefore, as negative.

The direction of the line of fixation is given by the *angle of elevation* and the *azimuth angle*. Fick, Meissner, and Wundt used two other angles for this purpose. In my measurements the line of fixation is first elevated with the plane of fixation and then turned sideways in this plane. Fick supposes the plane of fixation to be horizontal at first, and the line of fixation to be turned in this plane through an angle which he calls its *longitude*, the vertical axis of the eye being therefore analogous to the polar axis of a terrestrial globe. Then he lets the line of fixation ascend through an angle called its *latitude*. But in this measurement the values of both *longitude* and *latitude* depend on the initial position that is chosen for the plane of fixation; and as there is no satisfactory way of defining a fixed initial position

[1] ¶A. v. Rötth, Über das praktische Blickfeld. *Arch. f. Ophthalm.*, 115 (1925), 314-321. (J. P. C. S.)

of this plane, every time it is changed trigonometrical calculations have to be made for the other two angles. On the other hand, the *azimuth angle* used in my work is entirely independent of the primary position of the plane of fixation; while the *angle of elevation* merely has to be corrected by addition or subtraction when it is measured from a new origin.

Although the position of the line of fixation is completely given by the angles thus defined, this does not suffice to determine the adjustment of the eye; because the eyeball still might be able to turn any way whatever around the line of fixation as axis, without modifying at all the position of this line. Such rotations of the eyeball around the line of fixation as axis are generally called *torsional-rotations* (*Raddrehungen*) [or *rollings*], because in this case the iris rolls round like a wheel (*ein Rad*). In order to measure the magnitude of this particular movement, the angle has to be found between a fixed plane in the eye and the plane of fixation. For this fixed plane I have chosen the plane coinciding with the plane of fixation when the head is held erect and the two eyes look out in directions parallel to the median plane toward the far off horizon. This fixed plane in the eye is the so-called *retinal horizon*. Both in my own eyes and in the normal eyes which I have measured, this mode of determination was found not to involve any uncertainty. However, this is not so in the case of near-sighted eyes, as was afterwards discovered; and, therefore, in such cases a perfectly definite initial position of the plane of fixation must be prescribed. Or else, considering the applications to be made presently, it might be better, perhaps, in the case of such eyes to use that position of the plane of fixation in which straight lines lying in this plane are reproduced at corresponding places on the retinas of the two eyes. The latter seems to be generally the case with emmetropic eyes when they look in a direction parallel to the median plane, as above defined. The angle between the *retinal horizon* and the *plane of fixation* is called the *angle of torsion* (*Raddrehungswinkel*) of the eye; being reckoned as positive when the upper portion of the vertical meridian of the retina is deviated toward the right. Then the eye turns the same way as the hands of a clock at which it is looking.[1]

Let us proceed now to study the laws of motion of the two eyes for the special case when the two lines of fixation continue always parallel to each other, as when a row of far distant objects is being surveyed. When the eyes converge, there are slight departures from the law that is found to hold for parallel lines of vision.

[1] With reference to methods of terminology of torsional rotation (*Raddrehung*) and the angle of torsion, see Note 3 at the end of this chapter.—K.

The law, given first by DONDERS and confirmed by all subsequent investigations, is that *when the position of the line of fixation is given with respect to the head, the angle of torsion will invariably have a perfectly definite value for that particular adjustment*; which is independent not only of the volition of the observer but of the way in which the line of fixation arrived in the position in question.[1] Expressed in terms of our nomenclature, this law may be stated as follows:

When the lines of fixation are parallel, the angle of torsion in the case of each eye is a function simply of the angle of elevation and the angle of azimuth.

Particularly as opposed to the view previously advanced by HUECK, DONDERS pointed out that the magnitude of the rolling motion is not varied by altering the inclination of the head, provided the position of the line of fixation with respect to the head remains the same.[2] The adjustment of each eye was regarded also as being independent of that of the other eye. However, VOLKMANN demonstrated that, for near-sighted eyes at least, convergence certainly has some influence, slight as it may be. This will be referred to again. But even apart from that, fatigue of the ocular muscles, due to long-continued convergence-adjustments, does have some influence. Besides, under special conditions, which will likewise be considered presently, the effort to see objects singly, in cases where it can only be done by abnormal twistings of the eyes, may exert, not immediately, but after a while, an influence on the adjustment of the eye. Minute variations occur also from day to day. But all these deviations are slight, and in the main do not affect the validity of DONDERS' law.

The essential facts concerning the law of ocular rotations, that are common to all eyes, may be summarised in the following statements.

Among the various positions the eyes can have, one can be found such that when the eyes turn from it to look straight up or straight down, or straight to the right or left, there will be no rolling of the eye. This position is known as the *primary position of the line of fixation*. Starting, therefore, from this position, *no torsional rotation is involved in simply raising or lowering the eye without moving it to one side, or in moving it to one side without raising or lowering it.*

The position of the plane of fixation that passes through the primary

[1] ¶DONDERS' law is equivalent to the statement that the eye's freedom of movement is limited by the direction of the line of fixation. If this were not the case, a given object viewed with the head in a certain position relative to it would not always be depicted on the same elements of the retina. (J.P.C.S.)

[2] Subsequent researches have shown that, contrary to what is here stated, rolling motions do occur when the head is tilted. See Note 4 at end of this chapter.—K.

positions of the lines of fixation of both eyes is called the primary position of this plane.

When the plane of fixation is in an elevated position, lateral movements to the right cause the eye to turn to the left, and vice versa.

On the other hand, the case is exactly the reverse when the plane of fixation is in a lowered position.

In other words: *When the angles of elevation and azimuth have the same sign, the rotation of the eye is negative; and when these angles have opposite signs, the rotation is positive.*

For a given degree of elevation or depression, the amount of rotation increases with the degree of lateral displacement; and for a given lateral displacement, it increases with the degree of elevation or depression.

The best way of verifying these facts is by using after-images, as was originally suggested by RUETE. The way to do it is to stand opposite a wall covered with paper on which horizontal and vertical lines can be distinguished, the pattern, however, not being so sharply outlined as to make it difficult to see after-images on it. The best background is one of a smooth pale grey colour. Directly opposite the observer's eye, and on the same level with it, a black or coloured ribbon is stretched horizontally, two or three feet long; standing out in sharp contrast to the colour of the wall-paper. In order to secure the position of the head, it is a good plan to lean firmly against the back of it, taking care at the same time not to tilt or turn it to one side or the other. The median plane of the head should be kept vertical and perpendicular to the opposite wall. It is easy to tell whether this plane is vertical, by letting the eyes converge until there are double images of the black band—which must both lie in one straight line. Now let the observer look intently for a little while at the middle of the band, and then, without moving his head, turn his eyes suddenly to another place on the wall. An after-image of the band will appear there, and by comparing it with the horizontal lines of the wall paper, the observer can see whether the after-image is horizontal or not. The after-image itself is developed on those points of the retina belonging to the retinal horizon; and during the motions of the eyes it indicates those parts of the visual field on which the retinal horizon is projected. On the other hand, the line of intersection of the plane of fixation with the opposite wall must always be horizontal, provided the observer's head is in the proper position, such that the line joining the centres of rotation of the two eyes is itself horizontal and parallel to the wall. Accordingly, the horizontal lines on the wall-paper show the projection of the plane of fixation on the wall; and the way the after-image is

oriented with respect to these horizontal lines shows how the retinal horizon has been turned with respect to the plane of fixation.

If the head has been properly adjusted, on looking straight up or down, or to the right or left, we find that the after-image of the horizontal band coincides with the horizontal lines of the wall-paper. But if the look is directed *upward and toward the right*, or *downward and toward the left*, the after-image, always as compared with the horizontal lines on the wall-paper, will be turned toward the *left*; that is, the left-hand end will be lower than the other. And on looking *upward and toward the left*, or *downward and toward the right*, the rotation of the after-image is just the other way; that is, somewhat toward the right, with its right-hand end lower than the other.

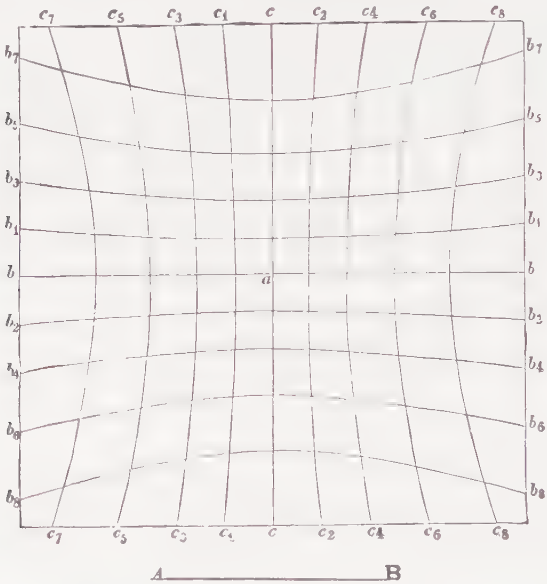

Fig. 1.

The sense of these rotations is exactly the same for both eyes. This can be most easily and completely verified by keeping both eyes open together while the after-image is being produced; then changing the direction of the look, and, while viewing the after-image, quickly covering first one eye and then the other with the hand. No matter which eye is covered (in case of the emmetropic eyes which I have investigated), the after-image maintains the same position perfectly.

If the ribbon is stretched vertically, and the after-image compared in the same way with the vertical lines of the wall-paper, the rotations

are apparently opposite. For instance, in looking upward and toward the right, the after-image does not appear to be turned toward the left with respect to the vertical lines of the wall-paper; but, on the contrary, toward the right. However, we cannot infer from this that the eye turns toward the right, because in this case the vertical lines of the wall-paper do not coincide with the projection of a line perpendicular to the plane of fixation. Such a line would appear rather to be rotated in the same sense as the after-image, only more so.

The whole process, according to the law that holds for emmetropic eyes, is exhibited in Fig. 1. The eye is supposed to be situated on the perpendicular to the plane of the paper at a distance equal to AB from the point a. In this case the after-images of a horizontal line passing through a, when projected on another part of the field, will coincide with the directions of the curves b_1b_1, b_2b_2, etc.; and the after-images of a vertical line passing through a will coincide, on the other hand, with the direction of the curves cc, c_1c_1, c_2c_2, etc. For normal ocular measurements these curves are hyperbolas.

Since now, in starting from the primary position and looking obliquely up or down, the after-images of vertical lines as compared with vertical lines on the wall apparently undergo a rotation opposite to that of the horizontal after-images as compared with horizontal lines on the wall, it may be conjectured at once, that for every ocular movement there is some direction of the after-image in between horizontal and vertical for which it remains parallel to the direction of the object itself. And in fact this is the case. It is found, for instance, that the after-image of a slant line fixated in the primary position remains parallel to the object, provided the eyes are made to look either along the prolongation of the object-line, or, starting from the primary position, along a line perpendicular to the object-line.

Thus, in Fig. 2, suppose that o is the point where the line of fixation in the primary position meets the plane of the diagram; and let aa and bb be a vertical and a horizontal line, respectively, drawn through o. If the eyes are turned so as to look toward p, the after-images of aa and bb will take the positions $\alpha\alpha$ and $\beta\beta$, respectively, neither being parallel to the original lines. But if the pair of rectangular lines cc and dd are drawn through o in such manner that cc has the same direction as the line joining o with p, the after images $\gamma\gamma$ and $\delta\delta$ will be parallel to cc and dd, respectively.

According to my experience, this law seemed to be more perfectly obeyed, the less near-sighted the eyes were.

In the experiment indicated in Fig. 2, the result, therefore, is found to be that, in turning the eyes to look at p, the lines $\delta\delta$ and $\gamma\gamma$

are imaged on the same parts of the retinas on which *dd* and *cc* are imaged when the eyes are directed toward *o*. Suppose we were to ask, How would the eyeball have to be turned to bring it from the first to the second position? The answer evidently is, that the axis of rotation must be parallel to the pair of lines *dd* and $\delta\delta$, and hence perpendicular to the plane passing through *op* and the centre of rotation of the eye. If this plane is supposed to be fixed with respect to the eyeball, its position will not be altered when it is turned with the eyeball around an axis perpendicular to it. Hence the line *op* in which it cuts the plane of the diagram will likewise remain the same during this movement, and so this line, to which also the segments *cc* and $\gamma\gamma$ belong, will be imaged always on the same parts of the retina, as required by the results of the experiment. But if a plane is supposed to be passed through the axis and the line *dd* parallel to it, and to be turned around the axis, at the end of the rotation the line $\delta\delta$ in which this plane intersects the plane of the diagram will also have to remain parallel to the axis, and therefore parallel also to the line *dd*. For when a plane contains a straight line (axis of rotation) that is parallel to another plane (plane of the diagram), the line of intersection of the two planes will be parallel to the said line (axis of rotation).

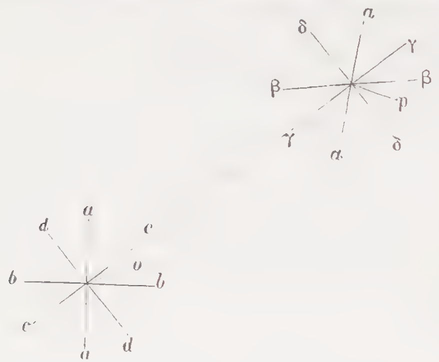

Fig. 2.

Accordingly, for the case of a pair of emmetropic eyes with parallel lines of fixation, the law of their motion may be stated as follows: *When the line of fixation is brought from its primary position into any other position, the torsional rotation of the eyeball in this second position will be the same as if the eye had been turned around a fixed axis perpendicular to the initial and final directions of the line of fixation.*

This is known as LISTING's law of ocular movements, because it was first stated by him in this form.

The law does not mean that the look actually has to proceed from the initial to the final position along a straight line, or that the eyeball really does turn around a fixed axis. On the contrary, the passage from the initial to the final position may be accomplished in any way

at all. But according to DONDERS's law, the final position is invariably the same; and the truth of this latter law is shown once more by the fact that, although the look may be transferred deliberately along different routes, the after-image $\gamma\gamma$ will be congruent with the line op; that is, the resultant rolling movement of the eye will always be the same.

Still in this case it should be remarked at least that at the first moment, when the line of fixation, after having made a variety of movements, has arrived at the newly selected point of fixation, sometimes the after-image will be noticed to have a little different position; but in a second or two it will assume the normal position.

If, according to LISTING's law as verified by experiments of this sort, the magnitude γ of the angle of rotation is calculated in terms of the angle of elevation (α) and the angular lateral displacement (β), the following equation will be obtained:

$$-\tan\gamma = \frac{\sin\alpha\sin\beta}{\cos\alpha+\cos\beta};$$

which may be put in the following form more convenient for logarithmic computation:

$$-\tan\frac{\gamma}{2} = \tan\frac{\alpha}{2}\cdot\tan\frac{\beta}{2}.$$

In the subjoined table the values of the angle of rotation are given for values of the other two angles for every 5° up to 40°.

Angle of Elevation

Lateral Displacement	5°	10°	15°	20°	25°	30°	35°	40°
5°	0° 13′	0° 26′	0° 40′	0° 53′	1° 7′	1° 20′	1° 35′	1° 49′
10°	0° 26′	0° 53′	1° 19′	1° 46′	2° 13′	2° 41′	3° 10′	3° 39′
15°	0° 40′	1° 19′	1° 59′	2° 40′	3° 21′	4° 2′	4° 45′	5° 29′
20°	0° 53′	1° 46′	2° 40′	3° 34′	4° 29′	5° 25′	6° 22′	7° 21′
25°	1° 7′	2° 13′	3° 21′	4° 29′	5° 38′	6° 48′	8° 0′	9° 14′
30°	1° 21′	2° 41′	4° 2′	5° 25′	6° 48′	8° 13′	9° 39′	11° 8′
35°	1° 35′	3° 10′	4° 45′	6° 22′	8° 0′	9° 39′	11° 21′	13° 6′
40°	1° 49′	3° 39′	5° 29′	7° 21′	9° 14′	11° 8′	13° 6′	15° 5′

Thus when the eyes turn from the primary position to look in any other direction, according to LISTING's law, the axis of rotation will always lie in a plane perpendicular to the line of fixation. Suppose

this plane containing the axes of rotation passes through the line AA (Fig. 3) which is perpendicular to the line of fixation OB. Consider a second plane P passing through the eyeball and rigidly connected with it, which coincides with the plane AA when the eye is in the primary position. Now if the line of fixation OB is turned into another position OF, the plane P will assume the position represented by CC. In order to pass from this first secondary position into some other position, the eye may now turn again around fixed axes all lying in one and the same plane, that is, *in that plane which bisects the angle between the planes AA and CC*, and which therefore intersects the plane of the diagram at right angles in the line HH. This is the plane of the axes of rotation for the given secondary position of the line of fixation OF.

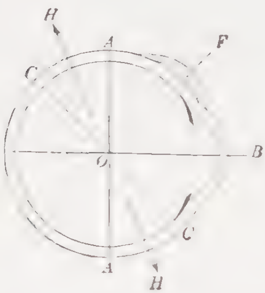

Fig. 3.

Finally, in order to pass from any position (a) of the eyeball to some other position (b), the planes of the axes of rotation should be constructed for the two positions (a) and (b). The line of intersection of the two planes is the axis about which the eye has to turn to go from (a) to (b). For, obviously, this axis must lie in both planes, since the same movement can be made also from (b) to (a), and the axis in question must satisfy the condition of movement equally, whether it starts from (a) or from (b), that is, it must lie in the planes of the axes of rotation belonging to both points of fixation.

In the tests that were made with emmetropic and slightly near-sighted eyes, LISTING's law was found to be very accurately true for all parallel positions of the lines of fixation of the two eyes. When it is properly carried out, the method of after-images enables us to determine the position of the eyeball accurately to within about half a degree. Another method, based on comparing the images in the two eyes, which was used first by MEISSNER, and subsequently elaborated by VOLKMANN, gives more accurate determinations still, to within about one-tenth of a degree. However, it does not determine the position of each eyeball by itself, but the differences of position of the two eyes. Experiments by this method will be described more fully farther on. The results obtained with my own eyes indicate departures from LISTING's law for the most extreme up and down peripheral positions, amounting for each eye separately to only nine minutes of arc. VOLKMANN, who was a little near-sighted, found the greatest deviations for his eyes when he looked obliquely downward to one

side or the other, amounting to 54 minutes for both eyes, which is equivalent to about 27 minutes for each eye separately. But in the case of more near-sighted eyes, like those of Dr. BERTHOLD, the differences were greater, particularly in the peripheral positions above and below. This may have been due to some mechanical obstruction to the movement of the near-sighted eyeball, which is elongated posteriorly.[1]

All the preceding cases have had to do with positions in which the lines of fixation of both eyes were parallel. VOLKMANN discovered appreciable discrepancies, variable in amount for different individuals, when the lines of fixation were converged in looking at a near object. In his own case he found that when he converged his eyes on points in a plane 30 cm away, a uniform increase of divergence of two degrees was produced in the apparently vertical meridians of the two eyes, as compared with the divergence those meridians should have had according to LISTING's law; assuming the same divergence and the same primary position as had been found when the positions of the eyes were parallel. Thus, so far as the effect of convergence can be seen in the altered difference of position of the two eyes, it might be supposed that when VOLKMANN's eyes were converged, the primary position was lower, or that the rotation of the eye in the primary position, which was taken as the origin of the rolling movements, was altered. This alteration increases with increased convergence.

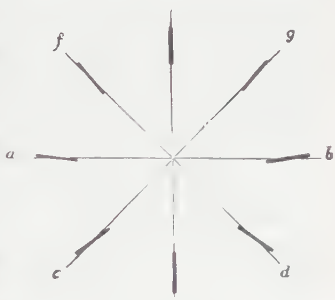

Fig. 4.

In my own case this ocular rotation due to convergence was much less in the central portions of the visual field than it was with VOLKMANN's eyes, being only about one-ninth as much, and therefore not being revealed at all in my experiments with after-images. On the other hand, in those experiments I did find that, when the eyes were directed sideways near the periphery, there were deviations of the after-image as a result of convergence amounting to between two and two and one-half degrees, in the same sense as if the primary position of my eyes for the positions of convergence were assumed to be a little lower than when the lines of fixation were parallel. The short thick lines in Fig. 4 represent the positions of the after-images when the eyes are converged, the amount of the deviation being exaggerated

[1] Concerning this, see Note 5 at end of this chapter.—K.

however. The objects by which the after-images were produced were situated at the centre, being placed parallel to the radii of the visual field that are shown in the diagram, and hence their after-images, in case the lines of fixation were parallel, continued to lie along these radii. The deviations are most marked at c and d, and least conspicuous at f and g.

Mr. Dastich, who succeeded very well with other observations of this kind, was not able to detect the slightest effect of convergence so far as his own vision was concerned. Accordingly, further investigations are needed as to the magnitude of this effect in the case of different individuals.[1]

I must add also that in my own case there is a certain variability in the ocular rotations. The primary position is a little higher on one day than on another, and in fact it fluctuates during the progress of a series of experiments. When the eyes are directed out toward the periphery, some straining is involved, and then especially I sometimes find noticeable differences of position in experiments made one just after the other, even when they are conducted as nearly as possible exactly in the same way. Accordingly, we must not expect quite the same precision in the eye as in a scientific instrument, although under ordinary conditions normal eyes do obey the laws of Donders and Listing pretty accurately.

Lastly, we must see how the separate ocular muscles contribute to the individual normal ocular movements. As has been stated (Vol. I, p. 38), the internal and external recti, acting by themselves, tend to turn the eye around a vertical axis. According to Ruete's findings, the axis about which the eye is turned by the superior and inferior recti is horizontal and makes an angle of about 70° with the line of fixation; its inner end being toward the front of the eye. The axis of the oblique muscles is likewise horizontal and makes an angle of about 35° with the line of fixation;[2] its outer end being toward the front. Rotations around the vertical axis produced by the internal and external recti are in accordance with Listing's law, and hence this pair of muscles may act alone. But rotations around the other two axes are not in accordance with this law. In order to produce an upward motion of the eye by rotation around an horizontal axis extending from right to left, there must be a combination of a rotation produced by the superior rectus with one produced by the inferior

[1] See Note 6 at end of this chapter.—K.

[2] ¶The word in the text is *Blicklinie*; but what is meant here is the optical axis of the eye. (J.P.C.S.)

oblique muscle; and for a downward movement the inferior rectus and the superior oblique muscle must act together. According to a familiar law of kinematics, when the rotations are small, the axes can be compounded by a rule which is the same as that for the parallelogram of forces; the angular displacement corresponding to the magnitude of the force, and all rotations, as viewed from the centre of rotation, being reckoned as positive when they take place toward the right (clockwise), and as negative when they occur in the opposite sense. Fig. 5 represents an horizontal section of the eye, showing the axes of rotation in this section. The positive ends of the axes are marked with the initial letters of the muscles concerned (*obliquus superior* and *inferior*, *rectus superior* and *inferior*). Moreover, the horizontal axis OU is shown that is required by LISTING's law for up and down movements; the letter O (*oben*) indicating the positive end of the axis for upward rotation and the letter U (*unten*) the positive end for downward rotation. The diagram represents the left eye as seen from above, or the right eye as seen from below.

Now if the length cb is proportional to the angular displacement produced by the superior rectus, and the length ca to that produced by the inferior oblique muscle, the diagonal cO of the parallelogram $cbOa$ will indicate the direction of the resultant axis of rotation and the amount of the angular displacement with respect to it. It is obvious from this diagram that in those positions of the axes when the eyes are directed straight ahead, the resultant axis of rotation UO is nearer to the axis of rotation of the inferior and superior recti than to that of the oblique muscles. That is why the side cb of the parallelogram is longer than the side ca, because the pair of recti involved here have to exert more force than the pair of oblique muscles. If, however, the eyeball is turned inward, the axis of rotation UO corresponding to the new position of vision comes nearer to the axis of the oblique muscles; and hence when the eyes are converged, the latter muscles have more to do than when the lines of fixation are parallel.

In this connection it should be remarked that the ocular muscles all have a fairly broad insertion on the eyeball, their fibres being spread out over it somewhat in the form of a fan. The consequence is that even when the eyeball has been turned considerably out of its primary position, the axes of rotation of the separate muscles do not change their positions in space much. For example, consider the superior and inferior recti, which are inserted above the cornea some 7 mm away from its edge (at m and n in Fig. 2 of Vol. I). When the eye is turned inward and the muscle shortened, it is mainly the fibres of the tendon directed toward the outer edge of the cornea that are stretched,

because most of them are elongated. This can easily be seen in anatomical preparations of the eyeball with its muscles. On the other hand, when the eye is turned outward, it is mainly the internal fibres of the two tendons that come into play. Thus, although the position of the eye is changed, the direction of the muscular pull continues the same.

These conclusions based on the mode of attachment of the muscles are confirmed by observations of individual muscles which have become paralyzed by disease. For instance, if the superior oblique muscle is paralyzed, the internal rectus acting by itself can still turn the eye downward. But rotation around the axis *RI* does not simply produce the desired rotation around the axis *UO* corresponding to the vector *cg* in Fig. 5, but at the same time it causes a minor rotation around *CH* as axis, which is represented by *cf*; that is, a negative rotation taking place around the line of fixation toward the left. The result is that the objects in the visual field undergo an apparent rotation to the right, that is, clockwise.

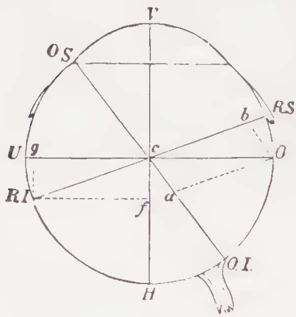

Fig. 5.

For oblique upward or downward movements from the primary position a component with respect to the axis *UO* must be connected with a vertical component. In order to turn the eye in and up, we need the internal rectus, which turns the eye in around the vertical axis, and at the same time the superior rectus and the inferior oblique muscle which unite to turn the eye upward around the axis *UO*.

These combinations may readily be understood from the diagram Fig. 5. But rotating models of the eye or so-called *ophthalmotropes*, which will be described later, enable us to do so more conveniently.

Aside from the limitations of the motion of each eye by itself, which have already been mentioned, the movements of the two eyes are also to some extent dependent on each other, just as accommodation is dependent on the adjustment of the eyes. Under the ordinary conditions of normal vision, we always direct both lines of fixation to a real point in the space in front of us, either near or far away. The lines of fixation of the two eyes meet in this *point of fixation*. Although each eye has its own independent muscular mechanism, and so possesses the possibility of executing every kind of movement entirely without regard for the other eye, still we have only learned actually how to execute such motions as are required for single and distinct

vision of a real point by both eyes. Thus both eyes can be raised together so as to look at a high point of fixation; and they can both be lowered together in order to observe an object that is down below. But without other means of assistance, we are not able to raise one eye and lower the other voluntarily; in which case the lines of fixation would not intersect in any real point of fixation.

Moreover, both lines of fixation can be turned either to the right or to the left, in order to see an object situated on one side or the other. And we can also make the eyes convergent, by turning the right eye to the left and the left eye to the right, in looking at a near point of fixation. But without special previous practice, nobody can make the two lines of fixation divergent, by turning the right eye to the right and the left eye to the left.

Lastly, normal eyes invariably accommodate for the distance of the point of convergence where the two lines of fixation intersect. When the latter are parallel, the eyes are accommodated for infinity. When the eyes are converged, they are accommodated for near vision; and as the convergence increases, so does the accommodation also. On the other hand, near-sighted eyes accommodate for their far point, as long as the lines of fixation intersect there or at some point more remote still. For nearer points of fixation accommodation and convergence go hand in hand. However, extremely near-sighted eyes are frequently unable to achieve binocular fixation and accommodation at all without glasses.

Although the impulse to move both eyes in harmony and to adjust the accommodation accordingly is apparently so unavoidable in normal vision that the earlier physiologists regarded these movements as belonging to the class of involuntary concomitant movements, yet it may be shown that the regularity of these associations is simply a matter of training. Generally speaking, it must be borne in mind here, that with all voluntary movements the invariable intent of our will is simply to achieve some directly and distinctly perceptible external result. In moving our limbs the sense of sight certainly does enable us to perceive how the member is adjusted by a certain act of volition; and therefore for them and for all parts of the body that can be perceived by sight and touch the adjustment of the part in question is the first conscious purpose of acts of volition in that direction. But in case of those parts of the body that cannot be seen or touched, it is not so much the position and motion itself as it is the result to be gained by it, which we know how to produce by a voluntary action. Thus we use the larynx and parts of the mouth with marvellous certainty and adroitness to produce the most delicate variations of

pitch and *timbre* of the voice in singing and speaking; and yet most people do not understand at all, and even physiologists themselves very imperfectly, what sort of peculiar movements are executed in this case. Here, therefore, the intent of the will is concerned simply with the tone to be produced and not with the movement of the separate parts of the larynx. We have learned to execute all such movements of the larynx as are necessary for the purpose, but no others.

It is the same way with the eyes. Unless we stand in front of a mirror, we cannot see the movements themselves. The best we can do is to feel them very uncertainly. But we distinctly perceive the shifting of the optical images on the retina, or rather the corresponding movement of the point of fixation in the visual field, when movements are made by the eyes. Accordingly, this is the effect which the intent of the will is bent on accomplishing and which we know how to produce voluntarily. When we wish to make a person turn his eyes to the right who has never learned to think about their movements, we do not tell him to turn his eye to the right, but we say: "Look at that object yonder on your right." And even the trained observer controls his ocular movements better by selecting suitable objects for fixation than by trying to give his eyes a definite position without such fixation. I know an eminent physicist, who is trained and experienced in optics to the highest degree, and yet who is quite unable to adjust the visual axes of his eyes parallel to each other without having a very distant object in front of him. He cannot separate the double images of binocular vision without some suitable object of fixation for the purpose, and even then he finds it hard to keep them separate the moment he begins to notice them. I instance this example because it shows the state of the natural eye that is not yet used to physiological experiments and has never learned to think about its adjustments, even in spite of the fact that the theory of vision may be thoroughly understood.

In using the eyes the intent of the will, therefore, is bent on seeing, as distinctly as possible with both eyes, various points of the visual field in succession. This is accomplished by forming an image of the given object in the *fovea centralis* of each eye; and, consequently, we have learned to adjust and accommodate both eyes in order to do this. Other movements of the eyes, which do not have for their object the attainment of the most distinct possible vision of something on which our will might be directed, we have not learned how to execute.

I am disposed to think that there is some connection here with its being easier to make the lines of fixation parallel, or even divergent, by looking up, where the horizon and the sky usually are; and easier

to converge them by looking down, where the floor is, or by looking at objects that can be held in the hands.

However, in trying to understand how the will acts when it is responsible for various adjustments of the eyes, anybody who performs many experiments in physiological optics will gradually learn also how to bring about those normal positions of the eyes, for which at the time being there is no object of fixation present, by looking, so to speak, at an imaginary object. Thus, for example, if such an object is supposed to be close in front of the bridge of the nose, or if we are, as it were, trying to find it, although it is not there, the eyes can be converged so much that they will look cross-eyed. And, conversely, near objects may be observed with parallel visual axes, by trying to look through them far off, or by turning toward them and "staring into vacancy," as the phrase is; that is, by assuming the kind of vision people have when they are buried in thought and not paying any attention at all to the objects in front of them, the accommodation being, therefore, completely relaxed, and the convergence likewise, the eyes being adjusted for far vision.

In changing from convergence to parallelism of the lines of fixation, without having any single definite object of fixation, slight divergences can be produced by making a greater effort than is needed for this purpose.

It is very important for any one who intends to make investigations in physiological optics to learn how to converge the lines of fixation of the eyes or to make them parallel at any moment, without having to use any corresponding visual object; and he should practise doing this. Even then he need not expect to have much success at first in producing such combinations of ocular adjustments as do not occur in ordinary vision. All that is necessary for this purpose is merely to bring the eyes under such conditions that single and distinct images can be obtained by simply deflecting them from their normal adjustments.

The connection between convergence and accommodation may be altered immediately by the insertion of a spectacle lens. For example, when weak concave lenses are interposed in front of a pair of emmetropic eyes, then in order to see distant objects distinctly, they will be obliged to accommodate for near vision, although the lines of fixation are parallel. Provided the glasses are not too strong, it is even possible for the eyes to adapt themselves at once to this new task, but they will be strained more than usual and will soon be fatigued. And this is why, when a person first begins to wear spectacles, it is generally always accompanied by appreciable strain. And, on the other hand, people who have worn spectacles for a long time, appear to be under

some strain without them and have a timid look, as it were, even in gazing at objects for which they can accommodate. It is a common experience that certain types of movements which we are in the habit of making can be performed with far less effort than those which we are not used to. Think of the effort made by an untrained swimmer or skater in trying to get started, and how easy it all is after he has learned how to do it! It is exactly the same way with the eyes when we have to combine their movements in some unaccustomed manner

Another way of producing a different connection between accommodation and convergence is by looking at stereoscopic pictures, and arbitrarily varying the interval between them. This matter will be more fully considered later on.

The eyes may also be made to diverge by viewing stereoscopic pictures and gradually separating them farther and farther apart, all the time trying to fuse them into a single image. I am able in this way to produce a divergence of the lines of fixation of my eyes amounting to as much as eight degrees. The same result can be accomplished by placing in front of the eyes two weak glass prisms of equal power (of refracting angle six or eight degrees), "base up," and then looking through them at distant objects. For that purpose, with the prisms in the said positions, the visual axes of the two eyes should be parallel, but turned more downward than if the prisms were not there. When the prisms are slowly turned "base in," it is still possible to continue to have distinct single vision of the objects previously seen. But in order to do it, the eyes now have to be divergent. The same thing can be accomplished by holding a single prism "base in" in front of one eye and looking first at near objects, which under these conditions still require convergent or parallel lines of fixation, and then gradually passing to more distant objects, for which the eyes have to be divergent.

Finally, both Donders and I myself have noticed that the two eyes can be made to have different elevations by using a weak prism in front of one eye with its "base out" at first. Looking thus at distant objects, the observer must adjust the visual axes so as to be a little convergent; which is accomplished without difficulty. Then the prism is turned very slowly so as to bring the base higher and higher, while the observer continues to try to fixate the object. After a little practice he can succeed in doing it. In this case the free eye sees the object directly with its line of fixation directed straight at it; but the eye with the prism in front of it has to turn appreciably inwards in order to fixate the object. If, after making this adjustment, the prism is suddenly removed, the object of fixation will be seen in double images one under

the other, implying that the two lines of fixation were not on the same level. In the vertical plane likewise I have no difficulty in producing deviations of the two eyes amounting to six degrees up and down.

These facts indicate that the connection existing between the two eyes is not an obligatory anatomical mechanism, but is rather something which can be altered by the mere influence of our own volition; and that the only restriction consists in controlling the intent of our will, so far as its sole purpose is distinct and single vision.

I have previously called attention to other experiences proving the same thing; which have been confirmed for me by other observers also. If the movements of the eyes were coördinated by some anatomical mechanical contrivance, it might be expected to function with even less resistance in the state of drowsiness, when the energy of the will is in abeyance. But my regular experience is that when I begin to get sleepy in the evening from reading, or when out of courtesy to the company I try to keep my eyes open after a long dinner, I am apt to see double images of the objects in front of me, indicating merely too much divergence of the eyes in some cases, sometimes different levels, and sometimes abnormal rolling motions of the eyes. As soon as my attention is aroused by these unusual double images, and I begin to recover myself, the double images generally fuse rapidly together again; and then when I deliberately try to separate them once more, all I can do is to get the customary adjacent double images as the result of the eyes being converged either too much or too little.[1]

The same kind of innveration, by which the movements of the eyes are associated with each other and with accommodation in each eye, is found to be present also with respect to the torsional rotation connected with a given position of the point of fixation. And it might have been expected in advance that our will had nothing to do with this torsional rotation, simply because no definite, practical and perceptible result can be accomplished by varying it. The correctness of this supposition I have now succeeded in demonstrating directly. The torsional rotation of the eyes may be varied very considerably by subjecting them to conditions in which the only way to get single vision is by rolling the two eyes differently.

[1] In E. Hering's *Beiträge zur Physiologie* (4 Heft, S. 274) some question was raised as to the correctness of this observation. Evidently, he has not seen the phenomenon under consideration. The observation last mentioned shows that I did not make the mistake which he attributes to me, and of which even a person with little training in observing double images could scarcely be guilty; namely, the mistake of supposing that the images were on different levels when they were really side by side, simply because my head happened to be tilted!

In this experiment I used two right-angle isosceles glass prisms. In looking through a prism of this sort in a direction parallel to the hypothenuse face, as represented in Fig. 6, the ray of light *ab* is refracted at the side where it enters the prism so as to fall on the hypothenuse at *c*; whence it is reflected along *cd*, so as to emerge from the prism at *d*. If the points *b* and *d* are equally far from the hypothenuse face, the ray *ab* will emerge from the prism in the same direction *de* as it had before it entered it. On the other hand, incident rays such as *ab′* and *ab″* which are not parallel to the hypothenuse face, and which after refraction are reflected from this face (at *c′* and *c″*), will subsequently emerge from the prism so that the incident and emergent rays, *ab′* and *d′e′* or *ab″* and *d″e″*, make equal angles with the hypothenuse face. Accordingly, under these circumstances a prism of this sort acts like a mirror; with this advantage, however, that the apparent direction of the central part of the reflex image remains the same as that of the object itself. When the observer looks through the prism in the direction *ab*, he sees objects on the other side of it "perverted" as to right and left, supposing the hypothenuse face is vertical, or perverted as to top and bottom, if this surface is horizontal.

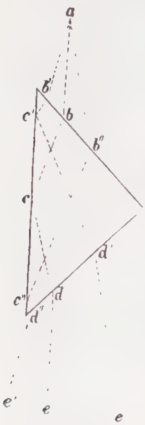

Fig. 6.

Now if the ray *de* reflected from the first prism is made to traverse a second prism in the same way, with its hypothenuse face parallel to that of the first prism, the perversion of the images in the first prism will be neutralized by the opposite perversion in the second prism. Thus all objects as seen through two such prisms will appear to be absolutely unaltered in position and orientation. If, however, the hypothenuse faces of the two prisms are not parallel, but one prism is turned slightly around an axis parallel to the ray *ae*, as shown in Fig. 7, the perversion produced by the first prism will not be completely neutralized by the second prism, and hence an object viewed through this combination will appear to have been turned slightly around the unrefracted ray *ae* as axis, the apparent rotation being twice as great as the actual rotation of one of the prisms with respect to the other. Incidentally, in case the two prisms are rigidly connected together, the combination can be rotated

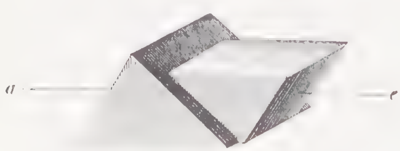

Fig. 7.

in any way around their common axial ray without producing any change in the apparent position of objects as viewed through the optical system.

Now if such a combination of two prisms, producing an apparent rotation of objects around the visual axis amounting to about five degrees, is held in front of one eye, and if both eyes are made to look at distant objects exhibiting a large variety of distinctly different features, at first, as was to be anticipated, double crossed images[1] of the objects will be seen, which are very obvious and easy to notice. However, on continuing to observe the objects, and allowing the gaze to wander frequently over the various conspicuous features, which may all be seen singly one after the other, the double images will finally disappear, and perfectly single images will be seen just as well as in ordinary vision. After having had single vision in this way for some minutes, take the prism system away, and look at the same objects with the eyes free. Then at the first instant double crossed images will be seen, but they will quickly fuse together again.

It might be supposed that in this experiment the double images were not fused, but that one of them was suppressed. This idea can be dispelled by holding a small vertical rod a little way in front of the observed object, and it will be seen in double images which are slightly inclined to each other as the apparently vertical meridians are. Hence it follows that the horizontal meridians of the retinas are so adjusted beyond the prisms as to receive corresponding images.

Moreover, in looking through the prisms, in order to check the observations, I have developed after-images of a horizontal mark in both eyes, and then projected them on a white surface after removing the prisms. At the first instant the after-images in the two eyes appeared to be slanted differently with respect to a certain objective line in the visual field. If the objective mark by which the after-images were produced was horizontal, and if the double prism was in front of the right eye, so that this eye had to be turned 5° to the left, then after removing the prisms and letting both eyes take their normal adjustment, the after-images in both eyes were turned a little to the left. This indicated that in looking through the prisms the left eye had been turned a little to the right, whereas the right eye, following the apparent rotation of the visual field, had been turned to the left. But under these circumstances the after-images must have been developed on corresponding places in the two eyes; and hence also corresponding places of both retinas had received the original image.

[1] By double crossed images I mean here images that have been rotated with respect to each other (*eine Raddrehung gegeneinander erlitten haben*).

Accordingly, the result of these experiments is, that the rolling movements of the eye may also be modified under special conditions; that is, when abnormal rotations of this sort are required in order to get uncrossed double images of the objects in an extended field of vision where there are many details. The greatest rotation of the visual field which I could follow with my eyes in these experiments amounted to seven degrees. Doubtless, in this case both eyes were turned equally in opposite directions; that is, each of them through about three degrees and a half. The difference of position of the two eyes in this case is not produced immediately by merely observing the divergence of the double images, but is the result of a series of corresponding movements of both eyes, during which they traverse the visual field in every direction, so as to maintain continually the singleness of the point of fixation.[1]

These experiments on the ocular muscles are of much importance in the theory of the arbitrariness of motions generally. It is usually supposed that the power of executing a definite arbitrary movement is something with which we have been previously endowed by nature, and does not need to be learned any further; except perhaps in cases such as walking, running on stilts, skating or swimming, where a certain artificial equilibrium has to be maintained in the motion, or where one has to be careful about the effect of other natural forces at the same time. But even for other motions the intentions of the will necessary for executing them need to be learned first. We are accustomed to move the limbs of our bodies with the greatest freedom, but it would be easy to show that even some of the movements of the upper parts of the body require special training before they can be executed. For example, the horizontal outstretched arm can be turned in the shoulder joint around its long axis; and, similarly, radius and hand around the ulna. The two rotations are performed by groups of muscles which are entirely independent of each other. But we are not used to performing both rotations in the same direction, because under ordinary circumstances our intention is merely to bring the hand into one or the other position of rotation. Try now to execute the two rotations in opposite directions, turning the elbow without moving the hand. This is a movement that has no practical purpose whatever, and so usually it is never performed. I have never yet found any one who could do it the first time. And yet this movement can be acquired just as easily as abnormal ocular movements. All that is necessary is to grasp a firm object with the hand and turn the elbow; then gradually

[1] With regard to this, see Note 7 at the end of the chapter.—K.

let go the grip of the hand, and make the same movement until the hand can be entirely released. Thus in this illustration we discover a perfectly similar restriction of arbitrariness in the combination of the movements. It seems insuperable at first, but yet it can be overcome by a proper course of training.

The question to be investigated next is what causes have operated in the training of the movements of the eyes, with the result that only certain definite rolling motions are associated with the various directions of the two visual axes.

So far as Donders' law is concerned, which states that the angle of torsion depends simply on the direction of the two visual axes at the time being, it is easy to see how strict compliance with this law would simplify and insure the solution of the problem of recognizing stationary objects as such, in spite of the movements of the eyes and in spite of the shiftings of the images on the retina. We let our eyes roam continually over the visual field, because that is the only way we can see as distinctly as possible all the individual parts of the field in turn. The way we contrive to see them as distinctly as possible with both eyes is by directing both visual axes at the point which is temporarily under observation and then accommodating the eyes for it. This being the case, the two eyes might still be turned any way at all around the line of fixation as axis, without our ceasing to fixate the given point with both eyes. Suppose now there is a visual field of this sort in front of us filled with stationary objects. Then as our eyes wander over the field, the sensations in the separate nerve fibres of the retina will also vary continually. Returning to look again at an object A, which was previously under inspection, suppose we find that a different rotation of the eyes is needed now from that which they had at first. The point of fixation would make indeed the same impression as before on the two foveas; but the images on adjacent parts of the retina would be in new positions, and the nerve fibres lying around the fovea would get entirely different luminous impressions from those they had at first. In order to prove that, in spite of this new system of sensations, the object has nevertheless remained the same, it would be necessary to bring the eye back exactly into its old position as to torsion also, so as to test whether the old impression is obtained once more by restoring the former position.

As far as recognition of objects is concerned, generally nothing is gained in natural vision by viewing them with different rollings of the eyes; and all that is necessary to recognize again a stationary object as being stationary is to let the eye return to a perfectly definite

unchanged position. Consequently, we have to accustom ourselves from the first to use definite amounts of torsion for definite directions of the visual axes.

Doubtless with sufficient practice in recognizing modifications of the retinal sensations when the eye turns around the line of fixation, it might even be possible to perceive correctly that the position of the object had not changed, although the retinal image had done so. However, this would be a new and big complication in training the eye for visual perceptions, and there would be no advantage in it at all; and so we avoid it from the start.[1]

I have called this the *principle of the easiest orientation of the eye for the positions of equilibrium.* It postulates in the first place that definite amounts of torsion of both eyes are associated with every definite direction of the two visual axes; although it is still indefinite as to what these values are.

The only case investigated so far is that in which the same object was viewed *directly* twice in succession. The case that has still to be considered is that in which a stationary object is recognized as being stationary after having been viewed first *directly* and then *indirectly.*

We shall study the case of a single eye first, which is supposed to be isolated; and then afterwards we can see what modifications are necessary when this eye is used in conjunction with a second eye. Moreover, let us assume that the displacements of the eye are infinitesimal; for if we continue to recognize the object as being stationary during the infinitesimal displacements occurring in the exceedingly brief intervals of a more extended movement, this recognition will be maintained also at the termination of the movement.

Let a, b, c, d, etc. be used to designate various points on the retina; and suppose that the point a corresponds to the *fovea centralis.* The points of the image that fall on these places of the retina may be designated by the corresponding capital letters A, B, C, D, etc. Thus the point A of the image corresponds to the point of fixation. Let us suppose that the interval between A and B, and hence also the interval between a and b, is exceedingly small. Now let the look glide from the image-point A over to B, in which case B will be imaged in the *fovea centralis* at a. Then the image-points A, C, D, etc. will fall at other

[1] In a previous article (published in *Arch. f. Ophthalmol.*, IX, 2, pp. 156, 157) I stated also that the position of the object in space should be correctly apprehended. E. Hering objected to this statement, pointing out that apprehension of position is generally interfered with by torsional rotations of the eyes. In certain cases, as we shall see in the next section, this is true, although the cases are much more restricted than Mr. Hering thinks. Accordingly, in the above argument I have left out the question as to the orientation of the actual position of the object, limiting the discussion to the essential point, namely, that stationary objects are recognized as being stationary.

points on the retina, which may be designated by α, γ, δ, etc. Thus while the former sensation of the point b passes over to a, the sensation that was at a passes over to α, that of c over to γ, that of d over to δ, etc. Now if the same system of changes of sensation invariably recurs whenever the sensation which was at b is caused, by an impulse of will resulting in motion, to pass over to a, we shall learn to regard this complex of changes as being the sensory expression of an ocular movement that corresponds to no change in the objects. The test of it will be that at any moment at all we can fixate A again and then find the first system of sensations exactly as before. However, it amounts simply to this, that without going through this test, we learn, while looking at B, that the change we notice is not any change of the objects.[1]

Now in order that, whenever the fixation is transferred to the point in the visual field corresponding to the point b on the retina, the points α, γ and δ shall simultaneously receive the images belonging to a, c and d, respectively, it is necessary for the eye to execute this movement by turning invariably around a definite axis, which is fixed with respect to the eyeball. Suppose we designate this axis by the symbol $\mathfrak{B}$.

Now b is only one of the points on the retina that are adjacent to a. Suppose that c is another point infinitesimally close to a and in a different direction from that of b. Then there must be another axis of rotation $\mathfrak{C}$ fixed in the eyeball, in order to shift the look in the direction ac, if this shifting is to be accompanied always by an equal shifting of the image on the retina, that is, by the same system of changes of sensation.

The look can then be directed to any other point F in the visual field adjacent to the point of fixation A by performing certain slight rotations, one around the $\mathfrak{B}$-axis and the other around the $\mathfrak{C}$-axis. When the rotations are infinitesimal, the axes can be compounded on the principle of the parallelogram of forces; and the diagonal of the axes $\mathfrak{B}$ and $\mathfrak{C}$ must always be in the plane passing through them. Hence in looking at F, the eye can be turned by a single rotation around an axis in the plane $\mathfrak{B}\mathfrak{C}$ into the same position as if it had been rotated first around $\mathfrak{B}$ and then around $\mathfrak{C}$. And since, by DONDERS' law, which we have just endeavoured to prove, the eye in looking at F must always have the same direction, no matter how it was turned thither, the result is that, in turning from A to look at F or at any other point exceedingly close to A, the movement is performed by turning the eyeball around an axis of rotation which lies always in one and the

[1] ¶See F. HILLEBRAND, Die Ruhe der Objekte bei Blickbewegungen. *Jahrb. f. Psychol. u. Neurol.*, 40 (1921), p. 213. (J. P. C. S.)

same plane $\mathfrak{B}\mathfrak{C}$, and which has a fixed position in the eye. This would be the condition, that whenever the eye was shifted ever so little, the movement must be accompanied by a constant system of changes of sensation in the fibres of the optic nerve. Ultimately, we learn to recognize it as being the sensory expression of the ocular movement connected with that particular change of gaze.[1]

The fact that, for any very small displacements of the eye from some definite fixed position, the axes of rotation must all lie in one and the same plane, as will be shown in the subsequent mathematical discussion, is an immediate consequence for all parts of the field of fixation, provided the rolling of the eye is a continuous function of the direction of the line of fixation, and therefore does not change abruptly. The principle of easiest orientation requires that this plane shall be fixed, if possible, with respect to the eyeball.

The changes of sensation during the movement of the eye are most easily recognized as being the expression of this movement, and not of a movement of the object, when the change of the eye in looking at the point in the visual field corresponding to the point b on the retina is invariably accompanied by the same shifting of the image on the retina, no matter what was the initial position of the eye. If it were always necessary to recognize objects as being stationary, even when the retinal image started from different initial places and was shifted differently, much more involved training would be required to learn how to use the eye. It is true, we cannot say in advance that it would be impossible to train the eye in this fashion. However, as we shall see, experience shows that this is not the case.

The condition of easiest orientation, as here stated, is not perfectly satisfied by the human eye in indirect vision, nor, as will be shown in the subsequent analytical discussion of the problem, can it be, except for a field whose dimensions are infinitesimal as compared with the radius of the sphere. It has already been stated that, according to LISTING's law, the planes of the axes of rotation have different

[1] In E. HERING's *Beiträge zur Physiologie* (pp. 274–283) he tries to show that this argument is not valid. It was pointed out above how he had misunderstood the first principle, and so led to take a secondary axis for the principal axis, and now this results in further misunderstanding here. He considers the second principle as superfluous along with the first. But that is not the case. For the first principle states simply that stationary objects are recognized as being stationary whenever the line of fixation returns to the *same* direction; whereas the second principle states that they are recognized as being stationary even when the line of fixation is in a *different* direction. Mr. HERING further points out that when the second principle is used without the first, the argument may be nonsense. But I have never used the second principle except as supplemental to the first. Besides it is self-evident that it cannot be used any other way. In the argument above I trust I have succeeded in expressing my ideas more accurately, and that the misunderstanding has been clarified.

positions in the eye for different adjustments of the line of fixation. Certain optical illusions, which are dependent on this fact, can be produced most distinctly with very remote objects like the stars, as to whose actual position our experience can give us no information.[1]

Find three bright stars in the sky sufficiently far apart and nearly in a straight horizontal line. Suppose all three of them are apparently in line when the head is raised until the primary position of the visual axes is directed toward the middle star. Then when the eye traverses the row of stars with the face turned a little lower down, that is, with the eyes higher up in the head, the same group of stars will appear to form a line that is concave downward. And when the face is lifted higher than at first, with the eyes sunk farther back in the head, the row of stars will appear to form a line that is convex downward. The explanation of these illusions is to be found in the torsional rotations of the eye. In looking at the right-hand end of the row of stars, with the eyes lifted, the retinal horizon is rotated with respect to the line of sight, its right-hand side being elevated. In this case the right-hand end of the line of stars appears to be lowered. Similarly, in looking at the farthest star on the left, that end of the row appears to be lowered, making the entire line appear concave downward. It is just the other way when the eyes are turned down toward the chin.[2]

Or suppose we compare the apparent inclination to the horizon of a row of stars like the three stars in the tail of *Ursa major*, by turning the face so as to look at the stars first with the eyes raised to the right and then with the eyes raised to the left. It will be found that in the first position the upper end of this row of stars apparently inclines more to the left, in the second case more to the right; and in both cases, therefore, toward the median plane of the head.

In these illustrations the question has nothing to do with an absolute direction of the row of stars in space, such as vertical or horizontal, because the form of the imaginary celestial vault is too vague for any definite directions of that sort. The only question is as to the agreement or non-agreement in the direction of the images observed when the eyes look in different directions. It is shown by these experiments that in extremely peripheral positions of the eyes our judgments vary as to the position of the object in the visual field and also as to the form of the field. Now since, as has been said, such rolling motions of the eyes as are responsible for misapprehensions of this sort cannot be entirely avoided in an extensive field, all that

[1] In the corresponding experiments which I have described elsewhere the convergence of the eyes has a peculiar effect, which will be discussed in the next chapter.

[2] As to these illusions, see Note 8 at the end of the chapter.—K.

can be demanded is that, for different positions of the visual axis, these particular movements shall be such that the sum of all errors of orientation due to rollings of the eye shall be as small as possible.

Complete fulfilment of the second principle would require that for all positions of the line of fixation the plane of the axes of rotation should always have the same position in the eyeball. The rotation then would never have a component whose axis would be perpendicular to that plane of the axes of rotation, which I have proposed calling the *atropic* line of the eye. Any rotation around this atropic line, whose position in the eye is indefinite at first, would have to be regarded as an error. Accordingly, the essential requirement of the second principle might be formulated by saying, that *the sum of the squares of these errors, for all infinitesimal movements of the eye, shall be a minimum.* The reason for taking the squares of the errors here is the same as that in estimating the errors by the method of least squares.

The analytical treatment of this problem will be given presently, but the result may be stated as follows: In order for the sum of the errors to be least, the atropic line must coincide with the line of fixation, no matter what is the form of the visual field; but, in general, the distribution of the torsional rotations will depend on the form of the field. For a circular field of fixation LISTING'S law would correspond best to the requirements of the problem, the primary position being in the centre of the field. In fields that are nearly, but not exactly, circular, departures from LISTING'S law would have to be manifest out toward the margin, but their amount will be reduced on account of the fact that the eye traverses these peripheral places less frequently. Apparently, we try to avoid movements of the eye in those directions that are parallel to the edge of the field of fixation, which might produce apparent movements of the object.

Thus for a single eye and for a circular field of fixation, LISTING'S law of the ocular movements appears under these circumstances to be the most advantageous for the orientation.

However, we use both eyes in seeing, and sometimes they are parallel and sometimes convergent. All that the principle of easiest orientation for positions of equilibrium requires is, that as soon as *both* eyes are back again in the same positions, their torsional displacements shall be the same. As a matter of fact, it is found that these displacements are slightly different when the eyes are convergent from what they are when the eyes are parallel. In normal vision, however, parallel positions do not generally occur except in those parts of the field where we are accustomed to have very distant objects, that is, in the upper part of the field.

In the lower part of the field of fixation the objects are almost without exception close by, the floor being the farthest of them. In Fig. 8 I have outlined the field of fixation of my two eyes when they are parallel. The primary position of the eye for distant vision is shown by *a*. The length *ac* of the arrow indicates the corresponding distance of the eye from the sheet of paper on which the field of fixation is projected. Under these circumstances the eyes are pointed in the direction of the perpendicular erected at *a*. Down below, the visual field of each eye is obstructed on the inside by the protrusion of the nose (*bb* in the drawing). The portion of the bridge of the nose that can be fixated is shown by the shading in the drawing. This lower part, which is partially overlapped by the double images of the nose, and is comprised between them, can hardly be used at all when the eyes are parallel; and it is also decidedly harder to represent these portions than it is in the upper part of the field. Accordingly, the boundaries of the field of fixation for parallel visual axes may be drawn about between *bb* in the diagram; and the remainder forms a field that is nearly circular. Now here, indeed, I find that LISTING's law holds, and that the primary position *a* is in the centre of this field. Incidentally, the two fields of my eyes are not perfectly symmetrical. My left eye can see farther downward and outward than the right eye.

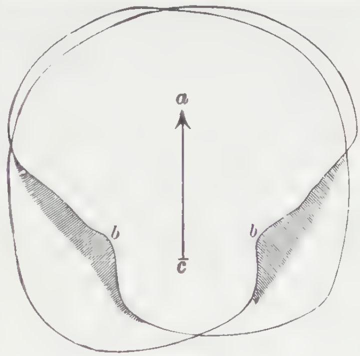

Fig 8.

When the eyes are converged, they are turned inward at first just because of the convergence, and then mainly downward. It is comparatively seldom that there are any near objects to be viewed in the upper part of the field of fixation. Besides, we are not able to push convergence as far in looking upward as in looking downward. Consequently, for positions of convergence departures from the law of motion for parallel positions are to be expected, as if the primary position for convergence were lower and more inward than in the case of parallelism. In fact, the deviations represented in Fig. 4 above are of this nature. Perhaps, therefore, the amount of these deviations must have something to do with the customary frequency of positions of convergence and with the degree of convergence. In near-sighted eyes, which are in the habit of being converged to look at things, the peculiar characteristics of these positions of convergence may even be com-

municated to the positions for distant vision, which are comparatively much less used.

In endeavouring to derive the law of ocular movements from the requirements for perception, of course, it has been necessary to leave out of account any information or estimation as to the linear and angular dimensions of the apparent field of vision; and indeed even any knowledge as to the arrangement of the image-points on the retina, because the only way any knowledge of this kind can be acquired (unless it is regarded as intuitive) is by movements of the eye. In reality this knowledge is probably developed side by side in both respects, and simultaneously; and hence the derivation of the law of rotation, as given here, is not to be regarded as an exact description of the actual process of development of this law during the early part of childhood. At present the most that can be expected from the empirical theory of visual perceptions in this way is to show that there is nothing in these perceptions or in the movements of the eye that might not be acquired by experience and by proper training in trying to discern the objects in the external world as accurately and surely as possible. Naturally, the description of this process of training and experience is obliged to be more methodical and more analytical as to its individual factors than the way it really happens generally in the variegated medley of random sensory impressions.

As a general principle governing the ocular movements, A. Fick and Wundt proposed choosing the particular torsional rotation which enabled the eyes to obtain the desired direction for the line of fixation with the slightest muscular effort. How this principle is carried out, will be discussed more at length presently. Probably it is actually fulfilled in the real normal movements that are made by the eyes. However, I did not think I could venture to accept this principle as final, because voluntary exertion can demonstrably produce those positions of the eyeball that are best suited for the purposes of vision. Besides, generally speaking, the muscles are so adaptable, that sometimes those which have to make more effort get to be the stronger. And yet when we reflect that for many generations in succession the muscular mechanism of the eyes has adapted itself to the needs of individuals, and that this arrangement has been inherited by their descendants, it is not to be denied that the fact that they are the easiest movements to make is enormously in favour of the practical execution of the most convenient rolling movements of the eye. However, the experiments cited above go to show that the easiest ocular movements are not chosen permanently, unless they are likewise at the same time the most advantageous for vision.

Laws similar to those for the motions of the eye are found to be true also for the motions of the head. AUBERT noticed that, when the head was suddenly tilted to one side in looking at a fixed point on a vertical or horizontal line, thereby rotating its retinal image, either there was an apparent rotation of the line with the movement of the head or at least there was some feeling of uncertainty in deciding whether a rotation had occurred or not.

The ordinary movements of the head are executed on the same principle as those of the eyes. The main occipital joint consists of two joints, one between the occipital bone and the first cervical vertebra or atlas, and the other between the atlas and the second cervical vertebra. The former permits rotation around a horizontal axis extending from right to left, as well as a rotation to a slight extent around a horizontal axis extending from front to back. The second joint has simply a vertical axis of rotation. Thus the two joints together enable moderate rotations to be made around any axis. In addition there is the possibility of movement in the cervical vertebral column. When the eyes are to be turned far to the right or left, the head is rotated around a vertical axis in the lower joint. When the look is directed straight up or down, the head is turned around the horizontal axis passing from right to left through the junction of the occipital bone and atlas. But if the look is directed obliquely upward on the right, the head is turned, in the same way as the eye, around an axis extending from above on the right downward on the left, thereby causing the right-hand side of the head to rise higher than the other side. On the other hand, when the look is directed downward on the right, the right-hand side of the head is made lower than the other side. Thus the rotations of the head are of the same nature as those executed by the eye, although they may be altered with more freedom.[1]

General Geometrical Discussion of the Rotations[2]

Consider an ordinary terrestrial globe fastened in a brass ring so that it can turn around its polar axis. This brass ring itself can be shifted at the place where it is inserted in the wooden stand. And, finally, the stand, resting on a horizontal table, can be turned around a vertical axis. Thus by this mode of attachment the globe can be oriented any way at all. It may be supposed to represent the eyeball, with its polar axis corresponding to the line of fixation.

[1] Concerning the connection between the movements of the eyes and of the head, see Note 9 at end of the chapter.—K.

[2] ¶See HORACE LAMB, The Kinematics of the Eye. *Phil. Mag.* 6th Series. XXXVIII. 1919. pp. 685-695. (J.P.C.S.)

The polar axis may be vertical at first, with the principal (steel) meridian of the globe in the plane of the brass ring. The vertical ordinates (which are parallel therefore to the line of fixation in its initial position) will be denoted by x; the xy-plane being the plane of the principal meridian and of the meridian ring, so that the y-axis is the horizontal line in the plane of the ring. The z-axis is perpendicular to this plane. These axes all pass through the centre of the sphere. The axes of y and z in the eye may have any two perpendicular directions in the yz-plane; and so let us assume that in its initial position the atropic line lies in the xy-plane. This simplifies the calculation considerably without affecting its generality. Thus in the globe representing the eyeball the atropic line would be somewhere in the steel meridian.

Consider now four rectangular systems of coördinates, which all coincide with each other in the initial position of the globe. The first system, denoted by x, y, z, is supposed to be absolutely fixed in space. The second system, denoted by x_1, y_1, z_1, is rigidly connected with the globe-stand and moves with it. The third system, denoted by x_2, y_2, z_2, is rigidly connected with the brass ring. And, lastly, the fourth system, denoted by ξ,υ,ζ, is rigidly connected with the globe itself.

When the stand is turned on the table, the system of coördinates x_1, y_1, z_1 will be shifted with reference to that of x, y, z; but since the x-axis is the axis of rotation, the x_1-axis continues to coincide with the x-axis and the y_1z_1-plane with the yz-plane. Consequently, after the rotation the distance x_1 of any point from the y_1z_1-plane is just the same as its distance x from the yz-plane. In Fig. 9 suppose that the plane of the paper is the same as the yz-plane (or the y_1z_1-plane); the y-axis and the z-axis being represented by OA and OH, respectively, the y_1-axis and z_1-axis by OE and OD, respectively. The point C is supposed to be the projection of the point whose coördinates are to be found. From C draw CA, CB, CD, and CE perpendicular to the four axes of coördinates; and from E draw EG and EH perpendicular to OA and OB, respectively, and let K designate the point of intersection of EG and CB.

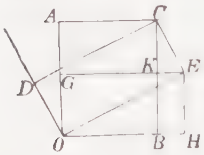

Fig. 9.

Then

$$OA = CB = y\,, \qquad OD = CE = y_1\,,$$

$$OB = CA = z\,, \qquad OE = CD = z_1\,.$$

Let ϑ denote the angle HOE, through which the frame $x_1y_1z_1$ is turned with respect to the frame xyz. Then

$$y = OA = OG + GA = OG + KC\,;$$

and since the angle $GEO = KCE = HOE = \vartheta$,
therefore

$$OG = OE \sin(GEO) = z_1 \sin\vartheta ,$$
$$KC = CE \cos(ECK) = y_1 \cos\vartheta .$$

Consequently,

$$y = y_1 \cos\vartheta + z_1 \sin\vartheta .$$

Similarly,

$$z = OB = OH - KE ,$$
$$OH = OE \cos(EOH) = z_1 \cos\vartheta ,$$
$$KE = EC \sin(ECK) = y_1 \sin\vartheta ;$$

and therefore

$$z = z_1 \cos\vartheta - y_1 \sin\vartheta .$$

Hence the values of the coördinates xyz of the given point after the rotation in terms of the coördinates $x_1y_1z_1$ are as follows:

$$\left.\begin{aligned} x &= x_1 \\ y &= y_1 \cos\vartheta + z_1 \sin\vartheta \\ z &= -y_1 \sin\vartheta + z_1 \cos\vartheta \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (1)$$

Moreover, if the brass ring of the globe is turned in the stand, the position of the $x_2y_2z_2$-system will be changed with respect to that of the $x_1y_1z_1$-system, but in this case the y_2x_2-plane will continue to be congruent with the y_1x_1-plane, and hence also the z_2-axis with the z_1-axis. Let α denote the angle of rotation; then the values of the coördinates $x_1y_1z_1$ expressed in terms of those of $x_2y_2z_2$ are found in a manner similar to that above, as follows:

$$\left.\begin{aligned} x_1 &= x_2 \cos\alpha - y_2 \sin\alpha \\ y_1 &= x_2 \sin\alpha + y_2 \cos\alpha \\ z_1 &= z_2 \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (1a)$$

Finally, when the globe is turned around its polar axis, the $\xi v\zeta$-system will be displaced with respect to the $x_2y_2z_2$-system, the ξ-axis and the x_2-axis as axis of rotation remaining congruent. Denoting the angle of rotation by ω, we have for the values of $x_2y_2z_2$:

$$\left.\begin{aligned} x_2 &= \xi \\ y_2 &= v \cos\omega + \zeta \sin\omega \\ z_2 &= -v \sin\omega + \zeta \cos\omega \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (1b)$$

Now if the values of $x_1y_1z_1$ as given by equations (1a) are substituted in equations (1), we obtain:

$$x = x_2 \cos \alpha - y_2 \sin \alpha \ ,$$
$$y = x_2 \sin \alpha \cos \vartheta + y_2 \cos \alpha \cos \vartheta + z_1 \sin \vartheta \ ,$$
$$z = -x_2 \sin \alpha \sin \vartheta - y_2 \cos \alpha \sin \vartheta + z_2 \cos \vartheta \ .$$

And, finally, if in these equations we substitute the values of $x_2y_2z_2$ as given by equations (1b), we obtain:

$$\left.\begin{aligned} x &= \xi \cos \alpha - v \cos \omega \sin \alpha - \zeta \sin v \sin \alpha \\ y &= \xi \sin \alpha \cos \vartheta + v(\cos \alpha \cos \vartheta \cos \omega - \sin \vartheta \sin \omega) \\ &\quad + \zeta(\cos \alpha \cos \vartheta \sin \omega + \sin \vartheta \cos \omega) \\ z &= -\xi \sin \alpha \sin \vartheta - v(\cos \alpha \sin \vartheta \cos \omega + \cos \vartheta \sin \omega) \\ &\quad - \zeta(\cos \alpha \sin \vartheta \sin \omega - \cos \vartheta \cos \omega) \end{aligned}\right\} \quad . \ . \qquad (1c)$$

These equations enable us to determine the coördinates xyz of any point which is given by its coördinates $\xi v \zeta$ on or in the sphere.

Let us determine first the position of the polar axis which is to correspond to the line of fixation of the eye. It is the ξ-axis, and for points on it, we have $v = \zeta = 0$ Hence, for a point on the polar axis whose distance from the point of rotation is ξ, we have:

$$x = \xi \cos \alpha \ ,$$
$$y = \xi \sin \alpha \cos \vartheta \ ,$$
$$z = -\xi \sin \alpha \sin \vartheta \ .$$

Accordingly, the angle between the polar axis and its initial position is α, and the projection of the polar axis on the horizontal plane is $\xi \sin \alpha$, which is inclined to the xy-plane at the angle ϑ. But this projection is the line in which a plane containing the vertical x-axis and the polar ξ-axis is intersected by the horizontal plane. Applying these results to the eye, obviously, α denotes the angle between the primary and secondary positions of the line of fixation, and ϑ denotes the angle made by the plane containing the primary and secondary positions with the original xy-plane. The two angles give the direction of the line of fixation.

Now in order to define precisely the meaning of the angle ω with respect to the eye, let us see how this angle should be chosen on the supposition that the eye moves according to Listing's law, and that its initial position, in which x, y, z, coincides with ξ, v, ζ, is the primary position. Then by this law the new adjustment would have to be the same as if the eye had been turned into the second position around an

axis lying in the planes of $v\zeta$ and yz. Since the points on the axis retain their same positions, then after the rotation we must have for them:

$$x=\xi\,, \qquad y=v\,, \qquad z=\zeta \quad . \; . \; . \; . \; . \; . \; . \qquad (2)$$

The position of the axis may always be found by these three conditions. According to LISTING's law, the axis of rotation must be in the $v\zeta$-plane, that is, for points on the axis we must have $\xi=0$; hence, after substituting these values in equations (1c), we obtain:

$$0=-v\cos\omega\sin a-\zeta\sin\omega\sin a\,,$$

$$v=\quad v(\cos a\cos\vartheta\cos\omega-\sin\vartheta\sin\omega)+\zeta(\cos a\cos\vartheta\sin\omega+\sin\vartheta\cos\omega)\,.$$

$$\zeta=-v(\cos a\sin\vartheta\cos\omega+\cos\vartheta\sin\omega)-\zeta(\cos a\sin\vartheta\sin\omega-\cos\vartheta\cos\omega)\,.$$

From the first equation, we have:

$$v\cos\omega+\zeta\sin\omega=0\,,$$

which is satisfied by putting

$$v=h\sin\omega\,, \qquad \zeta=-h\cos\omega\,,$$

where h denotes any arbitrary magnitude. Accordingly, the other two equations are equivalent to the conditions:

$$\sin\omega=-\sin\vartheta\,,$$

$$-\cos\omega=-\cos\vartheta\,,$$

which may be satisfied by assuming that

$$\omega=-\vartheta \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \qquad (2a)$$

Therefore this is the condition that the rotations given by equations (1c) shall obey LISTING's law. In this case the values of x, y, z become:

$$\left.\begin{aligned} x&=\xi\cos a-v\cos\vartheta\sin a+\zeta\sin\vartheta\sin a\\ y&=\xi\sin a\cos\vartheta+v(\cos a\cos^2\vartheta+\sin^2\vartheta)\\ &\quad+\zeta(1-\cos a)\sin\vartheta\cos\vartheta\\ z&=-\xi\sin a\sin\vartheta-v(\cos a-1)\sin\vartheta\cos\vartheta\\ &\quad+\zeta(\cos a\sin^2\vartheta+\cos^2\vartheta)\end{aligned}\right| \quad . \; . \; . \; . \qquad (2b)$$

Here it should be noted, that generally, even apart from LISTING's law, in any event the sum $(\omega+\vartheta)$ must be exceedingly small for very small values of a; otherwise, displacements of the line of fixation involving infinitesimal values of a will produce finite changes of position of the eye.

In equations (2b) x denotes the distance from the yz-plane of the point whose coördinates are there given; and ξ denotes the distance of

the same point from the $v\zeta$-plane. Both distances are reckoned as positive when they extend in front of the anterior sides of these planes. If we put

$$x=-\xi \quad \text{or} \quad x+\xi=0 \quad , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2c)$$

this will be the equation of all points that are equidistant from the anterior side of the plane $x=0$ and from the posterior side of the plane $\xi=0$. But this property is characteristic of points lying in the plane which bisects the dihedral angle ϑ between the planes $x=0$ and $\xi=0$; and so equation (2c) is the equation of this intermediate plane. Substituting the value of x as given by the first of equations (2b), we obtain for this equation:

$$0=\xi(1+\cos a)-v\cos\vartheta\sin a+\zeta\sin\vartheta\sin a\,; \quad . \quad . \quad . \quad . \qquad (2d)$$

and multiplying both sides of it by the factor

$$\frac{1-\cos a}{\sin a}\,,$$

we obtain:

$$0=\xi\sin a-v\cos\vartheta(1-\cos a)+\xi\sin\vartheta(1-\cos a) \quad . \quad . \quad . \qquad (2e)$$

Now if this latter equation is multiplied by $\cos\vartheta$, then

$$0=\xi\sin a\cos\vartheta+v(\cos a\cos^2\vartheta-\cos^2\vartheta)+\zeta\cos\vartheta\sin\vartheta(1-\cos a).$$

When this equation is compared with the value of y in equations (2b), it appears that

$$v=y\,.$$

A corresponding equation obtained by multiplying (2e) by $\sin\vartheta$ shows that

$$\zeta=z\,.$$

Accordingly for points lying in the plane bisecting the dihedral angle ϑ between the planes $x=0$ and $\xi=0$, we have:

$$x=-\xi\,, \qquad y=v\,, \qquad z=\zeta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2f)$$

Now suppose the eyeball takes another position for which the values of x, y, z, a, ϑ are denoted by x_0, y_0, z_0, a_0, ϑ_0, respectively; then for the pane that bisects the angle ϑ_0 between the planes $x_0=0$ und $\xi=0$ we ave similarly:

$$x_0=-\xi\,, \qquad y_0=v \qquad z_0=\zeta\,.$$

Hence, if the point ξ,v,ζ is in both of these bisecting planes, that is, is a point in their line of intersection, we must have for it:

$$x=x_0\,, \qquad y=y_0\,, \qquad z=z_0\,.$$

Thus points on this line of intersection have the same positions in space in both positions of the eye; and hence if the eye is to be brought from the first position to the second position by being turned around a fixed axis, the axis to be used is the line of intersection of these two bisector planes. The position of this axis is given by equation (2c) and the analogous equation for the second position, that is, by the equations

$$x+\xi=0 \text{ and } x_0+\xi=0 .$$

The angle through which the eyeball has to be turned around the resultant axis in order to bring it from the first position to the second is twice as great as the angle between the bisector planes whose equations are given above.

Entirely independently of LISTING's law, this rule, by which the resultant of two successive rotations is reduced to a single rotation, can be applied to any body that rotates about a fixed pivot. If a body executes successive rotations in this way around two different axes, and if the positions of the axes are known during the rotations around them, or, what amounts to the same thing, the positions of the two axes after the first rotation and before the second rotation, then a plane A may be passed through the two axes, its position before the first rotation being denoted by A_0, and its position after the second rotation by A_1. Since the axes of rotation are the lines of intersection of A_0 and A and of A_1 and A, there is no difficulty about determining these planes, provided we know the magnitudes of the angles of rotation, which are the angles A_0A and A_1A. The planes bisecting these two angles must be constructed, and then their line of intersection will be the resultant axis of rotation; and twice the angle between these planes (no matter which of the two angles we take) will be the angle of rotation.

In case the rotations are infinitesimal, the resultant axis of rotation will be infinitely near the plane containing the other two axes; and in the limit it coincides with the diagonal of the parallelogram, whose two sides have the same directions as the two axes, their lengths being made proportional to the magnitudes of the angles of rotation.

Let us return to the consequences which may be derived from LISTING's law for the movements of the eyeball. Since the axis of rotation, around which the eye has to turn in order to pass from the position given by equations (2b) into any other position with the co-ordinates x_0, y_0, z_0, must certainly be in the plane $x+\xi=0$, no matter what the second position may be, the consequence is that every time it is desired to pass from a given initial position of the eyeball to any other positions by turning the eye around fixed axes, these axes must

all lie in a certain plane, whose position will depend merely on the initial position of the eye, and not on the final position to be reached. And, moreover, every rotation of any amount whatever around an axis in the aforesaid plane will always bring the eye from the corresponding initial position into new positions that are in accordance with LISTING's law.

Thus the primary position of the line of fixation is distinguished simply by the fact that the corresponding plane of the axes of rotation is perpendicular to this line.

Hence, the position of the normal to the plane of the axes of rotation is found for any position of the line of fixation by bisecting the angle between the line of fixation at the given instant and the primary position of this line. This normal may be called the *temporary atropic line* for the given adjustment of the eye.

For every continuous rotation that the eye executes around an axis in accordance with LISTING's law, the temporary atropic line of the initial position will trace out a great circle on the spherical field of fixation, since it is perpendicular to the axis at the centre of rotation. However, the line of fixation, which in general is not perpendicular to the axis of rotation, will not describe a great circle, but a circle parallel to the great circle corresponding to the atropic line belonging to its initial position.

In Fig. 10 let O designate the centre of rotation of the eye; and let OA represent the primary position of the line of fixation, and OB a secondary position of this line. The circle $ACBDF$ represents the section of the given spherical field of fixation. The straight line GOC, which bisects the angle AOB, is the atropic line when the line of fixation is adjusted along OB; and if OD is a perpendicular to OC, a plane through OD perpendicular to the plane of the diagram will be the plane of the axes of rotation for OB.

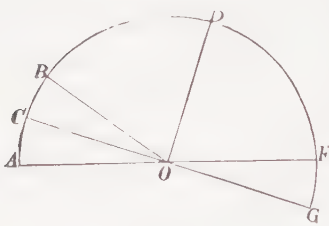

Fig. 10.

Prolonging AO to F, we observe that the angles BOD and DOF are equal, being complementary to the two equal angles COB and FOG. Consequently, if OE is any other axis in the plane of the axes of rotation, which is passed through OD, the angles EOB and FOE must also be equal.

If, therefore, the eyeball could be turned completely around the axis OE, the line OB would have to come into the position OF. Consequently, when the rotation takes place about a fixed axis according

to LISTING's law, the line of fixation, which was initially along OB, will trace out circles in the spherical field of fixation, which must all pass through the point F. However, the position of the point F is entirely independent of the position of OB, being dependent simply on the primary position OA. We may call it the *occipital point of the field of fixation.* Consequently:

The prolongations of all circular arcs in the spherical field of fixation, which are described by the line of fixation in turning around a fixed axis according to LISTING's *law, will pass through the occipital point of the field.* And conversely:

If the line of fixation corresponding to LISTING's *law describes an arc of a circle in the spherical field of fixation, which passes through the occipital point of the field, it must turn around a fixed axis which is perpendicular to the plane of the given circle.*

These circles on the spherical field of fixation that pass through the occipital point will be called *direction-circles.* In the subsequent chapters it will be seen how important they are for the orientation. Thus the direction-circles will not be great circles of the field of fixation unless they pass through the primary position of the line of fixation whose location in the field is called the *principal point of fixation.*

Moreover, evidently, if a linear after-image developed in the eye is projected in the field of fixation along a direction-circle corresponding to the given position of the line of fixation, and if the eye is moved in the direction of this circle, the after-image will apparently continue to lie along this circle and will be shifted simply along its own length. And if an after-image is developed which passes through the point of fixation at right angles to one of the given direction-circles, as the gaze traverses this circle, the after-image will remain perpendicular to it.

Lastly, it is evident likewise that the after-image will be congruent with the direction of all those direction-circles that have a common tangent at the occipital point coinciding with the original tangent there.

The equation of the direction-circle passing through a given position of the line of fixation, for instance, through that defined by equations (2b), is easily obtained from the condition that it must be the trace of a plane through the occipital point formed on the spherical field of fixation, whose centre coincides with the pivot of the eye and the origin of our system of coördinates Thus, suppose that

$$x^2+y^2+z^2=R^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

is the equation of the spherical field of fixation. The general equation of a plane is

$$ax+by+cz=A \; .$$

The coördinates of the occipital point are

$$x = -R\,, \quad y = 0\,, \quad z = 0\,;$$

and since they must satisfy the equation of the plane, we have therefore:

$$-aR = A\,.$$

This enables us to find the unknown magnitude A. Hence, the equation of any plane passing through the occipital point will be:

$$ax + by + cz = -aR \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{(3a)}.$$

And so the two equations (3) and (3a) are the equations of any direction-circle.

Writing these two equations as follows:

$$x^2\left(1 + \frac{y^2}{x^2} + \frac{z^2}{x}\right) = R^2\,,$$

$$x^2\left(1 + \frac{b}{a}\frac{y}{x} + \frac{c}{a}\frac{z}{x}\right)^2 = R^2\,,$$

and dividing one by the other, we obtain:

$$1 + \frac{y^2}{x^2} + \frac{z^2}{x^2} = \left(1 + \frac{b}{a}\frac{y}{x} + \frac{c}{a}\frac{z}{x}\right)^2 \quad . \; . \; . \; . \; . \; . \; . \; . \quad \text{(3b)}$$

This is the equation of a cone with its vertex at the origin, which passes through the direction-circle, because the equation was derived from equations (3) and (3a) in which x, y, z denote the coördinates of any point on the direction-circle. The surface represented by equation (3b) is a cone, because if the equation is satisfied by the coördinates of a point x, y, z, it will also be satisfied by the coördinates of all those points for which the ratios $\frac{y}{x}$ and $\frac{z}{x}$ have the same values.

But the equations $\frac{y}{x} = C_0$ and $\frac{z}{x} = C_1$ are the equations of a straight line going through the origin. Since, therefore, all points of a straight line going through the origin and through a point on the surface (3b) are on this surface, the surface must be a conical surface.

Any straight line drawn on the surface of this cone will be a direction that the line of fixation may have when it passes through the given direction-circle.

When a linear after-image is projected along a direction-circle, the after-image, as has been stated, continues to lie along the circle, when the eye traverses its various points. The after-images were projected

above on a plane perpendicular to the primary position of the eye, whose equation, therefore, is

$$x = C \ .$$

If x in equation (3b) is supposed to be constant, we obtain the equation of an hyperbola, which is the projection of the direction-circle on the aforesaid plane. The equation is

$$0 = (b^2 - a^2)y^2 + (c^2 - a^2)z^2 + 2bcyz + 2abxy + 2acxz \quad . \ . \ . \quad (3c)$$

In this general form the equation represents all hyperbolas along which any linear after-images may be shifted.

On the other hand, if the after-images are such as were originally parallel to some definite direction, say, the z-axis, then in equation (3a) representing the direction-circle we must put the coëfficient $c = 0$; and if, besides, we put

$$a = -\sin \frac{\alpha}{2} \ , \quad b = +\cos \frac{\alpha}{2} \ ,$$

equation (3c) will become:

$$0 = y^2 \cos \alpha - z^2 \sin^2 \frac{\alpha}{2} - xy \sin \alpha \ ,$$

or

$$\cos \alpha \left(y - \frac{1}{2} x \tan \alpha \right)^2 - z^2 \sin^2 \frac{\alpha}{2} = \frac{1}{4} x^2 \cos \alpha \tan^2 \alpha \ .$$

If we put

$$\frac{1}{2} x \tan \alpha = f$$

and

$$x \sqrt{\frac{\tan \alpha}{2 \tan \dfrac{\alpha}{2}}} = g \ ,$$

the equation of the hyperbola becomes

$$\frac{(y-f)^2}{f^2} - \frac{z^2}{g^2} = 1 \ .$$

Accordingly, the real axis of the hyperbola corresponds to f, and the imaginary axis to g, the distance of its centre from the line $z = 0$ being equal to the length of the real axis. One vertex of this family of hyperbolas lies on the x-axis at the point $z = 0$, $y = 0$, but those branches of the curves that go through this point are not optical projections of the

given direction-circle. They are rather merely geometrical projections of the posterior, invisible half of the circle. The hyperbolas in Fig. 1 were constructed according to these data.

It still remains to determine the rotation that the eye undergoes according to LISTING's law with reference to the visual plane (*Visierebene*). Let the plane $v=0$ be the retinal horizon of the eye, so that $y=0$ is its primary position and also the primary position of the visual plane. Then the y-axis will be the line joining the centres of rotation of the two eyes. Hence the visual plane must always contain this axis. The general equation of such planes is

$$ax+bz=0\ .$$

For the line of fixation, $v=\zeta=0$, and therefore by equation (2b):

$$x=\xi \cos a\ , \qquad y=\xi \sin a \cos \vartheta\ , \qquad z=-\xi \sin a \sin \vartheta\ ,$$

and since the line of fixation must be in the visual plane, these values of x and z must satisfy the general equation of this plane; and hence

$$a\,\xi \cos a - b\,\xi \sin a \sin \vartheta = 0\ .$$

This condition will be fulfilled by putting

$$a=\sin a \sin \vartheta\ , \qquad b=\cos a\ .$$

Thus the equation of the visual plane becomes

$$x \sin a \sin \vartheta + z \cos a = 0\ ,$$

or after substituting the values from equation (2b):

$$0=v \cos \vartheta \sin \vartheta (1-\cos a) - \zeta(\sin^2 \vartheta + \cos a \cos^2 \vartheta) \quad . \ . \ . \ . \quad (4)$$

If

$$ax+by+cz+d=0\ ,$$
$$\alpha x+\beta y+\gamma z+\delta=0$$

are the equations of two planes, the angle k which they make with each other is evidently

$$\cos k = \frac{a\alpha + b\beta + c\gamma}{\sqrt{a^2+b^2+c^2}\ \sqrt{\alpha^2+\beta^2+\gamma^2}}\ .^{1}$

[1] ¶The subsequent mathematical analysis as given originally in the first edition (pp. 496, 497), and as reproduced in the third edition (Vol. III, pp. 71, 72), contained a mistake, which, while it did not affect the final results, was corrected by HELMHOLTZ in the second edition. The revised version of the second edition was inserted by Professor v. KRIES in the third edition in Note 10 at the end of this chapter (pp. 123-125). The editor of the English translation has ventured to omit entirely the text of the first edition at this place and to substitute here Professor v. KRIES's Note 10 above mentioned. (J.P.C.S.)

Hence, the angle between the visual plane given by equation (4) and the retinal horizon, whose equation is

$$0 = \zeta \;, \qquad \qquad (4a)$$

is given by the formula

$$\cos k = -\frac{\sin^2\vartheta + \cos\alpha \cdot \cos^2\vartheta}{\sqrt{\sin^2\vartheta + \cos^2\alpha \cos^2\vartheta}}$$

or

$$\tan k = \frac{\cos\vartheta \sin\vartheta(1-\cos\alpha)}{\sin^2\vartheta + \cos\alpha\cos^2\vartheta} \qquad \qquad (4b)$$

The angle k between the temporary position of the retinal horizon and the visual plane is found by means of this formula.

The angle k' between the plane of the originally vertical meridian $v=0$ and a plane containing the vertical z-axis and the line of fixation, whose equation is

$$x \sin\alpha \cos\vartheta - y \cos\alpha = 0 \;,$$

is obtained similarly, as follows:

$$\cot k' = \frac{\cos\vartheta \sin\vartheta(1-\cos\alpha)}{\cos^2\vartheta + \sin^2\vartheta \cos\alpha} \qquad \qquad (4c)$$

Frequently the angles α and ϑ are not used for defining the position of the line of fixation, but instead of them either the *angle of elevation* λ and the *angle of azimuth* μ, as defined above, or the angles of longitude and latitude, as used by Fick, which may be denoted by l and m. These angles must be introduced in formulae (4b) and (4c) in order to adapt them for calculating results carried out in this way.

The *angle of elevation* λ is the angle between the visual plane

$$x \sin\alpha \sin\vartheta + z \cos\alpha = 0$$

and the plane $z=0$, and hence

$$\tan\lambda = \frac{z}{x} = -\tan\alpha \sin\vartheta \;.$$

The *angle of azimuth* is equal to the angle between the equatorial plane of the eye $\xi = 0$ and the plane through the y-axis perpendicular to the visual plane, namely,

$$x \cos\alpha - z \sin\alpha \sin\vartheta = 0 \;;$$

which, after substituting the values from equation (2b), may be written as follows:

$$0=\xi\,(\cos^2 a+\sin^2 a\,\sin^2\vartheta)-v\,\sin a\,\cos\vartheta(\sin^2\vartheta+\cos a\,\cos^2\vartheta)$$
$$+\zeta\,\sin a\,\sin\vartheta\,\cos^2\vartheta(\cos a-1).$$

Hence, by the same rules as above, for the angle μ between this plane and the plane $\xi=0$, we have:

$$\cos\mu=\sqrt{\cos^2 a+\sin^2 a\,\sin^2\vartheta}\,.$$

Thus for the determination of the angles a and ϑ we have the two equations:

$$\tan\lambda=-\tan a\,\sin\vartheta\,,$$
$$\cos^2\mu=\cos^2 a+\sin^2 a\,\sin^2\vartheta\,,$$

whence we get:

$$\cos a=\cos\mu\,\cos\lambda\,,$$
$$\sin\vartheta=\mp\frac{\cos\mu\,\sin\lambda}{\sqrt{1-\cos^2\mu\,\cos^2\lambda}}\,,$$

or

$$\tan\vartheta=\sin\lambda\,\cot\mu\,.$$

When these values are substituted in equations (4b) and (4c), we obtain:

$$\tan k=-\frac{\sin\mu\,\sin\lambda}{\cos\mu+\cos\lambda} \quad \ldots\ldots\ldots \quad (4d)$$

and

$$\tan k'=\frac{\sin\mu\,\cos\mu\,\sin\lambda(1-\cos\mu\,\cos\lambda)}{\sin^2\mu+\cos^3\mu\,\sin^2\lambda\,\cos\lambda}\,.$$

By a similar method we find:

$$\tan k=-\frac{\sin m\,\cos m\,\sin l(1-\cos m\,\cos l)}{\sin^2 m+\cos^3 m\,\sin^2 l\,\cos l}\,,$$

$$\tan k'=\frac{\sin m\,\sin l}{\cos m+\cos l} \quad \ldots\ldots\ldots \quad (4e)$$

These angles are reckoned as positive or negative, as was explained above.

By employing the half-angles instead of the whole angles k, μ, λ and

k', m, l, equations (4d) and (4e) may be put in forms convenient for logarithmic computation, as follows:

$$\left.\begin{aligned} \tan\frac{k}{2} &= -\tan\frac{\mu}{2}\cdot\tan\frac{\lambda}{2} \\ \tan\frac{k'}{2} &= \tan\frac{m}{2}\cdot\tan\frac{l}{2} \end{aligned}\right\} \quad \ldots\ldots \quad (4f)$$

Derivation of the law of rotation from the principle of easiest orientation.—The next step is to calculate the differences in the rolling movement of the eye when the rotations are executed around axes that are not perpendicular to the atropic line. In Fig. 11 suppose that ab represents the visual axis and ad the axis of rotation about which the eye turns; in which case the visual axis ab may be supposed to describe the infinitesimal arc ds perpendicular to the plane of the diagram. Let Δ denote the angular displacement due to the rotation around ad. This rotation may be regarded as compounded of a rotation around the axis ac perpendicular to ab and of a rotation around ab itself. The magnitude of the latter must be equal to $\Delta \cos \lambda'$ where λ' denotes the angle dab as shown in the diagram. But the magnitude denoted by Δ is defined by the fact that ab has to traverse the arc ds. In this case the perpendicular bf let fall from b on to the axis is the radius vector for the motion of the point b; that is,

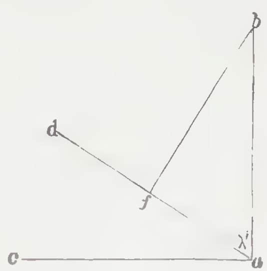

Fig. 11.

$$ab \cdot ds = fb \cdot \Delta$$

or

$$ds = \Delta \sin \lambda' \,.$$

Thus, the rolling movement around the line ab in case of this motion will be equal to

$$ds \cot \lambda' \,.$$

Suppose now that planes are passed through ab in different directions. The element ds may be resolved in each of these planes, and the corresponding axes of rotation must all lie in one plane, if the movements of the eye are to proceed continuously from ab into one another. One of the planes through ab must be perpendicular to the plane of the axes of rotation in which ad lies. For this plane suppose the angle λ'

has the value λ; and let ϵ be the angle between this plane and the plane containing the element of arc ds and the visual axis ab. According to a familiar formula of spherical trigonometry, in the right-angle triangle made by the plane of the axes of rotation and the planes of the angles λ and λ', we have

$$\cot \lambda' = \cot \lambda \cdot \cos \epsilon .$$

Thus the rotation around the line ab is

$$ds \cdot \cot \lambda' = ds \cdot \cot \lambda \cdot \cos \epsilon .$$

Now if the line of fixation ab in the same adjustment makes the angle μ with the plane that is normal to the atropic line, and if κ denotes the angle between the planes of the two angles μ and λ that contain ab, a calculation like that just made would show that the rotation around the visual axis would have to be equal to

$$ds \cdot \cot \mu \cdot \cos(\epsilon - \kappa) ,$$

if the rotations had to be performed everywhere according to the requirements of the easiest orientation, whereby the axes of rotation would have to be always perpendicular to the atropic line.

If ρ denotes the difference between the required rotation and the actual rotation, then

$$\rho^2 = ds^2 \left\{ \cot \lambda \cos \epsilon - \cot \mu \cos(\epsilon - \kappa) \right\}^2 .$$

Hence, according to the principle of easiest orientation, *the sum of all values of ρ^2 for all possible infinitesimal movements of the line of fixation in the field of fixation, amounting to* ds, *must be a minimum.*

Let us take first the sum of all values of ρ^2 for displacements ds in different directions from one and the same position of the line of fixation, that is, for different values of the angle ϵ. Then

$$\int_0^{2\pi} \rho^2 d\epsilon = \pi ds^2 \left\{ \cot^2 \lambda + \cot^2 \mu - 2 \cot \lambda \cdot \cot \mu \cdot \cos \kappa \right\} \quad . \quad . \quad . \quad . \qquad (5)$$

This expression has to be integrated again for all different positions of the line of fixation in the field that are given by the angles a and ϑ. Thus we have the integral

$$\int_0^{2\pi} d\vartheta \int_0^{a_0} da \int_0^{2\pi} d\epsilon \cdot \rho^2 \sin a = R \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5a)$$

where a_0 denotes the value corresponding to the limit of the field of fixation.

In order to perform this integration, the values of λ and κ must be found that correspond to the various values of α and ϑ. For this purpose let us differentiate equations (1c) with respect to α and ϑ, regarding the angle ω as a function of the other two angles, and considering ξ, v, ζ as constants. For the points on the axis of rotation, we must have

$$dx=dy=dz=0\ .$$

Hence we obtain the following equations which are also valid for points on the axis of rotation:

$$\left.\begin{aligned} a\,dx + a_{,}dy + a_{,,}dz &= 0\\ b\,dx + b_{,}dy + b_{,,}dz &= 0\\ c\,dx + c_{,}dy + c_{,,}dz &= 0 \end{aligned}\right\} \quad \cdots\cdots\cdot \quad (6)$$

where the magnitudes a, b, c, etc. denote the coëfficients of equations (1c); that is,

$$\begin{aligned} a &= \cos\alpha\\ a_{,} &= \sin\alpha\cos\vartheta\\ a_{,,} &= -\sin\alpha\sin\vartheta\\ b &= -\cos\omega\sin\alpha\\ b_{,} &= \cos\alpha\cos\vartheta\cos\omega - \sin\vartheta\sin\omega\\ b_{,,} &= -\cos\alpha\sin\vartheta\cos\omega - \cos\vartheta\sin\omega\\ c &= -\sin\omega\sin\alpha\\ c_{,} &= \cos\alpha\cos\vartheta\sin\omega + \sin\vartheta\cos\omega\\ c_{,,} &= -\cos\alpha\sin\vartheta\sin\omega + \cos\vartheta\cos\omega\ . \end{aligned}$$

These magnitudes are connected with each other by the following well-known formulae:

$$\left.\begin{aligned} 1 &= a^2 + a_{,}^2 + a_{,,}^2 \qquad & ab + a_{,}b_{,} + a_{,,}b_{,,} &= 0\\ 1 &= b^2 + b_{,}^2 + b_{,,}^2 & ac + a_{,}c_{,} + a_{,,}c_{,,} &= 0\\ 1 &= c^2 + c_{,}^2 + c_{,,}^2 & bc + b_{,}c_{,} + b_{,,}c_{,,} &= 0 \end{aligned}\right\} \quad \cdots \quad (A)$$

$$\left.\begin{aligned} 0 &= a\,da + a_{,}da_{,} + a_{,,}da_{,,}\\ &\ a\,db + a_{,}db_{,} + a_{,,}db_{,,} = -(b\,da + b_{,}da_{,} + b_{,,}da_{,,})\\ 0 &= b\,db + b_{,}db_{,} + b_{,,}db_{,,}\\ &\ a\,dc + a_{,}dc_{,} + a_{,,}dc_{,,} = -(c\,da + c_{,}da_{,} + c_{,,}da_{,,})\\ 0 &= c\,dc + c_{,}dc_{,} + c_{,,}dc_{,,}\\ &\ b\,dc + b_{,}dc_{,} + b_{,,}dc_{,,} = -(c\,db + c_{,}db_{,} + c_{,,}db_{,,}) \end{aligned}\right\} \quad \cdots \quad (B)$$

Substituting the following values of dx, dy, dz in equations (6):

$$dx = \xi da + v db + \zeta dc$$
$$dy = \xi da_{,} + v db_{,} + \zeta dc_{,}$$
$$dx = \xi da_{,,} + v db_{,,} + \zeta dc_{,,}$$

we obtain:

$$\left.\begin{aligned} 0 &= v(adb + a_{,}db_{,} + a_{,,}db_{,,}) + \zeta(adc + a_{,}dc_{,} + a_{,,}dc_{,,}) \\ 0 &= \xi(bda + b_{,}da_{,} + b_{,,}da_{,,}) + \zeta(bdc + b_{,}dc_{,} + b_{,,}dc_{,,}) \\ 0 &= \xi(cda + c_{,}da_{,} + c_{,,}da_{,,}) + v(cdb + c_{,}db_{,} + c_{,,}db_{,,}) \end{aligned}\right\} \quad . \; . \quad (6a)^{1}$$

These three equations[2] give each of the coördinates of the axis of rotation in terms of each of the other coördinates.

Let $\left(\frac{\pi}{2} - \lambda\right)$ denote the angle between a line normal to the plane of the axes of rotation and the ξ-axis (line of fixation), and let κ denote the angle between the plane of the angle λ and the $v\xi$-plane; corresponding to the notation in equation (5) and on the assumption that the latter plane passes through the atropic line. Then for the plane of the axes of rotation

$$\xi \sin\lambda + v \cos\lambda \cos\kappa + \zeta \sin\lambda \sin\kappa = 0 ;$$

which, after taking the values of v and ζ from the last two of equations (6a), and multiplying by

$$(bdc + b_{,}dc_{,} + b_{,,}dc_{,,}) = -(cdb + c_{,}db_{,} + c_{,,}db_{,,}) ,$$

may be written:

$$\left.\begin{aligned} 0 = \sin\lambda(bdc + b_{,}dc_{,} + b_{,,}dc_{,,}) - \cos\lambda \cos\kappa(cda + c_{,}da_{,} + c_{,,}da_{,,}) \\ = \cos\lambda \sin\kappa(bda + b_{,}da_{,} + b_{,,}da_{,,}) \end{aligned}\right\} \quad . \; . \; . \quad (6b)$$

[1] In HELMHOLTZ's collected scientific papers (Leipzig 1883) these formulae are derived somewhat differently.—K.

[2] It is evident from equations (B) that the third of these equations is an identical result obtained from the other two. If the magnitude ω in equations (1e) is a continuous function of a and ϑ, that is, if

$$d\omega = \frac{d\omega}{da} da + \frac{d\omega}{d\vartheta} d\vartheta,$$

the differentials da, db, dc, etc. will all be of the form

$$da = \frac{da}{da} da + \frac{da}{d\vartheta} d\vartheta.$$

If $\frac{da}{d\vartheta}$ is eliminated from two of the equations (6a), we get an equation which, after dividing through by ζ, is linear with respect to ξ, v, ζ, and which is therefore the equation of a plane which must contain all axes of rotation for infinitesimal movements of the eye from the given position. This constitutes the proof of the auxiliary proposition mentioned above, that for continuous movements of the eye and infinitesimal rotations there is a plane of the axes of rotation for each position.

This equation resolves into two equations, if $d\alpha$ and $d\vartheta$ are independent of each other, since every differential is of the form:

$$d\alpha = \frac{d\alpha}{d\alpha} d\alpha + \frac{d\alpha}{d\vartheta} d\vartheta \, .$$

Hence if the differentials are collected in equation (6b), and taken first with respect to α and then with respect to ϑ, we derive the following two equations:

$$0 = \sin\lambda \frac{d\omega}{d\alpha} - \cos\lambda \cos\kappa \sin\omega + \cos\lambda \sin\kappa \cos\omega \, ,$$

$$0 = \sin\lambda \left(\frac{d\omega}{d\vartheta} + \cos\alpha \right) + \cos\lambda \cos\kappa \sin\alpha \cos\omega + \cos\lambda \sin\kappa \sin\alpha \sin\omega \, .$$

By eliminating $\cos\kappa$ or $\sin\kappa$ from the last two equations, we get:

$$\sin\lambda \left(\sin\alpha \sin\omega \frac{d\omega}{d\alpha} - \cos\omega \frac{d\omega}{d\vartheta} - \cos\omega \cos\alpha \right) = \cos\lambda \cos\kappa \sin\alpha ,$$

$$\sin\lambda \left(\sin\alpha \cos\omega \frac{d\omega}{d\alpha} + \sin\omega \frac{d\omega}{d\vartheta} + \sin\omega \cos\alpha \right) = -\cos\lambda \sin\kappa \sin\alpha \, .$$

Dividing both equations by $\sin\lambda \cdot \sin\alpha$, we obtain from the first one the value of $\cot\lambda \cdot \cos\kappa$ that is needed for substitution in equation (5); and if both of them are squared and added, we get:

$$\cot^2\lambda = \left(\frac{d\omega}{d\alpha} \right)^2 + \frac{1}{\sin^2\alpha} \left(\frac{d\omega}{d\vartheta} + \cos\alpha \right)^2 .$$

Thus, finally, we get for the value of the integral R that has to be a minimum:

$$\left. \begin{aligned} R = \pi ds^2 \int_0^{2\pi} d\vartheta \int_0^{\alpha_0} d\alpha \Bigg\{ & \sin\alpha \left(\frac{d\omega}{d\alpha} \right)^2 \\ & + \frac{1}{\sin\alpha} \left(\frac{d\omega}{d\vartheta} + \cos\alpha \right)^2 - 2\cot\mu \left[\sin\alpha \sin\omega \frac{d\omega}{d\alpha} - \cos\omega \left(\frac{d\omega}{d\vartheta} + \cos\omega \right) \right] \\ & + \cot^2\mu \sin\alpha \Bigg\} \, . \end{aligned} \right\} \quad (6c)$$

In this expression ω and μ are variables. In order that R shall be a minimum, the variations with respect to both of these magnitudes must be put equal to zero. Hence

$$\left.\begin{aligned}0=\int_0^{2\pi} d\vartheta \int_0^{a_\circ} da \Bigg\{ &\sin a \frac{d\omega}{da}\cdot\frac{d\delta\omega}{da}+\frac{1}{\sin a}\left(\frac{d\omega}{d\vartheta}+\cos a\right)\frac{d\delta\omega}{d\vartheta} \\ -\cot\mu\Bigg[&\left[\sin a \cos\omega\frac{d\omega}{da}+\sin\omega\left(\frac{d\omega}{da}+\cos a\right)\right]d\omega \\ &+\sin a\sin\omega\frac{d\delta\omega}{da}-\cos\omega\frac{d\delta\omega}{d\vartheta}\Bigg]\Bigg\}\end{aligned}\right\} \quad . . . \quad (6d)$$

and

$$\left.\begin{aligned}&\cot\mu\int_0^{2\pi} d\vartheta\int_0^{a_\circ}\sin a\, da \\ &=\int_0^{2\pi} d\vartheta\int_0^{a_\circ} da\left[\sin a\sin\omega\frac{d\omega}{da}-\cos\omega\left(\frac{d\omega}{d\vartheta}+\cos a\right)\right]\end{aligned}\right\} \quad . . (6e)$$

The magnitudes $\frac{d\delta\omega}{da}$ and $\frac{d\delta\omega}{d\vartheta}$ may be removed from equation (6d) by partial integration; in which case two integrals will be obtained, one extending along the circumference of the field of fixation, and the other over its surface, both of which contain simply $\delta\omega$ as factor under the integral sign. However, before performing this integration, we must investigate whether the function to be integrated does not have multiple values or become discontinuous in the interior of the field of fixation. Now it has been already remarked that for very small values of a in the vicinity of the initial position of the eye the magnitude $(\omega+\vartheta)$ must be equal to zero. But ϑ increases from 0 to 2π when the line of fixation is made to describe an infinitesimal circle once around the initial position, in which case therefore ω will vary from 0 to -2π and will be discontinuous in the vicinity of the initial position. Consequently, it is better to introduce a new variable,

$$\eta=\omega+\vartheta ,$$

which will be continuous all over the field of fixation. Then

$$\frac{d\omega}{da}=\frac{d\eta}{da} \quad \text{and} \quad \frac{d\omega}{d\vartheta}=\frac{d\eta}{d\vartheta}-1 ;$$

$$\delta\omega=\delta\eta .$$

If after making this substitution we perform the partial integration of equation (6d), so as to take out $\frac{d\delta\eta}{da}$ and $\frac{d\delta\eta}{d\vartheta}$, then afterwards, by the principles of the calculus of variations, the factors that are mul-

tiplied by $\delta\eta$ must be put equal to zero in the two integrals, both the integral along the circumference and the one over the surface. Thus we obtain:

1. In case of the integral along the circumference, by supposing that it is traversed so that ϑ increases:

$$\left.\begin{aligned} 0 &= \sin\alpha\frac{d\eta}{d\alpha}d\vartheta - \left(\frac{d\eta}{d\vartheta} - 1 + \cos\alpha\right)\frac{d\alpha}{\sin\alpha} \\ &- \cot\mu\left[\sin\alpha\cdot\sin(\eta-\vartheta)d\vartheta + \cos(\eta-\vartheta)d\alpha\right] \end{aligned}\right\} \quad . \quad . \quad (7)$$

2. In case of the integral over the surface of the field of fixation:

$$0 = \frac{d}{d\alpha}\left(\sin\alpha\frac{d\eta}{d\alpha}\right) + \frac{1}{\sin\alpha}\,\frac{d^2\eta}{d\vartheta^2}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7a)$$

and in conjunction with this equation we must use equation (6e), which also admits of a single integration:

$$\cot\mu\int_0^{2\pi}(1-\cos\alpha)d\vartheta = \int\left[-\sin\alpha\cos(\eta-\vartheta)d\vartheta + \sin(\eta-\vartheta)d\alpha\right] \quad . \quad . \quad (7b)$$

Both integrals in this latter equation are to be taken over the entire circumference. The integral on the left-hand side which is multiplied by $\cot\mu$ is evidently the area of the field of fixation. In order to simplify these equations, let us introduce another variable in place of α, namely,

$$\beta = \log\cdot\tan\frac{\alpha}{2}\,,$$

so that

$$e^\beta = \tan\frac{\alpha}{2}\,, \qquad \frac{2e^\beta}{1+e^{2\beta}} = \sin\alpha\,,$$

$$d\beta = \frac{d\alpha}{\sin\alpha}\,, \qquad \frac{1-e^{2\beta}}{1+e^{2\beta}} = \cos\alpha\,;$$

and if ψ is a function of α, then

$$\frac{d\psi}{d\beta} = \frac{d\psi}{d\alpha}\sin\alpha\,.$$

Substituting these values in equation (7a), we obtain the following equation for the interior of the field:

$$\frac{d^2\eta}{d\beta^2} + \frac{d^2\eta}{d\vartheta^2} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7c)$$

Then from equation (7) for the circumference:

$$\left.\begin{aligned}0=\frac{d\eta}{d\beta}d\vartheta-\left(\frac{d\eta}{d\vartheta}-\frac{2e^{2\beta}}{1+e^{2\beta}}\right)d\beta\\-\cot\mu\cdot\frac{2e^{\beta}}{1+e^{2\beta}}\Big[\sin(\eta-\vartheta)d\vartheta+\cos(\eta-\vartheta)d\beta\Big]\end{aligned}\right\}\quad . \; . \; . \; . \quad (7d)$$

and finally from equation (7b):

$$\cot\mu\int\frac{2e^{2\beta}}{1+e^{2\beta}}d\vartheta=\int\frac{2e^{\beta}}{1+e^{2\beta}}\Big[\sin(\eta-\vartheta)\,d\beta-\cos(\eta-\vartheta)\,d\vartheta\Big]\quad . \quad (7e)$$

All real integrals of equation (7e) may be represented as the real part of any function ψ of the complex magnitude $(\beta+\vartheta i)$. Suppose we put

$$\psi=\varphi+\chi^{i}\,,\quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (8)$$

where φ and χ are real; then both φ and χ may be integrals of equation (7c).

If φ is to be an integral suitable for our purpose, in the first place it must be finite and single-valued all over the field of fixation, including $a=0$ or $\beta=-\infty$. And, secondly, it must also satisfy equations (7d) and (7e) along the contour of the field of fixation.

Denoting the derivative of ψ with respect to the complex variable $(\beta+\vartheta i)$ by ψ', we get from equation (8):

$$\frac{d\psi}{d\beta}=\psi'=\frac{d\varphi}{d\beta}+i\frac{d\chi}{d\beta}\,,$$

$$\frac{d\psi}{d\vartheta}=i\psi'=\frac{d\varphi}{d\vartheta}+i\frac{d\chi}{d\vartheta}\,.$$

Hence when ψ' is eliminated,

$$0=i\frac{d\varphi}{d\beta}-\frac{d\chi}{d\beta}-\frac{d\varphi}{d\vartheta}-i\frac{d\chi}{d\vartheta}\,,$$

or

$$\left.\begin{aligned}\frac{d\chi}{d\beta}+\frac{d\varphi}{d\vartheta}=0\\\frac{d\chi}{d\vartheta}-\frac{d\varphi}{d\beta}=0\end{aligned}\right\}\quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (8a)$$

Moreover, if we put

$$Y=Y_0+iY_1=e^{\chi-\phi i+\beta+\theta i}\,,$$

this magnitude likewise is a function of $(\beta+\vartheta i)$, and consequently

$$\left.\begin{aligned}\frac{dY_0}{d\vartheta}+\frac{dY_1}{d\beta}=0\\ \frac{dY_0}{d\beta}-\frac{dY_1}{d\vartheta}=0\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8b)$$

and

$$\left.\begin{aligned}Y_0&=e^{\chi}e^{\beta}\cos(\varphi-\vartheta)\\ Y_1&=-e^{\chi}e^{\beta}\sin(\varphi-\vartheta)\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8c)$$

Now if the magnitude φ is substituted for η in equation (7d), and the equation multiplied by the factor

$$e^{\sigma}=e^{\chi}(1+e^{2\beta})\ ,$$

where

$$\sigma=\chi+\log(1+e^{2\beta})\ ,$$

we obtain by taking account of equations (8a) and (8c):

$$0=e^{\sigma}\frac{d\sigma}{d\vartheta}d\vartheta+e^{\sigma}\frac{d\sigma}{d\beta}d\beta+2\cot\mu\Big[Y_1 d\vartheta-Y_0 d\beta\Big] \quad . \quad . \quad . \quad . \quad (8d)$$

This equation is a perfect differential, since by equation (8b)

$$\frac{dY_1}{d\beta}=\frac{d}{d\vartheta}\Big(-Y_0\Big)\ .$$

Indeed, when the function Y is integrated with respect to the complex variable $(\beta+\vartheta i)$, and the integral is

$$\Phi=\Phi_0+i\Phi_1\ ,$$

we have:

$$\Phi'=Y\ ,$$

or

$$\frac{d\Phi_0}{d\beta}+i\frac{d\Phi_1}{d\beta}=Y_0+iY_1\ ,$$

$$\frac{d\Phi_0}{d\vartheta}+i\frac{d\Phi_1}{d\vartheta}=iY_0-Y_1\ ,$$

that is,

$$Y_0=\frac{d\Phi_0}{d\beta}=\frac{d\Phi_1}{d\vartheta}\ ,$$

$$Y_1=\frac{d\Phi_1}{d\beta}=-\frac{d\Phi_0}{d\vartheta}\ .$$

Thus when equation (8d) is integrated we get for the contour of the field

$$C = e^{\sigma} - 2 \cot \mu \cdot \Phi_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8e)$$

or

$$\sigma = \chi + \log(1 + e^{2\beta}) = \log(C + 2 \cot \mu \cdot \Phi_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad (8f)$$

However, the constants C and μ must also finally satisfy equation (7e), if μ is to be the angle that corresponds best to the requirements of the principle of easiest orientation.

Now it can be proved that the value $\cot \mu = 0$ corresponds simultaneously to equations (8f) and (7e). For the integral taken over the whole contour of the field is

$$\int Y_0 d\vartheta + Y_1 d\beta = \int \frac{d\Phi_1}{d\vartheta} d\vartheta + \frac{d\Phi_1}{d\beta} d\beta = 0 \ ,$$

provided, as must be the case according to the above assumption concerning φ, that Φ_1 likewise is finite and continuous everywhere; because this integral is equal to the difference of the values of Φ_1 which this magnitude has at the same point of the periphery before and after traversing its entire length. Substituting the values of Y_0 and Y_1 as given by equations (8c), we have:

$$0 = \int \frac{e^{\sigma} \cdot e^{\beta}}{1 + e^{2\beta}} \Big[\cos(\varphi - \vartheta) d\vartheta - \sin(\varphi - \vartheta) d\beta \Big] \ .$$

Now if we put $\cot \mu = 0$, it follows from equation (8f) that the magnitude denoted by σ is constant all over the contour, and hence the factor e^{σ} can be taken from under the integral sign. Thus on the assumption that $\cot \mu = 0$, we may write:

$$0 = \int \frac{e^{\beta}}{1 + e^{2\beta}} \Big[\cos(\varphi - \vartheta) d\vartheta - \sin(\varphi - \vartheta) d\beta \Big] \ ;$$

whence it follows that equation (7e) is satisfied on the given assumption.

As far as I can see, for any arbitrarily given form of the field of fixation, it is not possible to answer the question as to whether the conditions of the problem might not be satisfied by other values besides $\cot \mu = 0$. However, since the actual field of fixation is pretty nearly circular, it will be sufficient here to show that in the case of a circular form there is no other real value except $\mu = 0$.

The law of rotation for a circular field of fixation

Since the required function η must be the real part of an arbitrary function of $(\beta+\vartheta i)$, which does not become infinite or multiple-valued for any point in the field of fixation, including also the point for which $\beta=-\infty$, the general form of it must be

$$\left.\begin{aligned}\eta=A_0+A_1e^{\beta}\cos(\vartheta+c_1)+A_2e^{2\beta}\cos(2\vartheta+c_2)\\ +A_3e^{3\beta}\cos(3\vartheta+c_3)+\text{etc}\ .\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (9)$$

where the magnitudes denoted by A and c are arbitrary constants. Then the corresponding value of χ will be:

$$\left.\begin{aligned}\chi=A_1e^{\beta}\sin(\vartheta+c_1)+A_2e^{2\beta}\sin(2\vartheta+c_2)\\ +A_3e^{3\beta}\sin(3\vartheta+c_3)+\text{etc}\ .\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (9a)$$

And if $\cot\mu=0$, the equation of the contour is:

$$\chi=\log\frac{C}{1+e^{2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9b)$$

The magnitude e^{β} occurring in all these equations is equal to $\tan\dfrac{a}{2}$. Accordingly, if the equation between a and ϑ that determines the contour-line can be brought into the form of equation (9b), the problem will be solved, since the angle η which measures the deviation from Listing's law can always be easily obtained from χ.

Let us proceed to investigate the form of the field on the assumption that η is constant. As the absolute value of this magnitude does not matter at all, suppose that

$$\eta=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

On the other hand, the value of $\cot\mu$ will be left indefinite.

On the assumption (10), it follows that $\chi=0$ also, and the magnitudes denoted by Y in equations (8c) become:

$$Y_0 \ =e^{\beta}\cos\vartheta\ ,$$

$$Y_1 \ =e^{\beta}\sin\vartheta\ ,$$

$$Y_0+Y_1i=e^{\beta+\vartheta i}=\Phi_0+\Phi_1 i\ .$$

Hence the equation of the contour-line (8f) becomes:

$$1+e^{2\beta}=C+2e^{\beta}\cos\vartheta\ \cot\mu\ .$$

Substituting $\tan\dfrac{a}{2}$ instead of e^{β}, we may write this equation as follows:

$$\tan\frac{a}{2}+(1-C)\cot\frac{a}{2}=2\cos\vartheta\ \cot\mu \quad . \quad . \quad . \quad . \quad . \quad . \quad (10a)$$

This is the equation of a circle. For in the spherical triangle shown in Fig. 12, according to a familiar formula:

$$\cos\rho = \cos\alpha\cos\gamma + \sin\alpha\sin\gamma\cdot\cos\vartheta\ ,$$

which, by expressing $\sin\alpha$ and $\cos\alpha$ in terms of $\tan\dfrac{\alpha}{2}$, may be put in the following form:

$$\cos\rho\left(1+\tan^2\frac{\alpha}{2}\right) = \cos\gamma\left(1-\tan^2\frac{\alpha}{2}\right) + 2\tan\frac{\alpha}{2}\sin\gamma\cos\vartheta\ ,$$

or

$$(\cos\rho+\cos\gamma)\tan\frac{\alpha}{2} + (\cos\rho-\cos\gamma)\cot\frac{\alpha}{2} = 2\sin\gamma\cos\vartheta\quad . \quad . \quad (10b)$$

Accordingly, if we put

$$\frac{\cos\rho-\cos\gamma}{\cos\rho+\cos\gamma} = 1-C \text{ and } \frac{\sin\gamma}{\cos\rho+\cos\gamma} = \cot\mu\ ,\quad . \quad . \quad . \quad (10c)$$

equation (10b) will be identical with equation (10a), and from the last two equations we obtain a constant value for ρ, which denotes the distance of the point B in the contour of the field of fixation from the point A measured along the spherical surface. Accordingly, for $\eta=0$ the contour of the field is a circle, with its centre at A, whose spherical radius is equal to ρ.

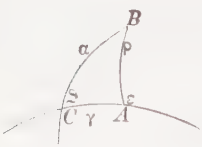

Fig. 12.

The second equation of the contour may be used in the form of equation (7b). The integral on the left-hand side of this equation, as was remarked above, and as may be most easily perceived from its form in equation (6e), is the area of the field of fixation, which is to be expressed now in terms of ρ, so that we have:

$$2\pi\cot\mu\,(1-\cos\rho) = -\int\sin\alpha\cos\vartheta d\vartheta + \sin\vartheta d\alpha\quad . \quad . \quad . \quad . \quad (10d)$$

Now in the spherical triangle in Fig. 12 let us apply the following formulae of spherical trigonometry:

$$\cos\alpha = \cos\gamma\cos\rho - \sin\gamma\sin\rho\cos\epsilon\ ,$$

$$\sin\vartheta\sin\alpha = \sin\rho\sin\epsilon\ ;$$

and differentiate both of them with respect to α and ϑ, regarding ρ as constant for the contour of the field of fixation. Then along this contour we shall have:

$$\cos\vartheta\sin\alpha d\vartheta + \sin\vartheta\cos\alpha d\alpha = \sin\rho\cos\epsilon d\epsilon\ ,$$

$$\sin\alpha d\alpha = -\sin\gamma\sin\rho\sin\epsilon d\epsilon\ ;$$

or

$$\sin \vartheta d\alpha = -\frac{\sin \gamma \sin^2 \rho \sin^2 \epsilon d\epsilon}{\sin^2 \alpha}\ .$$

Substituting these values in the integral of equation (10d), we obtain:

$$2\pi \cot \mu(1-\cos \rho)$$

$$= -\int_0^{2\pi} \frac{\sin \rho \cos \epsilon + \cos \gamma \cos \rho \sin \rho \cos \epsilon - \sin \gamma \sin^2 \rho}{1+\cos \rho \cos \gamma - \sin \gamma \sin \rho \cos \epsilon} d\epsilon\ .$$

If by way of abbreviation we put

$$1+\cos \gamma \cos \rho = a$$

$$\sin \gamma \sin \rho = b$$

$$\tan\frac{\epsilon}{2} = x\ ,$$

the integral may be expressed as follows;

$$2\pi\cot \mu(1-\cos \rho)$$

$$= -\sin\rho \int_{+\infty}^{+\infty} \frac{a+b}{b} \frac{dx}{1+\frac{a+b}{a-b}x^2} + \frac{a \sin \rho}{b}\int_{-\infty}^{+\infty} \frac{dx}{1+x^2} = \frac{\pi \sin \rho}{b}(a-\sqrt{a^2-b^2})\ .$$

Again expressing $\cot \mu$, a and b in terms of γ and ρ, we obtain:

$$\frac{2 \sin \gamma(1-\cos \rho)}{\cos \rho + \cos \gamma} = \frac{1}{\sin \gamma}(1+\cos \gamma \cos \rho - \cos \gamma - \cos \rho)$$

or

$$2 \sin^2\gamma(1-\cos \rho) = (\cos \rho + \cos \gamma)\ [1+\cos \gamma \cos \rho - \cos \gamma - \cos \rho]\ ; \quad (10e)$$

which can also be written thus:

$$(1-\cos \gamma)\ (1-\cos \rho)\ (2+\cos \gamma - \cos \rho) = 0 \quad . \ . \ . \ . \ . \ . \quad (10f)$$

whence it follows that the only real value of $\cos \gamma$ that will satisfy this equation is

$$\cos \gamma = 1\ ,$$

and therefore

$$\sin \gamma = 0 \text{ and } \cot \mu = 0\ .$$

The second value of $\cos\gamma$ given by equation (10f) would be less than -1, namely,

$$\cos\gamma = \cos\rho - 2 \ ,$$

and so it would correspond to an imaginary arc.

The preceding analysis[1] has been made on the assumption that movements of the eye are equally frequent in all parts of the field of fixation and in all directions. This is probably not actually the case, because as a rule the line of fixation is usually kept in the central parts of its field of motion. The peripheral parts of the field, therefore, will in general be traversed less than the central parts, and consequently they will necessarily also have less effect on the law of motion than the central parts. Without knowing what it amounts to exactly, it seemed to be unnecessary to take this circumstance into account, particularly as it is not easy to see how it would affect the final result. Equation (9) may be written as follows:

$$\eta = A_0 + A_1 \tan\frac{a}{2}\cos(\vartheta + c_1) + A_2 \tan^2\frac{a}{2}\cos(2\vartheta + c_2)$$
$$+ A_3 \tan^3\frac{a}{2}\cos(3\vartheta + c_3) \text{ etc . ;}$$

and in this equation the origin of coördinates can be shifted so as to make the term vanish that involves the first power of $\tan a/2$. Hence, for small values of a, η is approximately constant, and it will be only out towards the periphery of the field, where the values of $\tan a/2$ become larger, that there may be appreciable deviations from LISTING's law. Thus, assuming that the peripheral parts of the field of fixation are generally of less account, any deviation from LISTING's law that might be due to the field's not being circular in form will necessarily be still less than they would be if the peripheral parts were often traversed.

Besides, probably it would not be strictly true to suppose that in all parts of the field of vision the eye turned equally often in all directions. At least my own experience is that I try to avoid movements that are parallel to the periphery of the field of fixation, especially when I wish to recognize distinctly the form and extent of the object in question. Then, unconsciously, I have the impulse to turn my head so that the requisite movements of the eye will occur in meridians of the field of

[1] The analysis has been carried out here further than was done when these investigations were first published in the *Archiv fur Ophthalmologie*, IX, 2. There the angle μ between the line of fixation and the atropic line was considered as being fixed, and moreover as being small. I did not succeed until afterwards in being able to prove that the consequences of the fundamental principle require that this angle shall be equal to zero.

fixation that pass through the primary position. Thus I can raise my look high along a vertical line in front of me without changing the inclination of my head; but when I wish to look along an elevated horizontal line, it is more natural for me to lift my head until it is in the primary position than simply to raise my eyes.

Accordingly, there seemed to me to be a preference for movements of the eye along those meridians of the field of fixation that go through the primary position. These are likewise the movements for which there is no apparent rotation of the object; which may be the explanation of their being preferred. The effect of this circumstance must also be that, when once LISTING'S law is obeyed in the movement of an individual eye, the tendency to deviate from the law, due to any irregularities in the field of fixation, must be reduced.

Mr. E. HERING[1] has called attention to the fact that, owing to convergence for near objects, inward rotation of the eye is comparatively more common than outward rotation. However, as shown by VOLKMANN'S experiments, and as we have tried to demonstrate theoretically above, parallel adjustments of the eyes must be, and can be, kept separate from convergent adjustments, at least in studying the law of movement of near-sighted eyes; and hence the fact mentioned above does not need to be taken into account in considering the law of the rolling motion of the eye in parallel adjustments.

On the other hand, it certainly is important to note that parallel adjustments are used mainly for the upper part of the field of fixation, because it is there only, with rare exceptions, that infinitely distant objects occur; whereas, on the contrary, convergent adjustments are employed almost exclusively for the lower parts of the field, where the floor is and where our hands are with the objects in them. Mark two points on a sheet of paper whose distance apart is the same as that of the two eyes; and look at them with the visual axes parallel, and try to make them coincide under such circumstances. It is very much easier to do it when the visual plane is lowered than when it is raised. And, conversely, convergence on a near point is much harder when the visual plane is elevated than when it is depressed. And so possibly we might expect that, in general, for convergent adjustments of the eyes, the differences in the rolling of the eyes as compared with that when the eyes were parallel would be such as if the primary position of the convergent eyes were lower and more inwards than that of the parallel eyes. And this is in accordance with observations that have been made thus far.

[1] *Beiträge zur Physiologie*, IV, S. 272.

Incidentally, I think it is likely that many an idiosyncrasy may be the result of some kindred peculiarity in the movement of the eyes, such as would be only natural in case of a law which originated simply from practice, and which can be voluntarily disobeyed. Near-sightedness too seems to me to have considerable influence, it may be partly because convergent adjustments are the main ones employed, and partly on account of the deformation of the eyeball produced by mechanical difficulties. Indeed, even the habit of wearing spectacles that are probably not perfectly centered in front of the eyes may have some effect.

Lastly, I shall venture to call attention here to a method which enormously simplifies and clarifies the complicated calculations of the positions of a point on a body which turns around a fixed point; because the latter are not easy to follow. However, in order to understand this process, the reader will have to be made familiar with the use of complex coördinates for determining the positions of points in a plane.

The method of stereographic projection ordinarily used for maps will be employed here for projecting the points on the surface of a sphere onto a plane. In Fig. 13 let AB represent the plane of projection, and let C designate the centre of the sphere whose surface is to be projected on it. The perpendicular drawn from C to the plane AB is represented by CK, and the prolongation of CK meets the spherical surface in the point D. Suppose that there is an eye at D and that the points on the surface of the sphere are transferred to those points in the plane where they would be projected by the eye at D. Thus, draw the straight line DF and prolong it to meet the plane AB in A; then A will be the projection of F.

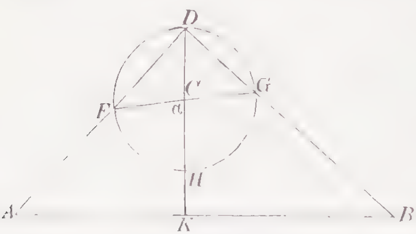

Fig. 13.

Now in this mode of projection the smallest elements of surface drawn on the sphere will be geometrically similar to the corresponding elements of the copy of this drawing in the plane, even though the scale of magnification is different in the different parts of the plane drawing. All circles drawn on the surface of the sphere will be projected as circles or as straight lines, which may be regarded as circles having an infinite radius. And, in fact, all circles on the sphere that

pass through the point D will appear as straight lines. This can readily be seen by thinking of the plane of such a circle; for it will intersect the plane AB in a straight line which is the projection of the circle in question.

Great circles, passing through the point D, and being projected therefore on the plane as straight lines, must likewise pass through the point H diametrically opposite to D; and hence their projections must go through the foot of the perpendicular CK. Accordingly, straight lines going through this point K, which is the centre of the plane figure, correspond to great circles.

For the points of that great circle of the sphere which is parallel to the plane AB, the angle FDK is equal to half a right angle, and hence the distance AK is equal to DK. Let us take this length for the unit of length. Hence, this circle will be projected on the plane as a circle of unit radius with its centre at K. We shall call it the *equatorial circle*.

All the other great circles of the sphere intersect the equatorial circle in two points diametrically opposite to each other. The corresponding points in the plane will be the opposite ends of a diameter of the equatorial circle. Hence all such circles in the plane correspond to great circles of the sphere intersecting the equatorial circle of the plane in two points diametrically opposite each other.

If the point G is diametrically opposite to the point F on the sphere, the angle FDG will be a right angle; and if B is the projection of G, then, since the right triangles AKD and DKB are similar,

$$AK : DK = DK : KB\ ,$$

and, since by hypothesis DK is the unit of length,

$$AK = \frac{1}{KB}\ .$$

Accordingly, the distances from the centre K of the projections of points at opposite ends of a diameter of the sphere are reciprocals of each other. Of course, the projections of such a pair of points will also be in a straight line passing through the centre K and will lie on opposite sides of K.

The projection of the point D on the sphere which is itself diametrically opposite the centre K, will be at infinity.

If the central angle FCH is denoted by a, the inscribed angle FDH standing on the same arc will be equal to $a/2$; and hence the distance of the projection A of the point F from the centre K will be

$$AK = DK \cdot \tan\frac{a}{2}\ ;$$

or since $DK=1$,

$$AK=\tan\frac{a}{2}\,.$$

As before, let us take the centre C of the sphere as origin of a system of coördinates ξ, v, ζ, whose ξ-axis is the normal CK_0 and whose $v\zeta$-plane is therefore parallel to the plane AB. Let t denote the angle between the plane of the diagram and the ξv-plane, and let r denote the radius of the sphere. Then the coördinates of the point F will be

$$\xi=r\cos a$$

$$v=r\sin a\cos t=2r\frac{\tan\frac{a}{2}\cdot\cos t}{1+\tan^2\frac{a}{2}}$$

$$\zeta=r\sin a\sin t=2r\frac{\tan\frac{a}{2}\cdot\sin t}{1+\tan^2\frac{a}{2}}\,.$$

Denoting the coördinates of the point A by ξ', v', ζ', we have:

$$\xi'=1-r$$

$$v'=AK\cdot\cos t=\tan\frac{a}{2}\cdot\cos t$$

$$\zeta'=AK\cdot\sin t=\tan\frac{a}{2}\cdot\sin t\,.$$

Hence,

$$v'=\frac{v}{2r\cos^2\frac{a}{2}}=\frac{v}{r+\xi}\,,$$

$$\zeta'=\frac{\zeta}{2r\cos^2\frac{a}{2}}=\frac{\zeta}{r+\xi}\,.$$

Now if v' and ζ' are combined in a single complex variable as follows:

$$\kappa=v'+i\zeta'=\frac{v+i\zeta}{r+\xi}=\tan\frac{a}{2}\cdot e^{it} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

where

$$i=\sqrt{-1}\,,$$

then there will be one point in the plane corresponding to each value of κ and hence also one point on the spherical surface.

The value of κ for the diametrically opposite point will be denoted by κ'. For this point the coördinates ξ, v, ζ have the same values, only with the opposite signs. Hence,

$$\kappa' = -\frac{v+i\zeta}{r-\xi} = -\frac{r+\xi}{v-i\zeta}$$

$$= -\frac{1}{v'-i\zeta'} = -\cot\frac{a}{2}\cdot e^{it\cdot}.$$

Therefore

$$\left.\begin{array}{ll} \dfrac{v+i\zeta}{r+\xi} = \kappa, & \dfrac{v-i\zeta}{r+\xi} = -\dfrac{1}{\kappa'} \\ \dfrac{r-\xi}{r+\zeta} = -\dfrac{\kappa}{\kappa'}, & \dfrac{2\xi}{r+\xi} = \dfrac{\kappa'+\kappa}{\kappa'} \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (11a)$$

Now let us form the corresponding expressions when the position of the sphere is changed by turning it around the point C. The coördinates x, y, z are given by equations (1c), p. 74. Let k denote the value of κ after the rotation; then corresponding to equation (11), we have:

$$k = \frac{y+iz}{r+x}$$

$$= e^{-i\vartheta}\,\frac{\xi\sin a + v(\cos a\cos\omega - i\sin\omega) + \zeta(\cos a\sin\omega + i\cos\omega)}{r+\xi\cos a - v\cos\omega\sin a - \zeta\sin\omega\sin a}$$

By expressing $\sin a$ and $\cos a$ in terms of $\tan\dfrac{a}{2}$, this expression may be put in the form:

$$k = e^{-i\vartheta}\,\frac{2\xi + (v+i\zeta)e^{-i\omega}\cot\dfrac{a}{2} - (v-i\zeta)e^{+i\omega}\tan\dfrac{a}{2}}{(r+\xi)\cot\dfrac{a}{2} + (r-\xi)\tan\dfrac{a}{2} - (v+i\zeta)e^{-i\omega} - (v-i\zeta)e^{i\omega}},$$

and if numerator and denominator of this fraction are multiplied by

$$\frac{\kappa'}{r+\xi}$$

we obtain by taking account of equations (11a):

$$k = \frac{\kappa' + \kappa + \kappa\kappa' e^{-i\omega}\cot\dfrac{a}{2} + e^{i\omega}\tan\dfrac{a}{2}}{\kappa'\cot\dfrac{a}{2} - \kappa\tan\dfrac{a}{2} - \kappa\kappa e^{-i\omega} + e^{i\omega}}\,e^{-i\vartheta}$$

or

$$k=e^{-i\vartheta}\cot\left(\frac{a}{2}\right)\frac{\left(\kappa+e^{i\omega}\tan\frac{a}{2}\right)\left(\kappa'+e^{i\omega}\tan\frac{a}{2}\right)}{\left(e^{i\omega}-\kappa\tan\frac{a}{2}\right)\left(e^{i\omega}+\kappa'\cot\frac{a}{2}\right)}$$

And since the factor $\left(\kappa'\cot\frac{a}{2}+e^{i\omega}\right)$ is common to numerator and denominator, we may write:

$$k=e^{-i(\vartheta+\omega)}\frac{\kappa+e^{i\omega}\tan\frac{a}{2}}{1-\kappa e^{-i\omega}\tan\frac{a}{2}} \quad \ldots\ldots \quad (11b)$$

Thus every rotation of the sphere means simply a linear transformation of the variable κ. However, every linear transformation does not mean merely a change of position of the sphere. For if we write this transformation in the general form

$$k=a\frac{\kappa+b}{1-\kappa c},$$

then we shall have:

$$\begin{array}{lll} k=0 & \text{for} & \kappa=-b \\ k=\infty & \text{for} & \kappa=\frac{1}{c} \\ \kappa=0 & \text{for} & k=ab \\ \kappa=\infty & \text{for} & k=-\frac{a}{c}. \end{array}$$

But 0 and ∞ are points on the sphere that are diametrically opposite; consequently,

$$-b \text{ and } \frac{1}{c}$$

$$ab \text{ and } -\frac{a}{c}$$

must be pairs of diametrically opposite points also. According to equations (11a), this means that b and c are conjugate complex magnitudes, and likewise ab and $\frac{c}{a}$. If the former is the case, it follows from the latter that the modulus of a must be unity. Accordingly, the

general form of such a transformation corresponding to a change of position of the sphere is:

$$k = e^{i\eta}\frac{\kappa + a + bi}{1 - \kappa(a - bi)} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (11c)$$

It is evident that equation (11b) is comprised in this formula. Taken in conjunction with the hypothesis that

$$\xi^2 + v^2 + \zeta^2 = r^2 = x^2 + y^2 + z^2$$

this single equation takes the place of the complicated system of equations (1b).

In order to find the axis of rotation, it should be noted that the points on the axis do not change their positions, and hence for them we must have $\kappa = k$. Imposing this condition in equation (11c), we get a quadratic in κ, whose two roots are the diametrically opposite extremities κ and κ' of the axis of rotation. The equation is

$$0 = \kappa^2 + \frac{e^{i\eta} - 1}{a - bi}\kappa + \frac{a + bi}{a - bi}e^{i\eta} .$$

Hence,

$$\kappa + \kappa' = \frac{1 - e^{i\eta}}{a - bi}, \quad \kappa\kappa' = \frac{a + bi}{a - bi}e^{i\eta} .$$

Since κ and κ' are of the form

$$\kappa = e^{it}\tan\frac{\beta}{2}$$

$$\kappa' = -e^{it}\cot\frac{\beta}{2},$$

we have:

$$\kappa + \kappa' = 2e^{it}\cot\beta ; \qquad \kappa\kappa' = -e^{2it} .$$

If we put

$$a + bi = re^{i\vartheta} ,$$

then

$$e^{it} = \sqrt{-\kappa\kappa'} = e^{i(\vartheta + \frac{1}{2}\eta)}$$

$$\cot\beta = \frac{\kappa + \kappa'}{2\sqrt{-\kappa\kappa'}} = \pm\frac{\sin\left(\frac{1}{2}\eta\right)}{r} ;$$

whereby the position of the axis of rotation is determined.

When $\eta=0$, then also $\kappa+\kappa'=\cot\beta=0$; therefore, in this case the axis of rotation is parallel to the plane of the drawing. Thus a motion of this sort corresponds to LISTING's law, provided the perpendicular through the centre of the sphere on the plane is considered as being the line of fixation in its primary position, so that its position is denoted by the coördinate $\kappa=0$.

This method will be employed to calculate the angle of deviation η for the case when the measurements are made from an initial position that is not a primary position; which is a problem involving exceedingly tedious calculations when it has to be solved by means of equations (1b).

Let $(a+bi)$ be the ordinate of the primary position of the line of fixation. By means of a rotation in accordance with LISTING's law, this line of fixation is brought to the zero point by the following transformation:

$$k=\frac{\kappa-(a+bi)}{1+\kappa(a-bi)}.$$

Now if, also in accordance with LISTING's law, the line of fixation is directed to a new point, for which $\kappa=c+di$, that is,

$$k=\frac{(c-a)+(d-b)i}{1+(c+di)\,(a-bi)},$$

the new variable $\mathfrak{k}$ according to this transformation is

$$\mathfrak{k}=\frac{k-\dfrac{(c-a)+(d-b)i}{1+(c+di)(a-bi)}}{1+k\dfrac{(c-a)-(d-b)i}{1+(c-di)\,(a+bi)}}.$$

Substituting for k its value in terms of κ, we obtain:

$$\mathfrak{k}=\frac{\kappa-(c+di)}{1+\kappa(c+di)}\cdot\frac{1+(c-di)\,(a+bi)}{1+(c+di)\,(a-bi)}$$

which may be written as follows:

$$\mathfrak{k}=e^{i\eta}\frac{\kappa-(c+di)}{1+\kappa(c-di)},$$

if we put

$$e^{i\eta}=\frac{1+(a+bi)\,(c-di)}{1+(a-bi)\,(c+di)} \quad \ldots\ldots\ldots\ldots \quad (11d)$$

This last equation enables us to obtain the magnitude denoted by η. Resolving it into its real and imaginary parts, and putting

$$a=bi=re^{it}\ ,$$

$$c+di=\rho e^{i\tau}=\tan\frac{\alpha}{2}\cdot e^{i\tau}\ ,$$

we have:

$$\cos\eta=\frac{1+2r\,\rho\cos(t-\tau)+r^2\rho^2\cos 2(t-\tau)}{1+2r\,\rho\cos(t-\tau)+r^2\,\rho^2}$$

$$\sin\eta=\frac{2[1+r\,\rho\cos(t-\tau)]r\,\rho\sin(t-\tau)}{1+2r\,\rho\cos(t-\tau)+r^2\,\rho^2}\ .$$

Accordingly, these expressions give the rotations in experiments made by starting, not from the primary position of the eye, but from some other position. When the original deviation r is small, the expressions are made more easy to understand by expanding $\log(e^{i\eta})$ in equation (11d) in an infinite series:

$$\frac{1}{2}\eta=r\,\rho\sin(t-\tau)-\frac{1}{2}r^2\,\rho^2\sin 2\,(t-\tau)+\frac{1}{3}r^3\,\rho^3\sin 3\,(t-\tau)\ \text{etc}\ .$$

This expression has the same form as equation (9), page 95, and may be conveniently used for calculation of errors.[1]

DONDERS' *Method of Finding the Centre of Rotation of the Eye.*[2] The horizontal diameter of the cornea is measured first with the ophthalmometer. A tiny flame, which is reflected in the cornea of the eye to be measured, is placed just above the ophthalmometer; a fixation mark being also adjusted by the side of the instrument. The latter, which may be shifted horizontally, serves as the point of fixation for the eye. Incidentally, this eye should be highly illuminated from the side by a bright lamp, the light from it being screened from the ophthalmometer. Then we try to adjust the instrument so that each double image of the reflex of the flame coincides with a double image of a lateral edge of the cornea. In order for this to be the case with both images of the luminous reflex at the same time, the centre of the cornea must be exactly opposite the ophthalmometer. This is accomplished by shifting the mark of fixation back and forth until the requirement above mentioned is satisfied. Then the angle through which the plates of the ophthalmometer are turned will correspond to half the width of the cornea; and hence it may be calculated by the rule given in Vol. I, p. 12. The angle made between the axis of the ophthalmometer directed toward the eye and the line of fixation directed toward the mark gives the deviation of the line of fixation from the axis of the cornea.

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[1] See further on a construction method that is useful for the same purpose.—K.

[2] *Archiv für die holländischen Beiträge zur Natur- und Heilkunde.* Bd. III, Hft. 3, S. 260–281.

Now in order to find the arc described by the cornea in traversing the length of its own horizontal diameter, a ring was suspended in front of the eye with a fine vertical hair stretched across it. The number of degrees (reckoned from the position in which the axis of the cornea was directed toward the ophthalmometer) was measured, on both sides, through which the eye had to turn, without moving the head, so as to make the edge of the cornea coincide with the vertical hair. This would give the angle through which the eye had turned around the centre of rotation. It soon developed that for normal eyes this angle was about 56°. Accordingly, thereafter, DONDERS began every measurement by causing the eye to turn, first 28° to the left, and then 28° to the right, from the first adjustment which was used to focus the reflex on the centre of the cornea. The head was turned in such manner that, when the eye was looking in one of the lateral directions, one edge of the cornea coincided with the hair-line; and then an experiment was made to see whether, on looking in the other lateral direction, the opposite edge of the cornea coincided with this line. Usually this was not exactly the case, but the investigator could tell whether the arc turned through by the eye was greater or less in one case than in the other. Accordingly, the two lateral fixation marks were shifted equally, either farther away from the central mark or nearer to it, until finally there was exact coincidence between the two edges of the cornea and the hair-line. By making the eye turn quickly, first, toward one mark, and then toward the other several times, the effect of any possible previous movement of the head was eliminated.

Let $2a$ denote the width of the cornea as found by the ophthalmometer, and let β denote the angular distance of each lateral fixation mark from the central mark as seen by the eye to be measured; then the distance of the centre of rotation from the maximum horizontal chord of the cornea is equal to $a . \cot \beta$.

In many instances, especially with near-sighted eyes, the movement of the eye was too limited for the cornea to traverse the necessary interval. In such cases DONDERS used a ring that had two parallel wires stretched across it whose distance apart (3.02 mm) was accurately measured. The fixation marks were adjusted, so that one wire would coincide with the inner edge of the cornea and the other with the outer edge, alternately. Then all that was necessary in order to find the interval traversed was to subtract the distance of the wire from the previously obtained width of the cornea; and this value was made the basis of the subsequent calculation.

The results of these measurements have already been given above.

Testing the Law of Ocular Rotation by the Method of After-images. For emmetropic eyes with their visual axes parallel, the simplest way to perform the experiment is in front of a large wall covered with bright grey paper, with no very conspicuous pattern on it, but with prominent horizontal and vertical lines. A horizontal red band is set up on a level with the eyes, a black point being made on it to serve as centre of fixation. Looking steadily at this mark for a short time, and then looking at the paper, one will see a bright green after-image of the band, and can easily tell whether it is parallel to the horizontal lines in the pattern of the paper, or whether it has a different direction.

In order to fix the direction of the primary position of the line of fixation with reference to the head, I use a little board, with a mark of fixation on it. This planchette is taken between the teeth. A geometrical projection of it is shown in Fig. 14. The length of the board AB is 13 cm, and its width 4 cm. At A a portion of the board is cut out in the form of an arc to fit the row of teeth; and at B there is a square wooden post on which a horizontal strip of stiff paper is fastened with wax, so that it can easily be

shifted. Both sides of the board at *A* are covered with a layer of warm shellac; and when it begins to harden, the planchette is seized between the teeth so as to get an impression in the shellac. Thereafter, when the mould has hardened, the exact position of the planchette between the teeth can be regained just as it was at first, thus enabling us to resume an experiment where it had been interrupted.

Fig. 14.

The length of the paper strip is the same as the distance between the two eyes. This can easily be contrived by looking at a very distant object. Then the strip of paper will be seen double; and all we have to do is to adjust its length and orient it until the ends of the double images next each other are exactly in contact. Then the distance apart of the two tips of the strip must be equal to the distance between the centres of rotation (or, to be more accurate, the distance between the centres of the entrance-pupils[1]) of the two eyes; and the line joining them must be in a plane with the line joining the centres of rotation.

Now if we wish to begin making the observations, either with both eyes or with one eye at a time, the first thing to be done is to find experimentally the primary position of the eyes. The way this is done is to select a place opposite the red band on the other side of the room, and gaze steadily at it for a long time, looking past the corresponding end of the strip *CC'*; and then shift its after-image either vertically up and down or horizontally right and left, observing whether it keeps parallel to the horizontal lines of the wall paper or not. If it does not, the strip of paper *CC'* must be shifted until the correct position is found for it. It will have to be shifted more to the left, supposing we find, on looking upward, that the left end of the after-image is higher, and on looking downward, that it is lower. On the other hand, if the right end of the after-image is found to be higher on looking upward, and lower on looking downward, the strip must be shifted to the right. Again, it must be shifted upward, if the left end of the after-image is found to be lower on looking over to the left, and the right end lower on looking over to the right; and *vice versa*.

When the position of the marker has been found for which each eye is in the primary position, it proves, in the first place, that there is a position of the eye from which the gaze travels horizontally by turning the eye around a vertical axis, or travels vertically by turning the eye around a horizontal axis.

However, although, when the gaze is shifted straight up or straight down and directly to the right or left, the after-images of horizontal and vertical originals remain horizontal and vertical, we find that this is not the case when the gaze is shifted obliquely upward or downward. What we do find, is that:

1. When the gaze is directed *upward* to the *right* or *downward* to the *left*, the after-image of a *horizontal line* is apparently rotated to the *left* with respect to the lines on the wall; and the after-image of a *vertical line* is rotated to the right; and

[1] ¶Helmholtz does not speak of the entrance-pupil of the eye; but this is what is meant by "the centre of the lines of sight." (J.P.C.S.)

2. When the gaze is directed *upward* to the *left* or *downward* to the *right*, the after-image of a *horizontal line* is apparently rotated to the *right*, and that of a *vertical line* to the *left*. Since horizontal and vertical lines exhibit different rotations, it follows that there must be intermediate lines whose after-images will be parallel to their original directions.

The simplest way of finding them is to turn the head sideways, so that the eye has to make oblique movements with respect to the head, in order to traverse the horizontal and vertical lines on the wall. By looking past the marker at the centre of the red band, with the head held obliquely in this way, we make sure of being able to return again to the initial primary position of the eye. The directions in which the images of the two ends of the paper strip used for the marker are projected, indicate on the wall the direction of the line joining the centres of rotation of the two eyes. If Listing's law is obeyed, the after-images of horizontal lines remain parallel to the horizontal lines of the wall, even when the head is tilted sideways, provided the point of fixation is shifted along the vertical and horizontal lines passing through the centre of the red band. The same thing is true of the after-images of a vertical line with reference to the vertical lines of the wall paper.

The advantage of projecting the after-image on a comparatively remote wall is that slight fluctuations of the head one way or the other have practically no effect at all on the position of the line of fixation as determined by the contrivance represented in Fig. 14. Moreover, the eyes are kept parallel of their own accord. On the other hand, generally speaking, the walls of ordinary rooms are not large enough for us to make the test for extreme positions of the line of fixation at the requisite distance from the wall. And so this method cannot be used with myopes, because they have to wear spectacles in order to accommodate for the wall, and unless the glasses are perfectly centered and perpendicular to the visual axis, the apparent slope of the lines as seen on the wall may vary. For close observations I have altered the method above described, so as to study more accurately the effect of convergence, and to determine the size and form of the field of vision.

A large wooden board, covered over smoothly with bright grey paper and fastened on the wall, is used for the field of vision. To keep the position of the head firmly fixed in front of it, a little table fastened to the floor is placed before it at a convenient distance for the patient's accommodation. An iron stand with adjustable arms, similar to those used in chemical laboratories, is attached to the table. It supports a little planchette like that shown in Fig. 14, but without the post and paper strip. The latter is grasped firmly by the teeth, simply to make sure that the position of the head is maintained with respect to the board on the wall. The position of the head can be kept fixed by the teeth much better than by any other mode of fastening which merely supports directly the soft portions of this part of the body. Another adjustable horizontal arm on the stand is screwed tight to support the forehead against it. A coloured strip of very stiff paper or thin wood, fastened to the board on the wall by a thumb-tack so that it can turn around its centre, is mounted opposite one eye. In my experiments this strip is made either half white and half black or half green and half red, the two colours being separated by a middle line parallel to the length of the strip. This line will afford then a well-defined after-image. Moreover, a pair of fine black threads are stretched over the centre of the strip, one vertical and the other horizontal; and the planchette is adjusted in the teeth so that when the after-images of the horizontal strip are displaced along the horizontal thread, they continue parallel to it; and likewise the after-images of the strip when it is vertical remain parallel to the vertical thread. But here it should be noted that the visual axes must be kept parallel; and in order to control this, I make two marks on the wall at the places where I am looking, whose distance apart is equal to

the interval between my eyes (68 mm); one of these marks being close to the line where I am looking and the other to one side on the same level, so that, on looking at the two points with parallel visual axes, they will be fused together.

The primary position of each eye may be found in this manner. In my own case their distance apart is the same as the distance between the two eyes themselves. Afterwards the strip whose after-image is to be taken can be adjusted in any oblique positions, and threads stretched over its centre so as to shift the after-images along them. In order to make the visual axes convergent, after the after-image has been developed in one eye, we can look steadily with both eyes at a point on the board itself, or fuse any desired pair of points for which the lines of fixation are convergent or crossed.

If then, as is the case with convergent positions, the after-images do not coincide exactly with the thread which the gaze is made to traverse, the strip itself may be adjusted obliquely to the thread until the position is found for which the after-image is parallel to the given peripheral part of the thread. The angle between the strip and the thread may easily be calculated by measuring the distance at both ends between the median line of the strip and the thread above it. Or, more conveniently, a short angular protractor can be applied to the ends of the strip.

The measurements between the directions of the after-images and threads can be made accurately to within about half a degree. Of course, this is not to be compared with the accuracy of astronomical measurements; but considering the nature of the case, I think it would be illusory to try to obtain much greater accuracy. For in these observations certain small variations have already been detected that are not due simply to convergence but also to the way in which the eye has been brought into the given position, and which seem themselves to alter from day to day. I myself have noticed them not infrequently, especially with oblique positions of the eye; and Dr. BERTHOLD, who worked in my laboratory, has observed them even more distinctly and larger in amount. I suspect that they are apt to be more considerable with near-sighted eyes, because the latter, being used to near objects, doubtless, vary their rolling movements more for the same direction of the line of fixation, according to the degree of convergence.

Mr. E. HERING has tried to control the accuracy of after-image experiments; and the conclusion he reaches is that the errors made in comparing their directions with objective lines may amount to as much as 5° perhaps. When the after-images are well-developed after sharp fixation of the object, errors of this degree, I must say, are entirely out of the question. I have stated above that, when the experiments are carefully conducted, the errors do not exceed half a degree. Differences of one degree, which I could easily make intentionally with the apparatus above described, may be detected with certainty when the experiment is well performed. Mr. HERING's experiments lead me to infer rather that his eye executed corresponding variations in its adjustment, which may be due particularly to the fact that the object of fixation was 10 inches in front of him, and when an object that close is regarded for a considerable length of time, it is usual for the convergence to undergo great variations.

Of all known methods of determining the position of each eye separately and independently of the other eye, the method of after-images is the most reliable, provided one has learned to be expert at it. Especially in the form of experiment described above, the eye does not have to continue long in peripheral positions (which seems to me a matter of much importance); but each individual experiment is quickly performed.

In WUNDT's method[1] also after-images were used for determining the positions of the eyes. In this case the after-images were projected on an adjustable disc. The latter was fastened to a movable lever and was always perpendicular to the line of fixation. The apparatus had scales for reading the so-called angles of longitude and latitude and the angle of torsion of the vertical meridian with respect to the vertical line.

Testing the Law of Rotation by means of the Blind Spot. This method also enables us to determine the position of each eye by itself independently of the other eye. It was used first by A. FICK.[2] On the grey wall of a large room, at the level of the eye of an observer seated in a chair, a suitable small object of fixation was placed, having the form of a white circle with a black serrated border. The position selected for the eye was about 6m away, so that the line of vision was perpendicular to the wall when the eye was directed at the object. The places on the floor were marked where the legs of the chair had to be when its front edge had certain definite inclinations with respect to the wall. In all these positions of the chair the middle of the line between the rear legs remained in the same place. FICK leaned back in the chair with his head erect, and found that in this way he could adjust the median plane of his head at right angles to the other edge of the chair as accurately as necessary. In order to determine the inclination of the head to the horizontal a wooden loop passing over his head was fastened to his ears by two set screws, and an iron rod coming down from its middle was supported on the nose. Thus the loop had a fixed position with respect to the head. The position of a plummet attached to the screw on the left ear was indicated on a graduated arc, which was rigidly connected with the wooden loop. Thus the inclination of the head to the horizon, or rather of a straight line supposed to be in the median plane, could be ascertained.

A sheet of grey cardboard, which could be turned around a peg at the point of fixation, was fastened to the wall. The observer could turn this card by the help of a cord passing over a pulley. A black spot was painted on the card, at such a distance that, with proper adjustment, it would fall on the blind spot. An assistant read the position of the head; and when a definite inclination was obtained, the observer adjusted the card by means of the cord until the black spot disappeared. The rotation of the card could be read on a tangent scale. In this way it was found how much the eye was turned with respect to its initial position. The rotation of the chair measured the angle called *longitude*, and the graduated arc on the ear measured the *latitude*. On repeating the experiment, differences in the angle of rolling of the eye were noted amounting to as much as three degrees. Doubtless, greater accuracy might be attained in this method by having some rigid connection between the pegs in the ears and the back of the chair, and by using a fairly bright white spot on a dark ground, having the same form and dimensions as those of the projection of the blind spot.

MEISSNER[3] held the head fixed, and moved the object of fixation which had the black spot on it. The head was placed so that the eye was situated at the centre of a graduated vertical semi-circle, 10 inches in radius, which could be turned around its vertical axis through an angle that had to be measured (FICK's *longitude* and MEISSNER's *latitude*). There was a slider which could be moved along the graduated arc through an angle that could be read on it (FICK's *latitude* and MEISSNER's *longitude*). On the side of this

[1] *Archiv für Ophthalmologie.* Bd. VIII, 2. pp. 16, 17.

[2] MOLESCHOTTS *Untersuchungen zur Naturlehre des Menschen.* V. 193–233.

[3] *Zeitschrift für rationelle Medizin.* Reihe 3. Bd. VIII.

slider toward the centre was the disc with the dark spot on it; and it could be turned around an axis directed toward the centre. MEISSNER's results are exhibited in the subjoined table, the angle being given which was read directly. and which corresponds to k' in equation (4e).

		Nasal Side				Temporal Side		
		+30	+20	+10	0	−10	−20	−30
Upwards	−30	− 3	0	+ 2	0	+ 3	+ 6	+10
	−15	+ 0. 5	+ 1. 5	+ 2. 5	0	+ 1. 5	+ 3	+ 5
	0	+ 7	+ 5	+ 4	0	0	0	0
Downwards	+15	+12. 5	+ 8. 5	+ 5	0	− 1. 5	− 2. 5	− 5
	+30	+19	+13	+ 7	0	− 3	− 6	− 9. 5
	+35	+20. 5	+14	+ 7. 5	0	− 3	− 7	−10
	+40	+20. 5	+14	+ 7	0	− 3	− 7. 5	−11
	+45	+21	+14. 5	+ 7	0	− 3	− 8	−12
	+50	+21. 5	+14. 5	+ 7	0	− 3	− 8. 5	−13

The fairly irregular procedure of the values would seem to indicate that changes of convergence, which are hard to avoid in the monocular fixation of a very close object, were not without influence. MEISSNER himself considered his experiments as being approximately in accordance with LISTING's law; but supposed that for inward adjustments of the eye a different primary position was to be taken, which is directed downward 45° below the horizon; whereas for the outer positions the primary position was in the horizontal plane itself. In order to exhibit this relation, he made another set of calculations from his experiments.

The averages of FICK's results are shown in the following table.

Longitude	*Latitude*										
	−33	−30	−28	−14	−11	−6	0	+1	+4	+18	+45
−29			−4.7°								
−26					+3.5°						
−21									+1.5		
−14		−2°								+5.7°	
−13				+2.5°							
−10								+2°			
0	+2.5						0				+0.1°
+10								+0.1°			
+13				+1.7							
+14		−4.7°								−1.8°	
+21									−0.3°		
+26					+3.4°						
+29			+7.5								
+38						+2.9°				−3.3°	

Testing the Positions of the Eyes by Comparing Corresponding Images in the Two Eyes. It would seem that the methods belonging under this head would admit of much higher accuracy than the method of after-images; but they can only be used for comparing the positions of the two eyes with each other, and not for finding the position of one eye by itself. Accordingly, they are very useful for detecting little individual departures from LISTING's law. Moreover, in certain cases, especially in the theory of binocular vision, the essential thing consists in ascertaining the differences of adjustment of the two eyes.

The first person to employ these methods was MEISSNER.[1] He called attention to the fact that when a person looks at a wire directly in front of him, and perpendicular to the plane of fixation, in such manner that the eyes are made to converge on a point either just in front of the wire or just beyond it, generally the wire will not be seen in parallel double images, but the latter will appear to have a certain inclination to each other; and that the wire itself must be inclined to the visual plane in order to see it in parallel images. From the position of the wire with reference to this plane it was easy to determine the positions of the corresponding vertical meridians of the two eyes; and thence the amount of torsion of the eye could be deduced, for the median positions of the point of convergence at least. In MEISSNER's investigations, carried out by this very ingenious method of his, he found that LISTING's law was practically obeyed; although, owing to certain sources of error discovered in subsequent researches, it was necessary to make some corrections in his results. Thus, in the first place, he was not aware then of the difference between the apparently vertical meridian of the eye and the real vertical meridian; and he supposed, according to his original assumption, that infinitely distant vertical lines had to be reproduced in identical meridians of the two eyes, which is not the case with most eyes. In the second place, he did not know about the effect of convergence on the torsional rotations of each eye separately, as VOLKMANN had discovered. Perhaps too the estimate of the parallelism of the double images may be influenced by the fact that one end of the wire is nearer the eye, sometimes more and sometimes less; and the observer, knowing this and perceiving it, gets the idea of the parallelism of the double images as being two material lines inclined to each other, instead of the idea that they are parallel in the field of vision, which is what it amounts to.

Consequently, VOLKMANN's modification[2] of MEISSNER's method would seem to be an improvement. VOLKMANN mounted two discs on a vertical wall in front of the eyes. The centre of rotation of each disc was on the line of fixation of the corresponding eye, when the latter was directed for an infinite distance. Through the centre of each disc a fine line was drawn, which turned with the disc. The change of position was measured on a graduated arc surrounding the disc. The observer looks at the lines on the two discs, using as little convergence as possible, seeing them, therefore, in double images not far apart; and by turning one of the discs, he tries to make the double images parallel.

By repeated trials very accurate average values can be obtained. Although VOLKMANN himself did not use this method for drawing conclusions as to the movements for different positions of the head, it can be employed for that purpose, by looking at the discs with the head at various angles.

I have found that VOLKMANN's apparatus can be conveniently simplified for this purpose. In experimenting on parallel positions with my own eyes, I hung two threads, with little weights tied to them, on a vertical wooden board. One thread was white in front of a black ground, and the other black in front of a white ground. The distance between the pegs where the threads were hung was 68 mm, corresponding to the interpupillary distance of my eyes. Down below, the threads were deflected by two pins stuck in the wood, and thus made to converge slightly. A horizontal line exactly on the level of my eyes was drawn behind the thread which was to be focused. In viewing the threads the visual axes of the two eyes were kept parallel, so that they

[1] *Beiträge zur Physiologie des Sehorgans*, 1851.

[2] *Physiologische Untersuchungen im Gebiete der Optik*. Leipzig. 1864. Heft 2. S. 199-240.

appeared to be at the same place in the common optical field (*Sehfeld*); and then one of the pins was shifted until the threads no longer appeared to cross each other; and so that when the eyes were converged a little, the two images, instead of being divergent, appeared to be parallel. By having the two threads of different colour it is easier to tell whether they are congruent in the visual field than it is when they are of the same colour; because in the latter case they may be readily fused stereoscopically, even when they are not quite coincident all over. Viewed as near double images, their centres appear to be separate and their ends to be united. Care must be taken to make the union take place the same way above and below.[1]

By tilting my head backwards or forwards, I was able to perform this experiment with the visual axes of the two eyes parallel, and either raised or lowered; and in fact slight deviations were obtained from the perfect parallelism of their positions as required by LISTING's law in this case, the angle between the apparently vertical meridians, when the lines of fixation were parallel and elevated to the upper edge of the field, being 0.3° more than when the lines of fixation were parallel and in the lowest position. In the former case the upper end of the vertical meridian of each eye was found to be turned 0.15° more outwards than it was in the second position. In subsequent repetitions of this experiment it was found to be a better way still, to expose one eye to an object consisting of a rectangular red strip 3 mm wide, and the other eye to a blue thread, both against a black ground. The thread must be seen in the middle of the red strip.

VOLKMANN himself modified this method of testing the position of the eyes. Instead of using the rotating discs with diameters marked on them, he drew only a radius on each of them. The problem consisted in looking at them with both eyes and trying to adjust them until the two radii were apparently in the same straight line. A suitable contrivance was employed to keep the head fixed during the experiment. The discs were viewed through two dark tubes, which could be pointed in any direction; so that each eye looked through one of the tubes at one disc, the latter being always at right angles to the line of fixation of the eye.

Experiments made with the visual axes parallel showed that in VOLKMANN's case the deviations from the congruence required by LISTING's law were very slight. When the eyes were directed straight up or down or straight to the right or left, from a position which VOLKMANN found for the primary position by means of experiments with after-images, there were no deviations at all. But oblique directions of the line of fixation, either up or down, did give small discrepancies. The following are the average numerical results of 60 observations, in half of which the movable radius corresponded to the right eye, and in the other half to the left eye. The numbers are the angles between the radii which appeared to make a vertical straight line.

Primary position	2.21°
30° upwards on the right	2.74°
30° upwards on the left	2.92°
30° downwards on the left	1.31°
30° downwards on the right	1.41°

The greatest deviation from the angle of the primary position was 0.9°; and by distributing this error equally between the two eyes, it would amount to 0.45° for each eye, a discrepancy which would certainly not be revealed by experiments with after-images.

[1] As to the technique, see Note 11 at the end of this chapter.— K.

Moreover, by the same method, VOLKMANN found that in converging for a point in the horizontal plane 30 cm away, the angle of the apparently vertical meridian was increased from 2.15° to 4.16°; that is, each eye was made to turn through about a degree, which would not have been the case if the visual axis of one eye had had this same direction, and that of the other eye had been parallel to it.

With my own eyes the deviation for convergence is very slight, but it occurs in the same direction as VOLKMANN found. The experiment was made with a fine black thread drawn through the eye of a needle. The latter was fastened in the flat field of a white door on a level with my eyes; and the two ends of the thread were carried over two other needles on the same level, weights being hung to them so as to stretch the thread. And so the thread formed two straight lines meeting in the eye of the middle needle and making an angle with each other that could be altered. Thus by raising or lowering the side needles a little, this angle could be made to open up or down. Meanwhile the two sides of the angle remained always in a plane parallel to the surface of the door. When I wished to experiment with parallel visual axes, I held a vertical strip of stiff paper 68 mm wide in front of the middle needle. When the lines of fixation were parallel, the lateral portions of the thread that were still visible seemed to meet in the middle and make an angle. I varied the positions of the needles until this angle seemed to me to be a straight angle, that is, the two sides were in the same straight line. Then I gazed at the eye of the needle from a distance of 20 cm, at the same time inserting a sheet of paper between the nose and the needle, so that I could see only the corresponding half of the thread with each eye. Even when the fixation occurred in the primary position of the plane of sight, the thread did not appear to be an unbroken straight line, but I had to lower one half a little, so as to make it look straight once more. The rotation of each of my eyes for a convergence of 20 cm amounted to 17 minutes, whereas with VOLKMANN it was 1.37°.

In VOLKMANN's case this rotation was sufficient to be detected in the after-image of a coloured vertical line, which he fixated with one eye and with parallel lines of fixation; provided the after-image was subsequently projected with converging lines of fixation close alongside of the line. Prof. WELCKER also got the same effect as VOLKMANN. Incidentally, J. B. SCHUURMAN[1] had tried perfectly similar experiments, with negative results; whereas Prof. DONDERS, with more convergence, obtained rotations of between one and three degrees, in the same direction as VOLKMANN and I found. Much more distinct deflections produced by convergence were noticed by me, as I have stated above, in investigating after-images in peripheral positions of the line of fixation.

Determination of the Points of Insertion and Axes of Rotation of the Ocular Muscles. The action of these muscles is easily deduced from their positions and insertions. As their tendons all pass over the eyeball for some distance and adjust themselves to its curvature like belts running over a pulley, all these muscles exert a tangential pull on the eyeball. To determine the direction of this pull more exactly, a tangent to the eyeball must be passed through the point where the tendon is attached. In the case of the superior oblique muscle this tangent must be drawn to its pulley, but in the case of the other muscles it goes to their bony origin.

In its natural fastening all the rotations of the eyeball are executed around

[1] Vergelijkend Onderzoek. der Beweging van het Oog. *Academisch Proefschrift.* Utrecht 1863.

the centre of rotation, and accordingly we have to consider the action of the muscles only in so far as they produce rotations of this sort. If a body, which, like the eye, is free to rotate around a point, is acted on excentrically by a force, the direction of the corresponding rotation can be found by passing a plane through the direction of the pull and the centre of rotation, and drawing a line through the latter point perpendicular to this plane. This perpendicular is the axis of the rotation in question. As has been explained, the direction of the pull is determined by the point where the tendon is attached to the muscle and the point where the muscle (or its pulley) is inserted in the bone. Hence these two points, together with the centre of rotation of the eye, will determine in each case the position of the plane normal to the axis of rotation. Accordingly, when the positions of those three points are geometrically defined, the position of the axis of rotation can be found.

Geometrical determinations of this sort were made by RUETE[1] and A. FICK. RUETE first removed the top of the skull, by sawing through it not far above the orbit, and then adjusted the head in the ordinary upright position it has during life. Then he sawed perpendicularly midway between the eye sockets through the frontal bone (*os frontis*) and through the middle of the *crista galli*, the *sella turcica* and the bridge of the nose; making a straight wire in a direction parallel to the visual axes when they were pointed straight out horizontally. This wire was for purposes of orientation afterwards. Then the two eyes were inflated to normal tension and pointed horizontally in parallel directions. They were fixed in this position by piercing each eye along its optical axis with a fine, sharply pointed steel wire, which was gradually turned round until it was pushed back to the bony socket. In some cases also, in order to make the positions of the eyes still more secure, a covering of plaster of Paris was poured over the closed eyelids.

The sockets were then carefully opened from above, and the origins and insertions of the muscles dissected with much pains, without removing any more of the intervening fat than necessary to show up these places. The angles were measured between the muscles and the optical axis by bending wires to fit them. The distances of the origins and insertions of the muscles from the centres of the two eyes were measured by compasses, above and below, right and left, forwards and backwards. RUETE's measurements were repeated by three assistants.

However, in the latter respect, it might have been better to do as FICK did, and measure the distances of the origins and insertions of the muscles, the vertex of the cornea and the place of entrance of the optic nerve from three fixed points, and then to calculate the coordinates and position of the centre of the eyeball, as there is nothing anatomical to distinguish the latter point, and since direct measurements with compasses of the vertical or horizontal distance of two points that are not exactly vertical or horizontal with respect to each other are necessarily not very accurate. The averages of measurements of four heads as made by RUETE are given in millimetres in the subjoined table. The distances denoted by x are reckoned horizontally from the centre of the eye toward the temporal side; those denoted by y are reckoned horizontally toward the back of the eye; and those denoted by z are reckoned vertically upward.

[1] RUETE, *Ein neues Ophthalmotrop.* Leipzig 1857.

	Insertions			Origins		
	x	y	z	x	y	z
Rectus superior	+ 2.00	−5.667	+10	−10.67	+32	+ 4
" inferior	+ 2.20	−5.767	−10	−10.8	+32	− 4
" externus	+10.80	−5.00	0	− 5.4	+32	0
" internus	− 9.90	−6.00	0	−14.67	+32	0
Tendo obliqui superioris	+ 2.00	+3.00	+11	−14.1	−10	+12
Obliquus inferior	+ 8.00	+6.00	0	− 8.1	− 6	−15

Diameter of the Eye = 24 mm.

Fick's results were as follows:

	Insertions			Origins		
	x	y	z	x	y	z
Rectus superior	0	− 7.9	+9.1	−16	+31	+ 6.5
" inferior	0	− 7.9	−9.1	−17	+30	+ 2
" externus	+ 9.1	− 7.9	0	−15	+31	+ 2
" internus	− 9.1	− 7.9	0	−18	+30	+ 4
Obliquus superior	+ 4.6	+ 2.7	+9.9	−19.6	−10.9	+12.8
" inferior	+10.4	+ 6.0	0	−18	+30(?)	+ 6
Entrance of Optic Nerve	− 3.4	+11.5	0			
Vertex of Cornea	0	−12	0			

There must be a mistake about the values of y and z for the origin of the inferior oblique muscle, as Ruete noticed; these values being invariably negative.

Ruete has calculated the positions of the axes of rotation from his measurements of coördinates; and the values of the angles a, b and c made with the positive directions of x, y and z by the negative half of the axis of rotation (according to our method of reckoning) are found to be as follows:

	a	b	c
R. internus	90°	90°	180°
R. externus	90°	90°	0°
R. superior	161.5°	109.5°	90°
R. inferior	19°	71°	90°
Obl. superior	51°	141°	84.5°
Obl. inferior	127°	37°	90°

It was explained above how the rotations are combined about different pairs of axes. As it is difficult to visualize these relations, Ruete[1] made an adjustable model of the two eyes called an *ophthalmotrope*, in which the muscles are represented by corresponding cords stretched by springs, the displacements being read on a scale. As a rule the simplified form of the instrument as devised by Knapp,[2] and illustrated in Fig. 15. is sufficient for explaining the process. The two artificial eyeballs can turn around their

[1] *Ein neues Ophthalmotrop*, Leipzig 1857.—*Das Ophthalmotrop, dessen Bau und Gebrauch.* Göttingen 1845, from the first volume of the *Göttinger Studien.*

[2] As to his subsequent measurements, see Note 12 at the end of this chapter.—K.

centres on a ball pivot. On them are shown the equator, the cornea, and the vertical and horizontal meridians. Strong silk cords of various colours are fastened at those places where the muscles are attached. In order for the cords to maintain the directions of the muscles, four of them corresponding to the four recti are passed through four adjacent holes in the board *A* and made to hang vertically behind it by means of weights tied to them. Two of the cords, however, corresponding to the pair of oblique muscles in each eye, are passed around the little pulleys at the upper and lower ends of the vertical brass piece *B*, and then carried to the middle of the board *A*, where they also go through holes and are stretched by weights. The muscles in the two eyes with the same name are represented by cords of the same colour. To produce any desired rotation of one eye or of both of them, the cords are pulled, corresponding to the muscles which are stretched in the given movement of the eye, and which would therefore tend to resist the motion. On the other hand, those cords whose corresponding muscles in the eye tend to become shorter in the execution of the movement, will be relaxed, and so their weights will descend; and hence they are in the position to produce or reinforce motion. Thus by being careful about what weights descend, and how far they go, it is possible to tell immediately what muscles have to come into play, and how much they have to act, in order to produce the given effect. The apparatus is very convenient for demonstrations and for getting a quick survey of the connections between the pathological deviations, which are often very complicated.

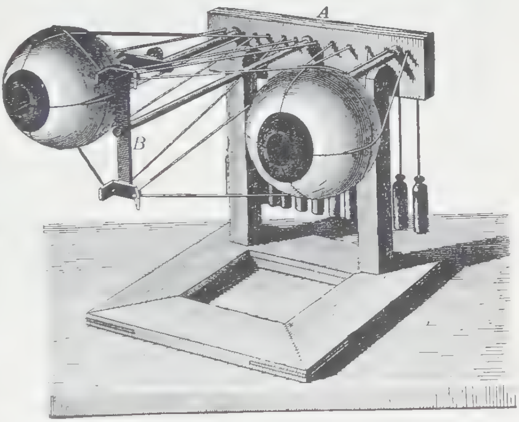

Fig. 15.

WUNDT[1] constructed another *ophthalmotrope* in which the cords were connected with spiral springs whose tension and length had been made as nearly as possible proportional to those of the ocular muscles. In this model the eyeball of itself takes the direction corresponding to WUNDT's experiments on the positions of the eyes, provided the axis corresponding to the line of fixation is adjusted in the desired position. WUNDT used this model especially for explaining his principle of minimum exertion, from which he derived the law of ocular movements.

The earliest researches concerning movement of the eyes had to do with the position of the centre of rotation.[2] JOH. MÜLLER was still of the opinion

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[1] *Archiv für Ophthalmologie*. VIII, 2. S. 88.

[2] ¶ It should be stated that both KEPLER and SCHEINER attached much importance to the mobility of the eye and to the necessity of taking it into account not only in the case of free unaided vision but in the design of optical instruments to be used in conjunction with

that the centre of rotation of the eye was necessarily at the centre of its posterior surface,[1] and this view was shared by Tourtual[2] and Szokalski.[3] Volkmann[4] tried to determine the position of the point of intersection of the lines of direction by means of his apparatus for measuring the visual angle, and to find the centre of rotation, as has been explained in Volume I, p. 117. He believed that the two points were coincident. The point which he found may indeed have been really the centre of rotation, which according to him should be 5.6''' [12.63 mm] beyond the cornea. The ensuing controversy with Mile, Knochenhauer, Stamm and Burow has also been mentioned previously. Burow made more accurate measurements of the centre of rotation.[5] The average of 40 determinations gave 5.42''' [12.23 mm] for the distance of this point from the vertex of the cornea, the maximum difference being 0.8''' [1.8 mm]. The measurements were repeated by Valentin[6] for both horizontal and vertical movements, the average in the former case being 5.501''' [12.41 mm] and in the latter case 5.08''' [11.46 mm]. The researches of Junge (published in Russian) and of Donders and D. Doijer,[7] mentioned above, appeared very much later.

Joh. Müller also started the investigations of the rolling movement of the eye.[8] He states that, by means of various points in the white of the eye which he had marked with ink, he was able to perceive that the eye did not turn about its longitudinal axis during its movements. This was the prevailing belief among physiologists until numerous investigations were started by a work of Hueck.[9] Hueck tried to defend a view which had been expressed by Hunter, namely, that when the head was tilted toward the shoulder, an opposite rotation of the eye took place around the visual axis. He ascribed this rotation to the oblique muscles. He supposed he had proved the correctness of this thesis, because he had observed the displacements of the conjunctival vessels during movements of the head, not on himself only, but on others.

Hueck's propositions were assumed by most physiologists to be correct. Although Tourtual[10] rightly observed that axial rotation was absolutely not

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the eye. Kepler had called attention to this subject as early as 1604; and in 1611 he pointed out the importance of the so-called *centrum visus* or *centrum oculi*. The theory of direct and indirect vision was developed by Scheiner, but it was completely overlooked and forgotten. We do not hear again of the centre of rotation of the eye (as it was afterwards called by Volkmann) until 1826 in the work of J. Müller; although it is just possible that Wollaston in 1804 may have had some conception of this point (see his paper "On an improvement in the form of spectacle glasses" in *Phil. Mag.*, XVII, 327-329; and subsequent papers, *Phil. Mag*, XVIII, 165, 166; *Phil. Mag.* 1813, 387-388). Recently, H. Boegehold has called special attention to L. J. Schleiermacher's work on *Analytische Optik* (published in 1842), in which this writer shows very clearly the fundamental importance of the centre of rotation of the eye.—See M. v. Rohr, Das Auftreten des Augendrehpunkts in der Physiologie und in der technischen Optik. *Zft. f. Instrkde.*, 1915. XXXV. 197-215.—Idem, *Die Brille als optisches Instrument*, Berlin, 1921, p. 86.—H. Boegehold, L. J. Schleiermacher und die Augenbewegung. *Zft. f. ophthalm. Optik*, 1920, VIII. 1-10. (J.P.C.S.)

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1 *Zur vergleichenden Physiologie des Gesichtssinns.* Leipzig 1826. S. 254.

2 Müllers *Archiv* 1840. S. XXIX.

3 *C. R.* 1843.

4 *Neue Beiträge zur Physiologie des Gesichtssinns.* 1836. S. 33.

5 *Beiträge zur Physiologie und Physik des menschlichen Auges.* 1842.

6 *Lehrbuch der Physiologie des Menschen.* Bd. II. 1844.

7 *Archiv für die Holländischen Beiträge zur Natur- und Heilkunde.* 1863. III, S. 560.

8 *Zur vergleichenden Physiologie des Gesichtssinns.* 1826. S. 254.

9 *Beiträge zur Physiologie des Auges*, S. 8.

10 *Repertorium.* 1842. S. 407.—*Lehrbuch der Physiologie.* II, S. 332.

necessary for the functions of vision, and although RITTERICH and RUETE contradicted the fact, still HUECK's opinion was supported by TOURTUAL, BUROW,[1] VALENTIN,[2] KRAUSE,[3] and VOLKMANN.[4] By investigating the position of the blind spot, TOURTUAL satisfied himself that the apparent rotation of the eye in the head was certainly not enough to account for the complete lack of variation in the orientation of the meridian of the eye. By means of after-images, RUETE[5] showed that mere tilting of the head, without moving the eye with respect to the head, does not generally produce any rotation of the eye. These ideas of RUETE were utilized by DONDERS[6] in making a more thorough test of the matter. In the first place he showed that what had misled HUECK in his observations was his not taking enough pains to keep the position of the eye steady in the head, while the position of the latter was being changed, and that the rotations he noticed were due to the former circumstance and not to the latter. Moreover, he found that the after-images of objects continued parallel in purely horizontal and purely vertical movements of the eyes, but were rotated in oblique lateral elevations and depressions. He did not obtain any precise law for the amount of this obliquity.

But a law of this sort was proposed by LISTING,[7] which indeed seems to be very exactly obeyed by most emmetropic eyes. However, LISTING did not give any proof of it and did not even publish it himself. MEISSNER[8] was the first to test the law experimentally by the method of after-images. On the whole, he found that it was verified by his experiments. He tried to show that the reason for LISTING's law was that it gave the greatest horopter. This subject will be discussed later.

FICK[9] and WUNDT[10] tried to find another explanation of the law of the rolling movement of the eye. They disregarded LISTING's law. FICK determined the positions of his eye by means of the blind spot, and WUNDT did the same thing with the help of after-images. Their idea was that the eye rolls just enough to enable it to attain the desired direction of the visual axis with least effort. It is extremely likely that this is correct, but our knowledge of the conditions on which muscular efforts depend is too meagre as yet for us to make safe calculations on that basis. WUNDT also made a kind of ophthalmotrope or model of the eye, which was movable around a pivot. The ocular muscles were represented by brass springs of suitable length and strength. The rotations of the eyeball in this model for the various positions of the visual axis agreed fairly well with WUNDT's observations on his own eyes.

However, in view of the fact that the strength of the muscles themselves is adapted, during the life of the individual, to the demands made on it, it did not seem to me that this principle, even if it should prove to be practically correct, could be the final peculiar basis of the law. Using the method of after-images, I tested LISTING's law on my own eyes and on those of some

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1 *Handbuch der Anatomie.* 1843. S. 550.

2 Article: Sehen in WAGNERS *Handwörterbuch*, S. 273.

3 *Lehrbuch der Ophthalmologie.* S. 14.—*Das Ophthalmotrop.* 1846. S. 9.

4 *Nederlandsch. Lancet.* August 1846.—*Holländische Beiträge zu den anat. und physiol. Wissenschaften.* 1848. I, S. 105–145; 384–386.

5 RUETE, *Lehrbuch der Ophthalmologie.—Ein neues Ophthalmotrop.* 1857.

6 *Beiträge zur Physiologie des Sehorgans.* 1851.—*Archiv für Ophthalmologie.* II. 1855.

7 MOLESCHOTT, *Untersuchungen.* Bd. V. S. 193.—*Zeitschrift für rationelle Medizin.* 1854. IV, S. 801.

8 GRAEFES *Archiv für Ophthalmologie,* VIII. 1862. S. 1-114.

9 *Die Achsendrehung des Auges.* 1838.

10 MÜLLERS *Archiv* 1840. S. LV and LIX; 1846. S. 346.

other observers, whose vision was normal, and found that it was very accurately obeyed. The same result was obtained in the case of my own eye by testing it with double images. I took particular pains to modify the method, so as to be surer about maintaining the position of the head, and also so as to avoid fatiguing the muscles by making angular measurements in lateral positions of the eye; and I sought to find the basis of the law in the principle of easiest orientation as given in this chapter.[1] I have endeavoured above to answer E. HERING'S objections[2] as to the methods of observation and the basis of the law. VOLKMANN'S data above given are taken mainly from unpublished correspondence.

1826. JOH. MÜLLER, *Zur vergleichenden Physiologie des Gesichtssinns.* Leipzig. S. 254.

1836. VOLKMANN, *Neue Beiträge zur Physiologie des Gesichtsinns.* S. 33.

1838. HUECK, *Die Achsendrehung des Auges.* Dorpat.

1840. TOURTUAL, MÜLLERS *Archiv für Anatomie und Physiologie*, 1840, in the Jahresbericht. S. XXIX; LV; LIX.

1842. BUROW, *Beiträge zur Physiologie und Physik des menschlichen Auges.* Berlin.

1842. VALENTIN, *Repertorium.* 1842. S. 407.

— C. F. KRAUSE, *Handbuch der menschlichen Anatomie.* S. 550.

1843. SZOKALSKI in *C. R.* 1843.

1844. VALENTIN, *Lehrbuch der Physiologie des Menschen.* II, 332.

1846. TOURTUAL in MÜLLERS *Archiv für Anat. und Physiol.* 1846. S. 346.

— RUETE, *Lehrbuch der Ophthalmologie,* S 14—*Das Ophthalmotrop*, S. 9. Göttingen.

— F. C. DONDERS in *Nederlandsch Lancet.* August 1846.

— VOLKMANN, Article, "Sehen" in WAGNERS *Handwörterbuch der Physiologie.* II, 337-358; 281-290

1847. F. C. DONDERS, Beitrag zur Lehre von den Bewegungen des menschlichen Auges, in *Holländischen Beiträgen zu den anat. und physiol. Wissenschaften.* I, 104-145; 384-386.

1854. G. MEISSNER *Beiträge zur Physiologie des Sehorgans.* Leipzig.

— CZERMAK, Über Abhängigkeit der Akkommodation und Konvergenz. *Wiener Ber.* XII, 337-358; XV, 438-454.

— A. FICK, Die Bewegungen des menschlichen Augapfels, in *Zeitschrift für rationelle Medizin.* IV, 801.

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[1] *Archiv für Ophthalmologie.* IX, S. 153-214.

[2] *Beiträge zur Physiologie.* Leipzig. 1864. S. 248-286.

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1864. Giraud Teulon in *C. R.* LVIII, S. 361 (about centre of rotation).—Also Meissners Jahresberichte über die Fortschritte der Physiologie in *Zeitschrift für rationelle Medizin* from 1856 on.

Supplement to §27 on The Ocular Movements*

In regard to the theory of the ocular movements, there is one matter which I should like to mention, by way of supplement to the above, as it is, perhaps, not altogether unimportant. The mode of attachment of the eye to the conjunctiva and even in the connective tissue and fatty part of the socket is such that relatively the least tension is produced in these places by any movement of the eye that is in accordance with Listing's law. If the eye were to execute a rather large rolling movement which departed from this law, it would certainly result in tearing some parts of the conjunctiva and partial folding of individual pieces. Thus also from this point of view there would seem to be some connection between obeying Listing's law and having the least exertion and inconvenience, in analogy with the conclusions which Fick and Wundt reached as to the muscles of the eye.

Note (in continuation of text on page 107).— A convenient method of both illustrating and measuring the magnitudes of the torsional rotations of the eye by means of a simple line-drawing is afforded also by the stereographic projection of a sphere on a plane, supposing we wish to avoid long calculations.

Let the points in the field of fixation be plotted stereographically on a plane. If we use the angles called latitude and longitude (by Fick, Meissner and Wundt) for making the measurements, a system of lines can be employed in the projection of the field of view like the parallels and meridians on a map of the eastern or western hemisphere. Fick's longitude and latitude will be measured by the meridians and

* ¶Helmholtz's supplement to this chapter was placed originally at the end of the *Handbuch der physiologischen Optik;* and there it remained also in the third edition (Vol. III, pp. 454-457). Apparently, it was omitted in the second edition. The editor of the English translation has ventured to insert this supplement here at the end of §27, where it really belongs. In making this change, however, it seemed best not to alter the number of Fig. 76. (J.P.C.S.)

parallels, respectively; the longitude corresponding to l, and the latitude to m, in equations (4e) and (4f), pages 84, 85. On a map of this sort the meridians will be arcs of circles all going through the two poles and crossing the rectilinear equator at points whose distances from the centre are equal to $R \tan l/2$, where R denotes the radius of the circular outline of the map. The angular distances from the equator of the points where the parallels meet this circumference are equal to m, so that the parallels cross the vertical diameter at distances from the equator equal to $R \tan m/2$. These circles can all be constructed by means of these data.

When the *angle of elevation* (λ) and the *angle of azimuth* (μ) are employed, the equator should be drawn vertical with the poles to the right and left. In this case, also, the angles λ and μ will be measured by the meridians and parallels, respectively.

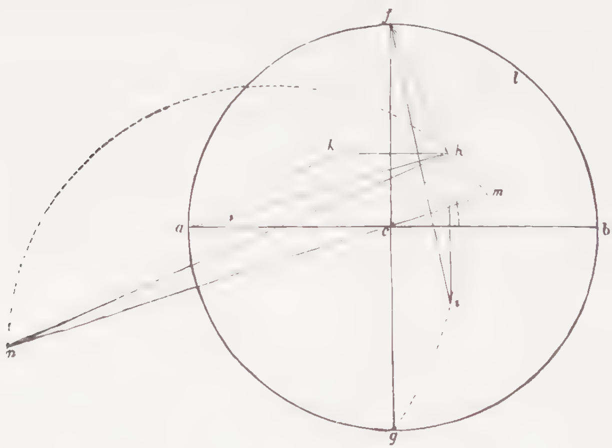

Fig. 76.

Volkmann determined the positions of the points by meridians of the field of fixation and their angular distances from the pole of this field. If that method is adopted, systems of lines must be drawn like those on maps of the northern and southern hemispheres. Then the meridians of the field of fixation will be straight lines passing through the centre of the circle and making the same angles with each other as the meridians themselves. The arc α, measured from the pole, will be represented on these lines by the length $R \tan \alpha/2$.

The stereographic projection of the contour of the hemispherical field is represented in Fig. 76 by the circle *afbg*. The centre, designated by c, is the origin of the angular measurements. The point for which

the torsional rotation of the eye is to be found is designated by h. There are two cases to be considered.

1. *Case when the centre* c *corresponds to the primary position of the eye.*

Construction.—Drop a perpendicular from h on the horizontal line ab, and prolong this perpendicular to a point i which is on the other side of ab and just as far from it as h. Draw hf and if. Then the angle hfi will be equal to the angle through which the vertical meridian of the eye has turned around the vertical line. This is the angle denoted by k' in equations (4e) and (4f).

Proof.—If the gaze travels from the primary position c to the secondary position h along the straight line ch (which represents a meridian of the field of fixation), then according to LISTING's law ch will be shifted along itself. Now since equal angles on the globe are always reproduced by equal angles on the flat chart, the element of a vertical line in the visual field that passes through the point of fixation c must continue to make the same angle with the line cl when the eye is turned to look at h as it did before; that is, it must still be vertical. But an absolutely vertical plane passing through the eye and the point h will intersect the field of fixation in the great circle represented by the arc $fhig$. The angle between the vertical meridian of the retina and the vertical plane containing the line of fixation is equal to the angle between the tangent to the circle $fhig$ at the point h and the vertical line hi, which is a chord of this circle. This latter angle, therefore, is equal to the angle hfi inscribed in this circle on the arc hi; for if the vertex of this angle is taken infinitely close to h, the inscribed angle becomes ultimately the angle between the tangent and the chord.

The angle hfi may be constructed without drawing the circle $fhig$; and that is why I have used it in the method given above. The sense of the rotation is shown by noting that the rotation of the vertical meridian of the retina around the absolute vertical takes place in the same way that the line fi lies with respect to fh (that is, for the case represented in the figure, the rotation is toward the right).

Construction of the position of the retinal horizon.—Drop a perpendicular from h on the vertical diameter fg, and prolong it on the other side of this line to the point k which is just as far from fg as h. Draw ha and ka. Then the angle kah will be the angle between the *retinal horizon* and the *visual plane*, when the eye is gazing at h; the retinal horizon being turned with respect to the visual plane in the same way as ak is turned with respect to ah (that is, in the above figure, toward the right). The proof is the same as before.

2. *Case when the centre* c *does not correspond to the primary position of the line of fixation.*

In this case a correction has to be made in the angles hfi and hak; which may be found by construction as follows.

Construction.—Suppose the primary position of the eye is shown by the point marked m on the projection. Draw the straight line mc and produce it beyond c to a point n such that

$$nc.mc = ac^2.$$

Then this point n will be the projection of the point that is diametrically opposite to the point m in the spherical field of fixation. Draw hn. Then the rotation of the vertical meridian of the retina with respect to the vertical will be

$$\angle hfi - 2\angle hnm$$

and the rotation of the retinal horizon with respect to the visual plane will be

$$-\angle kah - \angle hnm$$

The difference between these angles must be taken when the sides on which the point h does not lie are both turned in the same way with respect to the sides on which the point h does lie. Otherwise, we must take the sum of them.

Proof.- Since m and n are at opposite extremities of a diameter of the spherical field of fixation, both the circle and the straight line drawn through these points will represent meridians of the field of fixation that contain the primary position of the line of fixation; accordingly, when the gaze traverses these meridians, they will be shifted along themselves. Suppose that the gaze is directed first to c, and that the eye receives the after-image of a vertical line. This image, being itself vertical, will fall in the line fg. Now let the gaze wander to the point m. The after-image must again be vertical at the place where it passes through m. Finally, let the gaze pass from m to h along the great circle represented by the arc mhn: the angle between the after-image and the tangent at h must be equal to the angle between the vertical and the tangent at m. Consequently, the after-image must turn from its vertical position through an angle equal to the angle between the tangents to the circle at m and h, that is, through $2\angle hnm$ inscribed in the circle mhn on the arc mh. And so the angle between the after-image and the vertical great circle $fhig$ will be this much less than it was formerly, when the primary position was at c.

The same argument applies to the case of a horizontal after-image lying in the retinal horizon.

Notes on §27 by v. Kries

1. While it is correct as a first approximation to regard the ocular movements as being performed around a fixed point [see page 39], numerous instances have been investigated and verified in which this has been found not to be the case. The simplest examples of this kind are those in which the motion is one of pure translation without any rotation at all; as exhibited especially by forward and backward movements of the whole bulbus, which have been repeatedly investigated. According to J. J. Müller,[1] forcible widening of the lid aperture (drawing back the *levator palpebrae sup.*) may produce a displacement of the vertex of the cornea amounting to as much as 1 mm; whereas when the lid aperture is contracted, the bulbus retreats slightly (Donders[2]).

Thorough investigations of these movements have been made especially by Tuyl[3]; in which the excursions of the vertex of the cornea were registered graphically. When the lid aperture was widened, he observed forward movements amounting to 0.8 mm. He noticed also some periodic displacements synchronous with respiration, although they amounted only to two or three hundredths of a millimetre. Other investigations of the same kind were made by Peschel[4] and by Ludwig.[5]

Besides movements of pure translation, the most important of those that are not executed around a centre of rotation are screw motions, consisting of a combination of a rotation around an axis with a displacement parallel to it. The movements observed by Berlin[6] are of this nature. In his experiments a thread was stretched in front of the eye, horizontally, for example. Beyond this thread three little beads were adjusted in a row, one exactly opposite the centre of the eye and the others at equal distances from it on the right and left; so that the thread appeared to divide each of the beads in half, when the eye (supposed to be in the primary position) gazed steadfastly at the middle bead. Now when the eye was turned to the right or to the left, on the supposition that it had rotated around a vertical axis, the part of the thread at which it was gazing then should have again bisected the lateral bead. Generally, however, this was not the case.

[1] J. J. Müller, *Archiv f. Ophthalm.* XIV. 1868. S. 105.

[2] Donders, ibid. XVII. S. 99. 1871.

[3] Tuyl, *Archiv f. Ophthalm.* LII. 1901. S. 233.

[4] M. Peschel, *Zentralblatt für Augenheilkunde.* 1904. S. 11.

[5] A. Ludwig, Zur Demonstration des Hervortretens des Bulbus bei willkürlicher Erweiterung der Lidspalte. *Klinische Monatsblätter f. Augenheilkunde.* 1903. S. 389.

[6] Berlin, *Archiv f. Ophthalm.* XVII, 1871. 2. S. 193.

Similar deviations were perceived when the thread and beads were placed vertically or in some inclined direction, and corresponding movements were executed by the eye. The character of the deviations obtained is indicated in the accompanying diagram (Fig. 16).

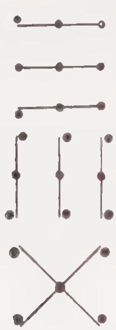

Fig. 16.

Thus, for example, the experiments show that, when the eye is turned to the right or left, the place where the line of sight crosses the horizontal thread will be raised or lowered a little. Now it is true this might be caused by *rotations*, provided they were executed around an axis that was not exactly vertical. But it is easy to see that the only way this would be possible would be for the axis to be absolutely fixed in space; which the other conditions of the motion absolutely preclude. Unquestionably, therefore, the motions here are of the general form of a screw motion, that is, they consist of a combination of a rotation around an axis together with a displacement parallel to the axis.

2. Volkmann[1] and afterwards Woinow[2] endeavoured in the next place to decide the question as to whether the movements of the eye consist principally of rotations around a fixed centre [see page 40]. They tried to ascertain whether, when the eye executed a finite movement in a certain direction, the rotation was performed continually around the same centre. They both used the same principle in their experiments, which consisted in determining the lines of sight.

Volkmann mounted a great number of pins along the circumferences of two concentric circles, each pair of corresponding pins on the two circumferences being on the same radius. Thus all the lines joining these pairs of pins met at the common centre. Then he tried whether it were possible to put the head in such a place that when the eye looked at the various pins in succession, the interior ones appeared everywhere to be exactly in front of the exterior ones; which would indicate that all the principal lines of sight coincided with the radii of the circle. It turned out that a position fulfilling this condition very approximately could always be found. And so Volkmann believed

[1] Volkmann, *Berichte der k. sächs. Ges. d. Wissensch. math.-phys. Kl.* 1869. S. 28.
[2] Woinow, *Archiv f. Ophthalm.* XVI. 1. 1870. S. 247.

he was justified in concluding that the eye did turn around a fixed point.

Under HELMHOLTZ's direction, WOINOW employed a similar method using the graduations on measuring rods; and got the same result.

Strictly speaking, these observations simply show, as was correctly pointed out by BERLIN, that all the principal lines of sight intersect in a point; but, without further proof, we have no right to assert that this point is the fixed centre of rotation of the eye. The easiest way of understanding this is by considering at first only the initial and final positions of the eye in the case of a certain finite rotation.

In Fig. 17 ab, $a'b'$ are intended to represent the positions of a line before and after undergoing a certain rotation, their point of intersection being designated by c. The line might have been brought from the first position into the second by turning it around an axis passing through c; but the same result could have been accomplished just as well by turning it around one of numerous other points. And, indeed, the centre of rotation will not be the point c, unless the point on the line that was originally at c has remained fixed there. Suppose that after the rotation the point that was originally at c proves to be at the point c' in the line $a'b'$; then the position of the axis of rotation will be found by erecting a perpendicular at the middle of cc' and locating a point D on it, such that the angle cDc' is equal to the change that has occurred in the direction of the line, that is, is equal to the angle bcb'.

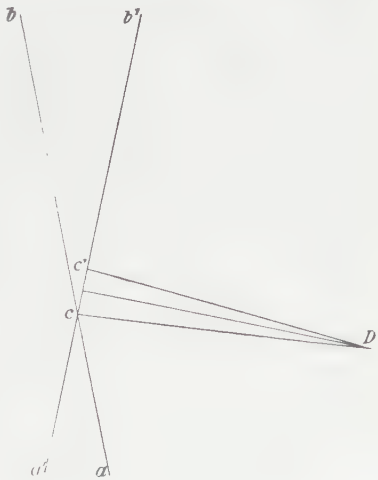

Fig. 17.

Now in a case like this, where a rotation of finite extent is supposed to be executed around a certain point, the place of intersection of the various positions of the principal line of sight would continually vary. In order for the point of intersection to stay permanently at a given place, we have to consider infinitely small rotations in the same way; and it is easy to see that the instantaneous centre of rotation must be on the line drawn through the given point perpendicular to the position

of the principal line of sight at that instant, but that it may lie anywhere on this line. Now this is all that is absolutely proved by the experiments of VOLKMANN and WOINOW.

Their inference, that the point found was the centre of rotation of the eye, can only be justified on the assumption that the centre of rotation at each instant is on the principal line of sight.

HERING[1] also pointed out that the observations of VOLKMANN and WOINOW were not conclusive evidence as to the centre of rotation of the eye; his comments being *substantially* equivalent to what has been said above. However, as HERING's argument here is based primarily on an essentially different mode of regarding the ocular movements (which has been adopted also by ZOTH[2] and others), in order to obviate any misunderstanding, it will be advisable to discuss briefly this way of treating the subject. HERING thinks that the observations of VOLKMANN and WOINOW may indicate that the eye does turn around a point fixed in space (that is, in the orbit), and that it is the same as the point found to be the place of intersection of the lines of sight; but he maintains that there is no proof that this point is likewise fixed in the eye. On first thought, it might seem queer in some ways to speak of a motion as being a rotation around a point which is fixed in space, and yet not fixed in the eye, particularly if we are accustomed to the usage and methods of mechanics. There the centre of rotation is a point, either in the eye itself or in its imaginary continuation, which remains at the same place during the motion. Thus it does not seem to be clear as to what is meant by a centre of rotation which is fixed in space and yet changes its position in the eye.

Perhaps the easiest way of understanding what is meant by rotation about a point fixed in space, but not in the eye, is by considering definite mechanical relations that approximate this state of affairs. Thus, we may imagine a plane metallic disc turning around a peg fastened in a similar disc underneath it. If there is a slot in the disc instead of a round hole, it can turn around the peg, and at the same time move to and fro, so that the peg slides in the slot, or the slot slides along the peg. The disc then will turn around a fixed point in space (the peg), although the position of the latter varies with respect to the disc itself, just because the disc slides along it. Now this is precisely the kind of thing we have to consider in the case of the experiments here under discussion. The line of sight might turn around the point, which is found to be the place of intersection of its initial and final positions, and at the same time slide to and fro along itself for any distance.

As was explained above, a motion of this sort in its purely geometrical aspect is to be regarded as simply a rotation around a new axis, which, while it is likewise perpendicular to the plane of the disc, goes through some other point on it. The rotation around a point fixed in space, but not fixed in the eye, or the combination of a rotatory motion with a sliding motion at right angles to the axis is nothing more or less than rotation around a new axis parallel to the one under consideration.

Another thing to be noted is that, in considering parallel displacements perpendicular to the axis in addition to the rotatory motion, the axis of rotation may be regarded as passing through any point whatever, and the rotation as capable of being resolved into movements of rotation and translation in the most various ways. Consequently, there is something arbitrary about this

[1] E. HERING, Der Raumsinn und die Bewegungen des Auges in HERMANNS *Handbuch der Physiologie*. Bd. III. S. 457.

[2] ZOTH, Die Gesichtswahrnehmungen in NAGELS *Handbuch der Physiologie*. III. S. 294.

process in the first place; and the only way to make the problem definite is by imposing some restriction on the movement, either as to the position of the axis or as to the direction of the translatory motion. Thus, for example (as is tacitly taken for granted in HERING's discussion), we can suppose that the translatory motion takes place in the instantaneous direction of the principal line of sight.

Consequently, it is not to the point to say that the result of VOLKMANN's experiments is to demonstrate that the rotation is performed around a point fixed in space (or in the orbit), but not fixed in the eye. The motion could always be regarded as being of this nature anyhow, even if there were *no* fixed point of intersection of the principal lines of sight. We ought rather to add, that the observations do imply a point that is fixed in space, provided that, along with the rotation, the only displacements that can occur are along the principal line of sight; so that the rotation is considered as being resolved in this perfectly definite manner.

Accordingly, either way of looking at the results of VOLKMANN's experiments leads to the same interpretation. The point that is found to be the placel of intersection of the initial and final positions of the line of sight cannot be caimed to be the centre of rotation of the eye from this fact alone, since (according to our way of expressing it) the axis does not have to go through the principal line of sight, or since (as HERING puts it) a sliding motion may be combined with the rotation around a point of the principal line of sight, causing this line to be displaced along itself. In other words, the rotation can take place around a point, which, while it may be fixed in space, is not fixed in the eye.

As remarked above, this way of describing a motion (as being a rotation around a fixed point in space which, however, is not fixed in the eye) appears somewhat strange at first, especially to any one who is accustomed to the usages of mechanics. That is why I have chosen the explanation given above, because it is in accordance with the methods of theoretical mechanics. Incidentally, it cannot be gainsaid that HERING's mode of treating the problem may have advantages in some ways. This is especially true in considering continuous rotations in the same direction, which may exhibit a unity from this way of considering them that would not be brought out practically in any other way. For instance, if (in HERING's way of expressing it) the bulbus is turned around a point fixed in space, involving at the same time any arbitrary displacements of the line of sight along its instantaneous direction, according to the other ways of looking at it, this motion would have to be described as a rotation around continually changing axes; and thus the unity, that in a certain sense is characteristic of it, would not be stressed.

The above assumption was made without any strict proof; but BERLIN[1] has tried to test it directly and verify it, by employing a process, which, incidentally, is related to WOINOW's method, and in which two excentrical objects were used besides the two that determined the principal line of sight. The former were adjusted so that one appeared to be directly behind the other; and thus two lines of sight were obtained, the effect of which was to keep the point fixed where the lines of sight crossed. For one eye, at least, it was found that this point was at constant distances from the point in which the various

[1] *Loc. cit.*

positions of the principal lines of sight intersected; and hence the latter was indeed a centre of rotation. But in case of the other eye slight variations were indeed noticed. In this case the calculated position of the centre of rotation was found to be off the lines of sight (0.541 mm, medially), as might have been anticipated from the anatomical relations (the fovea being situated a little off the optical axis).

Berlin found also that vertical rotations were accompanied by forward and backward movements of the eye, indicating that the centre of rotation was situated above or below the principal line of sight.

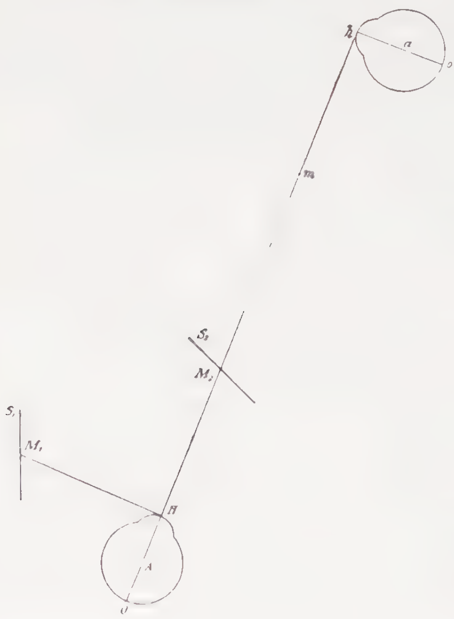

Fig. 18.

J. J. Müller[1] also carried out his experiments primarily for the purpose of deciding whether the movements of the eye were executed around a fixed centre of rotation; and with this object in view, he tried to determine directly the paths traversed by the vertex of the cornea (or, to be more accurate, by the point of intersection of the principal line of sight with the cornea), in case the eye executes rotations in which the point of fixation traverses a straight line. The method as given by Fick was as follows.

Suppose the plane of the diagram (Fig. 18) is parallel to the plane passing through the eye and the straight line containing the row of points of fixation. This plane may be supposed to be horizontal. The observer's eye is designated by A. It is situated between two mirrors that are rigidly connected, these mirrors being both perpendicular to the aforesaid plane and inclined to each other at an angle of 45°. Their traces in the plane of the diagram are shown by S_1 and S_2.

A little consideration shows that the image of the eye produced

[1] *Loc. cit.*

by successive reflections in the two mirrors will be turned through a right angle with respect to the actual eye itself; as is represented at a. Thus, the observer sees his own eye in profile. Moreover, vertical lines are marked on the two mirrors, the ends of these lines being shown in the diagram at M_1 and M_2. The observer's task is to adjust the mirrors so that these two marks coincide with each other and are tangent to the cornea at its vertex.

When this is the case, the point m which is the image of M_1 in the mirror S_2 must lie in the line HM_2h, where H designates the point where the principal line of sight meets the cornea, and h designates the corresponding image-point produced by reflection in the pair of mirrors.

This line, being the principal line of sight, is perpendicular to the principal line of sight in the image; and hence also HM_1 must be at right angles to HM_2, and the angle ahm is the image of AHM_1 turned through 90° by a double reflection in the mirrors. The result, therefore, is that as soon as the mirrors are adjusted as described, the eye must be in a perfectly definite position with respect to the system of mirrors, *the position of the principal line of sight being given by the line mM_2, and its intersection with the cornea by the perpendicular let fall from M_1 on this line.* The eye is now brought into a series of different adjustments by being turned to the right or left, and each time the apparatus is adjusted until the marks on the mirrors coincide, as explained, with the image of the vertex of the cornea in the optical system. The apparatus is provided with two straight edges and a slate with its surface parallel to the plane of motion (which in this instance is horizontal). These contrivances enable us to mark not only the position of the principal line of sight, but also that of any edge that is perpendicular to it.

In the first place, the point of intersection gives the projection on the horizontal plane of a definite point in the principal line of sight; and since the point where the principal line of sight meets the cornea is at a fixed distance from this place depending on the constants of the apparatus, the projection of the latter point also can be found by laying off that distance on the principal lines of sight. Accordingly, the projections on the horizontal plane of the places successively occupied by the corneal point mentioned above can be obtained in this way. The result of the experiment was that these projections were found to be all very nearly on the arc of a circle.

In drawing conclusions from this fact, it should be noted (as Hering very properly indicated) that the circular path of the point under observation here (namely, the place where the principal line of

sight meets the cornea) is not of itself sufficient to prove that the rotation of the eye is permanently around a definite point. In Fig. 19 let the circle *ab* represent the path described: then for an infinitesimal rotation, by virtue of which the point *a* undergoes a slight displacement along this arc, the centre of rotation may be said to lie somewhere in a line perpendicular to this bit of arc. The centre of rotation must always be on the radius drawn from the centre of the circle to the instantaneous position of the point in question, but it does not have to be exactly at the centre itself. From a purely geometrical point of view it might vary with those radii and be anywhere on them.

However, MULLER's experiments enabled him to tell whether the centre of the circular path is the real centre of rotation, because for every position of the eye they gave also the position of the principal line of sight. If the centre of the circle is the centre of rotation, all the positions of the principal line of sight must be tangent to a smaller concentric circle, as shown in Fig. 19. (If the centre of rotation should be on the principal line of sight, all the positions of this line would have to intersect at the centre of the circle.) If therefore the positions of the principal line of sight have this relation to each other, we are actually justified in concluding that the centre of the circle is a constant centre of rotation.

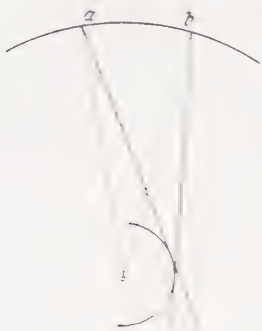

Fig. 19.

This may have been the case in a part of MULLER's experiment, where, as he says, the points of intersection of the successive positions of the principal lines of sight "formed a small circle concentric with the circular path." Under these conditions, the results were in accordance with those of BERLIN above mentioned, and indicated that the centre of rotation was slightly on the *median side* of the principal line of sight. If in other cases all positions of the principal line of sight were found to intersect nearly in a point not very far from that centre, in the strict mathematical sense this is not in accordance with the assumption of its being the centre of rotation. However, with the degrees of accuracy attainable by this method, perhaps here too the conclusion may be drawn that the changes of direction of the principal line of sight were very nearly in accordance with the central angle corresponding to the given portion of the path, and therefore that the centre of the circular path represents very closely the centre of rotation.

As to the absolute position of the centre of rotation, it was obtained directly in J. J. MÜLLER's experiments; being given with reference to the vertex of the cornea. It was likewise obtained directly in BERLIN's experiments, but with reference to the point of intersection of the principal lines of sight, that is, the centre of the pupil, the distance from the vertex of the cornea being found by adding in the distance (3.2 mm) between the pupillary plane and the vertex of the cornea.

The average distance from the vertex of the cornea as obtained by MÜLLER from his measurements of the horizontal movements was: 14.56 mm (left); 13.19 mm (right). BERLIN's measurements gave: 14.41 mm (left); 14.66 mm (right).

Incidentally, both observers corroborated each other as to the fact that the position of the centre of rotation (for horizontal movements) was not the same when the plane of fixation was raised or lowered as it was when this plane was horizontal; the centre of rotation being a little farther from the vertex of the cornea when the plane of fixation was raised, and a little nearer when it was lowered. However, the difference involved was never more than a fraction of a millimetre, as can be seen from the subjoined table.

		Displacement of the centre of rotation when the eye is	
		raised	lowered
MÜLLER	left	−0.6	+0.22
	right	−0.1	+0.32
BERLIN	left	−0.4	+0.09
	right	−0.34	+0.47

For vertical movements BERLIN found the centre of rotation on the average 1.56 mm more toward the front of the eye than it was for horizontal movements.

The earlier experiments of VOLKMANN and WOINOW, which have been referred to above, were for the purpose of establishing the existence of the centre of rotation, without determining its position; but they also conducted further experiments for the latter purpose. VOLKMANN's method consisted in observing the displacements of the centre of the pupil which accompany measured rotations of the eye. In this case also the distance was first found between centre of rotation and pupillary plane, and then the distance calculated from the vertex

of the cornea, as in BERLIN's method. WOINOW, on the other hand, tried to find the displacement of the vertex of the cornea.

Lastly, measurements were also made by WEISS,[1] which consisted in observing the displacements of a tiny reflex image in the cornea, the process being similar to that used by JUNGE.

The following table, as given by ZOTH, exhibits the results obtained by the various investigators.[2]

Distance of the Centre of Rotation from the Vertex of the Cornea

Observer		Refraction	Average value in mm	Length of visual axis in mm	Remarks
VOLKMANN		?	13.61	—	Horizontal movements, 51 observations
			13.37	—	Vertical movement, 43 observations
			13.45	—	Average for 10 persons
WOINOW	l.	hyperm. (?)	14.1	21.83	Horizonal movements
	r.		14.0	21.83	
J. J. MÜLLER	l.	−4 dptr	14.56	—	Horizontal movements Average of 100 separate measurements
	r.		13.19	—	
BERLIN	l.	−4/3dptr	14.41	—	Horizontal movements
	r.	−3 dptr	14.66	—	
WEISS		emm.	12.9	—	
DONDERS and DOIJER		hyperm.	13.22	22.10	Horizontal movements
		emm.	13.45	23.53	
		myopia	14.52	25.55	
MAUTHNER[3]		hyperm.	13.01	23.08	Horizontal movements
		emm.	13.73	24.98	
		myopia	15.44	27.23	

3. HELMHOLTZ's system of nomenclature as given on page 43 is not altogether satisfactory in some ways. Thus, he uses the word *Raddrehung* (or "torsional rotation") to describe *a certain kind of ocular*

[1] WEISS, *Archiv f. Ophthalm.* XXI (2). 1875. S. 132.

[2] ¶ See L. KOEPPE, Wo liegt näherungsweise der Schwerpunkt des lebenden Auges? *Deutsche optische Wochenschrift.* Nos. 16-18. 1923. (J.P.C.S.)

[3] *Vorlesungen über die optischen Fehler des Auges.* 1876. MAUTHNER's measurements referred to here were made by the method of DONDERS and DOIJER.

movement. In this sense a rotation of the eye around the line of fixation as axis would have to be termed a pure *Raddrehung*; and yet a rotation around any axis, which was not perpendicular to the line of fixation, but inclined to it at some other angle, would have to be considered as having a certain component amount of *Raddrehung*; this component vanishing in case the axis of rotation were perpendicular to the line of fixation. On the other hand, however, the "angle of torsion" (*Raddrehungswinkel*), as HELMHOLTZ calls it, denotes the measure of a certain angle which describes, not a motion, but a *position* of the bulbus. There would be no objection to this ambiguity, provided the connection was such that variations of this angle were invariably and solely the result of movements which were either "torsional rotations" in the first sense or contained a component rotation of this kind; that is, were executed around an axis inclined to the line of fixation. But this is not the case. The definition of the angle of *Raddrehung*, as given in the text, seems rather to imply that very often this angle undergoes variations when no component of *Raddrehung* is involved in the movement. This is always the case, for instance, when the eye passes directly from its primary into a tertiary position. Here the rotation takes place around a frontal axis, the line of fixation continues in one plane, and the movement does not have any component of *Raddrehung*. Nevertheless, the angle of *Raddrehung* assumes various values (positive or negative), which are different from zero. Unquestionably, the nomenclature here is calculated to make one expect the opposite and to this extent is apt to cause confusion. It has too been the source of much misunderstanding; and, indeed, some have ventured to suppose that HELMHOLTZ himself was under an erroneous idea here and had the wrong impression, that a component of *Raddrehung* was contained in the movements referred to above. There is no basis whatever for this supposition, as is shown by carefully reading what HELMHOLTZ says. But it is beyond question that the use of this terminology does run the risk of misunderstanding. Undoubtedly, it would be better to use different expressions for describing the position of the bulbus and the mode of motion.

HERING's terminology, therefore, has rightly found favour. He uses the word *rolling* (*Rollung*) to describe a certain mode of the *motion*, and reserves the word *Raddrehung* or, better still, *Raddrehungswinkel* to denote a *position* of the bulbus. Accordingly, *pure rolling* implies a motion around the line of fixation as axis; whereas every motion around an axis inclined to the line of fixation contains a certain component of rolling motion.

This is likewise the place to allude to certain differences with respect to the mode of treating and representing the ocular movements, which I think ought to be clarified to avoid misunderstandings.

The method, which is generally used in mechanics, and which is consistently employed by HELMHOLTZ also, consists in describing a rotation by giving the axis about which it takes place. It is a well-known principle of kinematics (as has been also stated above), that any infinitesimal movement may be regarded as being composed of a rotation around a given axis along with a translation parallel to this axis. If the latter can be left out of account, every infinitesimal rotation of the eye amounts to a rotation around a definite axis. In ordinary mechanics what is meant by this axis is the assemblage of those points in the eye (or its imaginary prolongation) which stay fixed during the motion.

HERING has a different method of dealing with this subject also, and so he sees it in certain aspects, which at first sight do not seem very clear to anyone who has considered it from the other point of view. He attaches importance to the movements of the line of fixation in the beginning; and thus he succeeds in resolving each infinitesimal rotation in a perfectly definite way, one component being around an axis perpendicular to the line of fixation, and the other around the line of fixation itself (rolling). Obviously, every rotation can be resolved in this fashion; and it is equally clear that while, from a purely geometrical point of view, this resolution is an arbitrary one, the advantage of it would seem to be indicated by the importance that is attached to the line of fixation.

So far as infinitesimal rotations are concerned, it is of comparatively little importance which mode of treatment is preferred; but when finite rotations are involved, the differences between the two methods are more significant. Thus, a steady continuation of the same rotation means something utterly different in the two systems of treatment. In the language of theoretical mechanics, and in HELMHOLTZ's mode of discussion, this sort of rotation is implied when the eye is turned through a finite angle around a definite line, which is the axis of rotation in the above sense. When such rotations are performed continuously around the same axis, the line of fixation will move in a plane, provided the axis is perpendicular to this line. But when the axis and line of fixation are inclined to each other at some other angle, the line of fixation will lie on the surface of a cone, and one end of it will traverse a parallel circle on a sphere. Such is the case, for instance, when a direction-circle is traversed; and hence, under such circumstances, there will be a continual rolling of the eye, depending in amount on the angle between the axis and the line of fixation. In HERING's method, on the other hand, when we speak of a continuous rotation of the eye around an axis, we mean that the line of fixation moves in a definite plane, whether any rolling occurs at the same time or not. In the latter case HERING speaks of a rotation around an axis fixed in space, which, however, continually changes its position in the eye. This description is justifiable and intelligible only on the basis of this special mode of treating the problem; whereas such a motion regarded in the ordinary physical way would be described as a rotation around continually changing axes. We need not express any opinion here as to which of these two methods has the advantage. To some extent it is a matter of taste, and concerns the special applications which one has in mind. As a matter of fact, in many cases it is justifiable to consider the movement of the gaze in the same direction as being a unitary motion, as, for example, when the eye travels up and down along a vertical line situated on one side of the field of view. This motion (assuming that it obeys LISTING's law exactly) would be described in the terms used in Physics as being a rotation around continually changing axes;

but in Hering's way of looking at it, it would be a rotation around an axis fixed in space; which is, however, accompanied by rollings, so that the position of the axis in the eye is variable.

On the other hand, it must be noted that Hering's method of treatment does not relieve us of the necessity of distinguishing carefully between the designation of the modes of motion and that of the adjustments of the eye. As above stated, this is precisely the advantage that is gained by having the two terms rolling and *Raddrehung*. The former is used to denote a mode of motion of the eye; whereas *Raddrehungswinkel* denotes an angle indicative of the instantaneous adjustment of the eye. When, therefore, (as is occasionally the case in the following pages) we use the term "angle of rolling" to describe an angle which has the same significance as the angle above mentioned, we are guilty of exactly the same fault of which we complained in Helmholtz's method. For instance, this is what Zoth[1] does when he proposes to use the term angle of rolling to denote the angle of "inclination of the upper end of the originally (or initially) vertical meridian of the cornea towards either the nose or the temple." Since the eye can be brought from its primary position into any other possible position without rolling, it is evident that the value of the angle of rolling, as thus defined, may vary also, although the motion does not have any component of rolling motion. This was precisely the objection that was found to Helmholtz's double use of the term *Raddrehung*. Accordingly, if we desire to describe the positions of the eye by means of other angles besides the angle of *Raddrehung*, (as, of course, we may do in various ways), new names must be devised for the purpose. Thus Meinong[2] proposed using the term *aberration* to denote the deviation from the vertical plane of the meridian which was originally vertical in the primary position.

4. The question [see page 44] as to whether, when the head is tilted on one side, the eye undergoes a rolling motion in the opposite sense (so-called compensatory rolling), has been very much debated. On the basis of the works of Ruete and Donders, Helmholtz has answered it in the text in the negative. However, subsequently, the existence of rolling motions of this description has been established by reliable investigations, and they have been carefully measured by trustworthy methods, the values obtained being found in good agreement (Javal, A. Nagel, Mulder, Skrebitzky, Woinow, Donders, W. Nagel).[3]

[1] Zoth, Augenbewegungen und Gesichtswahrnehmungen, in W. Nagel's *Handbuch der Physiologie*. Bd. III. S. 300.

[2] Meinong, *Zeitschrift f. Psychologie*, etc. XVII. 1898. S. 182.

[3] Javal in Wecker, *Traité théorique et pratique des maladies des yeux*. Paris 1866.—A. Nagel, Über das Vorkommen von wahren Rollungen des Auges um die Gesichtslinie. *Archiv f. Ophth.* XVII (1). 1871. S 247.—E. Mulder, Over parallele Rollbewegingen der Ooogen. *Academ. Proefschrift. Onderzoekingen physiol. Laboratorium te Utrecht.* III, 1. 1874. S. 168.—Idem, Über parallele Rollbewegungen der Augen. *Archiv f. Ophth.* XXI (1). 1875. S. 68.—Skrebitzky, Ein Beitrag zur Lehre von den Augenbewegungen. *Archiv f. Ophth.* XVII (1). 1871. S. 107.—Woinow, Beitrage zur Lehre von den Augenbewegungen. *Archiv f. Ophth.* XVII (2). 1871. S. 233.—Donders, Pflügers *Archiv* XIII. 1876. S. 419.—Idem, *Arch. f. Ophth.* XVI (1870).—Idem, Über das Gesetz der Lage der Netzhaut in Beziehung zu der der Blickebene. *Archiv f. Ophth.* XXI (1). 1875. S. 125. W. Nagel, Über kompensatorische Raddrehungen der Augen. *Zeitschr. fur Psychologie.* XII. S. 331
It is true that more recently there have been some experiments with negative results; see,

Particular attention may be called to W. Nagel's work which contains a very thorough discussion of the literature of this subject and the methods of investigation.

The most important thing in the technique of these methods is to eliminate entirely all other kinds of ocular movements; and, consequently, the position of the line of fixation with respect to the head should be kept absolutely steady. Several methods fulfil this requirement.

The planchette which is held between the teeth may be provided with a device for holding a conspicuous vertical band of some suitable sort against a neutral background and at a proper distance from the eyes for the production of after-images. After looking steadily at a point on the band for a long time, and then inclining the head toward one shoulder, the band will be seen to be crossed by its after-image at a more or less considerable angle (Donders).

A little mirror can also be attached to the planchette, by which the eye can see its own image and fixate the pupil. When the head is tilted on one side, the rolling movements then will be visible immediately (W. Nagel). The blurred patterns due to (regular or irregular) astigmatism of the eye may be employed also. Nagel mounted a little sheet of mica on the planchette in front of the eye; and made the blurred pattern of a distant luminous point coincide in a given way with a cross scratched on the mica. Then, on tilting the head, rotations of the blurred figure were observed with respect to the cross. The latter moves in perfect accord with the head, whereas the former, which moves with the eye, lags behind to a certain extent, executing a compensatory rolling.

Lastly, the position of the blind spot may be used for observing the rollings of the eye. The observer gazes steadily at a definite point on an opposite wall, and is required to adjust a mark, corresponding exactly to the blind spot, so that it completely disappears, that is, so that its image is directly on the blind spot. In order to make sure that during this adjustment there has been no change in the position of the eye with respect to the head, a spectacle-frame is rigidly attached to the head with a mark on it, which is made to coincide with the point of fixation. The experiment is performed repeatedly for various inclinations of the head toward the shoulder; and the angles between the line connecting the point of fixation with any point in the blind spot and the initial position of this line may be compared with the inclinations of the head, which are observed at the same time.

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for example, Contejean and Delmas, *Archives de physiologie normale et pathologique.* 5. S. VI. 1894. p. 687. The explanation of this contradiction is not yet clear.

The method of after-images and the method of the blind spot are both suitable for measurements; and under some circumstances the method of the blurred astigmatic patterns can be employed for this purpose too.

The results show that the rolling motion of the eye always amounts to only a small fraction of the inclination of the head. This fraction which is compensated by the opposite rolling of the eye, is about one-fifth when the rotations are slight, and gets as low as one-tenth when the rotations are considerable; as may be seen from the following tabulation of the results.

Inclination of head	15°	25°	35°	45°	55°	65°
Rolling (MULDER)	3	4	5	5.5	5.5	6
Rolling (KÜSTER)	4	6	6.5	7	8	9
Rolling (SKREBITZKY)	2	2.6	4.2	5.5	6.8	7.7

W. NAGEL'S Observations

Inclination of head	10°	20°	30°	40°	50°	60°	70°	80°	90°	100°
Rolling	1.3?	3.8	5.2	5.4	6.3	6.7	6.8	8.0	8.1	8.6
Fraction of Compensation	$\frac{1}{7.7}$?	$\frac{1}{5.3}$	$\frac{1}{5.8}$	$\frac{1}{7.4}$	$\frac{1}{7.9}$	$\frac{1}{9}$	$\frac{1}{10.3}$	$\frac{1}{10.0}$	$\frac{1}{11.1}$	$\frac{1}{11.8}$

DELAGE[1] found unequal amounts of compensatory rolling motion for the right and left eyes. However, according to ANGIER'S observations[2] this may possibly have been due to illusions.

The interest in this whole question consists mainly in its being dependent on the static organs, and as to how this connection is affected by experimental agencies, and in its being different in different animals, etc.; questions that cannot be discussed here.

5. Deviations from LISTING'S law that belong here [see page 50] have been described by HERING. They are such that when the eyes are raised or lowered, a certain amount of rolling accompanies the movement. HERING found (*loc. cit.*, p. 480) "that when the mean longitudinal sections are divergent upwards in the primary position, the divergence is increased by elevating the plane of fixation; whereas by lowering it they may gradually become parallel or indeed convergent upwards."

Moreover, it appeared also that the divergence of the mean longitudinal sections increased when, for any arbitrary inclination of the plane of fixation, the visual axes were turned to the right or left;

[1] YVES DELAGE, Le mouvement de torsion de l'oeil. *Arch. de zoologie experimentale et générale.* 1903.

[2] ANGIER, Vergleichende Messung der kompensat. Rollung der Augen. *Ztschr. für Psychologie u. Physiol. d. Sinnesorg.* XXXVII. S. 235.

and that this increase was greater, the more the plane of fixation was elevated.

6. The combination of convergent movements and rolling motions [see p. 52] has subsequently been frequently investigated (Hering,[1] Landolt,[2] and Donders[3]). These authors found, as Volkmann had found, that there was some connection in the sense that for convergence of the lines of fixation, the meridians that were apparently vertical had an increasing divergence upwards. According to Hering, the differences increase with increase of the angle of convergence and with lowering of the plane of fixation, and may get as high as 5° (*loc. cit.*, p. 96). The greatest deviations of this nature were observed by Landolt, whose results are given in the subjoined table.

Convergence	Elevation				Depression			
	25°	20°	10°	0°	10°	20°	30°	40°
0°	1°30′	50′	40′	[30′]	10′	5′	− 10′	−10° 5′
6°	2°30′	2°	1° 5′	1°45′	1°20′	1°	1°10′	1°30′
8°	3°	2°20′	2°20′	2° 5′	1°30′	1°30′	1°30′	1°35′
10°	4°20′	3°30′	3°	2°30′	2°	1°40′	1°50′	1°40′
12°	5°	3°40′	3°10′	2°55′	2°10′	2°	2°	1°30′
14°	7°30′	4°	3°40′	3°30′	2°40′	2° 5′	2°20′	1°30′
16°	8°	5°30′	4° 5′	3°55′	3°	2°10′	2°30′	2°
18°	9°	5°30′	5°	4°50′	4°	3°	2°50′	2°30′
20°	11°	7°30′	6°	5°40′	4°30′	3°30′	3°10′	2°45′
25°	15°	8°	7°	5°52′	4°50′	4°	3°30′	2°
30°	16°30′	10°	8°	6°50′	5°50′	4°	4°	1°

For unsymmetrical convergences, Hering found that the positions of the retinas with respect to each other were about the same as for equally great symmetrical convergence.

7. All the movements described here [p. 62], which have for their result the abolition of binocular diplopia, and which are made therefore "in the interest of single vision," have been termed lately *fusion movements*. Among the more recent investigations of this subject those of Nagel, Schmidt-Rimpler, Schmiedt, Guillery, and Hofmann and Bielschowsky may be mentioned here.[4]

[1] Hering, *Die Lehre vom binokularen Sehen.* 1868.

[2] Landolt, *Handbuch der ges. Augenheilkunde.* II. 1876. S. 660.

[3] Donders, Pflügers *Archiv* XIII. S. 419. 1876.

[4] Nagel, *Das Sehen mit zwei Augen.* 1862. S. 51.—Idem, *Archiv f. Ophthalm.* XIV (2). 1868. S. 235.—Schmidt-Rimpler, *Archiv f. Ophth.* XXVI (1). S. 115. 1880. Schmiedt, *Archiv f. Ophth.* XXXIX (4). 1893. S. 233. —Guillery, Pflügers *Archiv* LXXIX. 1900. S. 597.—Hofmann and Bielschowsky, Pflügers *Archiv* LXXX. 1900. S. 1.

As to the possible amount of the modifications of the position of the eye in this way, these researches agree in the main with the results given in the text. Thus, by ocular rolling A. NAGEL was able to overcome rotations of the test-object amounting to as much as 10°. HOFMANN and BIELSCHOWSKY found that they got the greatest values of the changes of position of the eyes when the displacements of the test-object exceeded the value that could just be compensated, and hence they made a distinction between a "compensating maximum" and the maximum of the deviation. For differences of height they could push the latter up to 5° or 6°; for rolling motions in the most extreme case they got it up to 16°; and for divergences to about 8°; by repeating the experiments systematically over and over again. It is remarkable that the rolling is approximately uniformly divided between both eyes, even when only the object viewed by one of them undergoes a real rotation; in other words, that the two eyes roll symmetrically in this case. This was verified both by A. NAGEL and by HOFMANN and BIELSCHOWSKY in accordance with HELMHOLTZ's data.

The experiments of GUILLERY and SCHMIDT-RIMPLER were concerned especially with the rate at which the fusion movements take place, how they are affected by practice, psychic conditions, narcotics, etc.

Incidentally, in the case of fusions, it is not strictly a question of definite movements, but rather of certain gradually developing modifications in the adjustment of the two eyes with respect to each other, which are thereafter maintained in mutual agreement by arbitrary movements. Thus it is probably a question of constant innervations that are being continually superposed on the fluctuations of the ordinary arbitrary innervation.

A more important matter so far as ophthalmology is concerned, which may be mentioned here because it also has to do with the significance of the fusion impulse, is the question as to the position of the eyes when the impulse for fusion, which is furnished normally by the object of vision, is annulled, as, for instance, when the field of view is completely darkened. In this case there should be normally an adjustment corresponding approximately to the primary position. Considerable deviations from it are called *heterophoria* (more specifically, endophoria,[1], exophoria, hyperphoria, and hypophoria– the terminology being easily understood). These are anomalies of the muscular mechanism, which are not directly manifest in ordinary vision, just because the fusion impulse here is sufficient to produce normal positions of the eyes. But it requires an abnormal muscular

[1] ¶ Or *esophoria*, as it is usually called in this country. (H.S.G.)

effort, and a whole series of troubles is usually connected with it. It is these latter that make this kind of condition a matter of practical concern. As the positions of the eyes cannot be observed in complete darkness, other methods have to be used in testing heterophoria. For example, the fusion impulse can be annulled by putting a powerful cylindrical lens (so-called MADDOX rod) in front of one eye. When the rod is vertical, a bright vertical line is seen as a broad band, and then it can be ascertained at what place in this band the sharp line appears as seen by the other eye. Accordingly, one can tell then whether the line of fixation of the eye with the MADDOX rod is likewise directed toward the bright line, or if it is not, how much it deviates from it, and which way.

In some instances abnormal movements of the eyes have been observed which were independent of the fusion impulse, but subject to the will in some way. The chief interest in these phenomena lies in their significance for the ideas they may suggest as to the origin of the law of motion; and hence the consideration of such motions will be postponed until we come to the Appendix.

8. There is a certain inaccuracy about HELMHOLTZ's treatment of this subject [page 67], which is in apparent conflict at least with certain subsequent facts. It is stated here that lines appear in different positions depending on the direction of the line of fixation by which they are viewed. On the other hand, it is shown afterwards (and, indeed, specially emphasized in this latter connection) that objects depicted on the retinal horizon are not by any means always regarded as being horizontal; in other words, that in estimating the objective position of the visual object, the angles of torsion or the deviations of the retinal horizon from the plane of fixation are not disregarded at all, but are taken into account approximately as they really are; and that, consequently, the eye is very accurate in regarding as horizontal dimensions that are really horizontal, no matter what the direction of fixation may be.

The solution of this difficulty is obtained by making a clearer distinction than HELMHOLTZ has done here between the perceptions that we get with different *adjustments* of the eye and those that are produced by the motion itself and are concomitant with the motion as such; at the same time carefully noting that between these two things there is not necessarily any perfectly simple connection at all. This fact, which may seem surprising at first, is not brought out absolutely clearly and definitely until afterwards. Indeed, there is always a tendency at first to suppose that the impression we get on moving the eye, that the objects themselves are shifted, is not obtained unless the latter are

perceived in different positions for the two adjustments of the eye, and that the motion discerned corresponds to the perceived difference between the positions of the eye in the two adjustments. Actually, however, this is not so at all. The conditions for the impressions of motion are more apt to be special, and are by no means deducible directly from the impressions of position.[1] And so, for example, it may very well happen that in moving the eye we get an immediate and powerful impression of a movement of the objects, although in the initial and final positions of the eye, both before and after this apparent motion, these objects were seen in the same positions. This is exactly the case that is considered here. If the plane of fixation is elevated and the gaze made to travel along a high horizontal line, the impression is very distinct that the eye glides along a line which is not straight but concave downward. On the other hand, as soon as the eye is arrested and caused to gaze steadily at some point on the line, the portion of the line in the vicinity of the point of fixation will appear to be horizontal, as it really is. Thus the perception of stationary objects occurs exactly in the way Helmholtz has described it in the other place referred to above, and hence the passage here under consideration certainly needs to be modified. However, it does not follow at all that impressions of displacement are produced by movement of the eyes.

It is all the more important to keep these relations in mind because the whole argument on which Helmholtz bases the principle of least orientation needs to be expressed more precisely in a similar manner. Helmholtz proceeds on the assumption that when the eye is turned to look at a point whose retinal image, originally situated excentrically at the place e, glides with the movement of the eye to the fovea centralis, the concomitant displacement of all other retinal images will be most easily recognized as being the expression of a movement of the eye, when this movement for the purpose of looking at the object imaged at e is invariably accompanied by the same displacement of all other retinal images; in other words, therefore, when the displacements are produced by rotation around the same axis. The truth of this proposition will be beyond doubt, provided it is shown that a definite change of fixation is never accompanied by apparent motion, when it is produced by rotation about a given axis; whereas apparent motions always occur when it is produced by rotation about another axis. Now this is actually the case. When the gaze is turned to the point that was imaged excentrically at e (that is, when the fovea occupies the place where e was formerly), apparent motions do

[1] The conditions alluded to here concerning the direct perception of motions will be discussed somewhat more fully hereafter; see Note 1 at the end of § 29.

occur, provided the movement of the eye is made by the shortest route (without rolling); whereas in every other instance it is accompanied by apparent rotations of the objects. Those illusions, whose reduction to a minimum amount requires the principle of easiest orientation, are, therefore, actually present.

This view of the matter corresponds to HELMHOLTZ's formulation of the principle, when he says that the sums of the squares of the errors are a minimum for all actual movements of the eye; this formulation being at the basis of his mathematical analysis. On the other hand, where several times in the course of the discussion, instead of using this principle, HELMHOLTZ considers the differences of perception for various positions of the (passive) eye, he introduces a modification, which, as we are obliged to say at present, cannot be substituted, with reference to the motion as such, for the other principle.

9. In connection with the problem which HELMHOLTZ considers here [page 71], HERING[1] and RITZMANN[2] afterwards made more accurate experiments concerning the relations between the movements of the eye and head. In considering these relations, it should be stated, first of all, that there are two things to be kept separate. Thus, in the first place, the question may be whether movement of the head on the body is performed according to the same laws as the movement of the eye in the head; that is, whether in case of the head also the entire adjustment can be represented by a unique function of those positions belonging for the time being to the originally sagittal line, and whether it is likewise true of it that movements from the primary position are executed around frontal axes. This question must be kept distinct from the other question, as to whether there is any rigid connection between the existing movements (or adjustments) of the head on the body and those of the eyes in the head. A question of special interest here would be to know whether, when both the head and the eyes turn out of their primary positions into some new positions, the axes about which we have to think of the rotations as being executed are necessarily always parallel to each other. If this were the case, a particularly simple consequence would obviously be that, when the eye was turned to look at any place, the position of the retinal horizon and the projection of after-images would have to be the same, no matter whether the given direction of the gaze was achieved simply by moving the eye alone or by moving the eye and head together. On the other hand, as can easily be shown by taking suitable examples, an agreement of this kind would not exist, if both the movements of the

[1] HERING, *Die Lehre vom binokularen Sehen.* 1868. S. 106.

[2] RITZMANN, *Archiv f. Ophthalmologie.* XXI. 1. 1875. S. 311.

head and the movements of the eye obeyed LISTING's law separately, without any fixed combination between the two movements. Thus, suppose that the head were raised, and that then the eye (obeying LISTING's law by itself) were turned to one side; the retinal horizon would continually coincide with the plane of fixation. But if the line of fixation were brought into the same direction by ocular rotation alone, the angles of torsion that were different from zero would remain the same.

The researches mentioned above throw light mainly only on the second question, namely, as to the connection between the movements of the head and eyes. RITZMANN's method consisted in rigidly connecting the head (by means of the planchette in the teeth) with a little tube, the axis of which coincided exactly with the line of fixation in its primary position. Now if by combined movement of eye and head the gaze were directed at any point on a wall just opposite the observer, then by looking through the tube the point could be designated toward which the line of fixation would have been directed by moving the head alone. Moreover, an arc, which could be rotated around this axis, and which was graduated in degrees, was connected to the same frame, and there was an adjustable mark on it. By turning this arc until it passed through the point of fixation, and making the adjustable mark coincide with it, it was possible to determine the axis around which the eye had turned from its primary position, and how far it had moved. In the first place, RITZMANN obtained a simple rule from his experiments; that is, he found that when, starting from the primary position, the points of fixation were directly above or below or to one side of the primary point of fixation, the movements of the head and of the eye take place around the same axes (vertical or horizontal and frontal). But even in this case the quantitative relation between movements of head and eyes is very different for different individuals. In elevating the gaze some of the persons experimented on made only one-third of the movements with their eyes, whereas others made as much as four-fifths of them in that way. However, in lowering the gaze, the movements of the eyes in all cases contributed most, those of the head least. In turning to look at points that were both upwards and sidewise, there was often no exact correspondence between the axes, unless definite rules could be established for the deviations. Finally, however, it was shown that when the gaze dwelt on one point for a long time, the relations usually changed gradually, and the part contributed by the motion of the head in the total deflection increased, while that contributed by the eyes diminished (the latter tending, therefore, to return to their primary position). And the position of the head for a

given direction of gaze may differ to a still greater extent, depending on its previous directions and on the path by which the given point of fixation was reached.

The experiments show that in any case there is no hard and fast connection between the movements of eyes and head, and that a considerable measure of freedom exists. They do not succeed in settling whether the movements of the head by themselves are according to LISTING's law, because all that was found was the position of a line fixed with respect to the head (the primary position of the line of fixation).

In HERING's experiments the position of the head by itself was generally not controlled. The tests consisted rather in finding the positions of after-images for combined movements of eye and head and then comparing them with those observed when the eyes alone moved. And HERING found "that when the experiments with after-images were repeated with the head free to move, if the gaze were allowed to travel up and down along a vertical line situated to one side, the after-image of a vertical cord that was fixated in the primary position always coincided approximately with that vertical line." "In this case the head is turned sideways, but not enough for the vertical cord just mentioned to come into the meridian plane." As a matter of fact, therefore, the position of the after-image is different, according as the fixation of certain points is accomplished by a combination of movements of eyes and head both or by movements of the eyes alone. According to what was stated above, we find, therefore, that there is no such fixed connection between the movements of the head and eyes as was mentioned there (that is, parallel positions of those axes about which head and eyes have to be turned in order to be transferred from the primary position into the other position). But the experiments also show that the movements of the head could not have occurred exactly according to LISTING's law. If the movements of head and eyes had corresponded absolutely to this law, the vertical after-images would certainly have coincided continuously with the vertical line, but not unless the lateral movement had been executed by the head alone and the up and down movements by the eyes; which, however, was not the case.

Hence, it seems justifiable to conclude that even the movements of the head considered by themselves are not strictly and generally in accordance with LISTING's law; and that especially for the modes of motion tested here they deviate from this law in a definite way.

Incidentally, another question that may be asked is whether the nature of the movements may not also be influenced by the special

character of the object presented to the eye. For instance, in the case of these very experiments of HERING's, it is easy to suppose that some unconscious tendency to make the nearly vertical after-image coincide with the vertical cord may have had some influence on the movement of the head.

On the whole, therefore, the conclusion is that the second of the two questions originally proposed must certainly be answered in the negative; there is no perfectly regular connection between adjustments of head and eyes, but rather the two may be combined together with a considerable degree of freedom. Nor can the first question be answered unequivocally in the affirmative. It is more than probable that under some circumstances the modes of movement of the head are different from LISTING's law.

We must not omit to add here that it would be a gross misunderstanding of what HELMHOLTZ says in the text to suppose that the facts that have just been stated conflicted with it. All that HELMHOLTZ says, and certainly all that he meant, is that there is a certain analogy between the movements of the head and those of the eyes, simply in so far as the former are likewise executed from the primary position around frontal axes, as we may say. The observations which are reported in the text may be considered as proving that this rule is approximately true, but HELMHOLTZ did not intend to say that it was *absolutely accurate*. Up to the present time no special investigation has been made to determine the extent of the deviations from this rule, and it cannot be ascertained from the preceding results.

Still less did HELMHOLTZ intend to assume that LISTING's law was obeyed perfectly *generally* by the movements of the head (particularly movements from secondary to tertiary positions). There is no reason to suppose that he considered it likely that there was any fixed functional connection between movements of eyes and head. At any rate there is no expression or statement of his that could justify us in such a conjecture.[1]

11. The binocular method of testing the law of rotation by the experiments mentioned in the text [page 115] was used especially by HERING[2] and by DONDERS,[3] with certain modifications of procedure.

In both sets of experiments a wire or hair as seen by one eye was made to coincide binocularly with the middle of two other parallel threads as perceived by the other eye. With extraordinary accuracy the first thread can be adjusted so that it appears to lie parallel between the other two.

The apparatus as made by DONDERS is known as the *isoscope*.

12. Further measurements of this same sort [p. 118] were subsequently made by VOLKMANN.[4] The subjoined table, compiled by

[1] ¶ Note 10 was inserted in the text (page 83), and is consequently omitted here. (J.P.C.S.)

[2] HERING, *Die Lehre vom binokularen Sehen.* Leipzig 1868.

[3] DONDERS, *Archiv f. Ophthalm.* XXI. 3. 1875. S. 100.

[4] VOLKMANN, *Berichte der königl. sächs. Gesellschaft der Wissenschaften, Math.-phys. Klasse.* 1869.

ZOTH,[1] contains the average results of VOLKMANN's measurements of 30 skulls, side by side with the data of RUETE and FICK which were given in the text, so that they can all be compared together. The direction of the x-axis is frontal (being reckoned as positive toward the outside), that of the y-axis is sagittal (being reckoned as positive toward the front), and that of the z-axis is vertical (being reckoned as positive upward).

Coördinates of the Points of Origin and Insertion of the Ocular Muscles for the Initial Position

Muscles	Coördinates	Origins			Insertions		
		FICK	RUETE	VOLKMANN	FICK	RUETE	VOLKMANN
Rect. sup.	x	−16	−10.67	−16	0	+ 2	0
	y	−31	−32	−33.05	+ 7.9	+ 5.67	+ 6.34
	z	+ 6.5	+ 4	+ 3.6	+ 9.1	+10	+10.48
Rect. inf.	x	−17	−10.8	−16	0	+ 2.2	0
	y	−30	−32	−33.05	+ 7.9	+ 5.77	+ 6.73
	z	+ 2	− 4	− 2.4	− 9.1	−10	−10.24
Rect. ext.	x	−15	− 5.4	−13	+ 9.1	+10.8	+10.08
	y	−31	−32	−35.29	+ 7.9	+ 5	+ 5.21
	z	+ 2	0	+ 0.6	0	0	0
Rect. int.	x	−18	−14.67	−17	− 9.1	− 9.9	− 9.65
	y	−30	−32	−31.29	+ 7.9	+ 6	+ 7.55
	z	+ 4	0	+ 0.6	0	0	0
Obl. sup.	x	−19.6	−14.1	−15.27	+ 4.6	+ 2	+ 2.9
	y	+10.9	+10	+ 6.95	− 2.7	− 3	− 5.70
	z	+12.8	+12	+12.25	+ 9.9	+11	+11.05
Obl. inf.	x	−18	− 8.1	−11.1	+10.4	+ 8	+ 8.71
	y	[−30?]	+ 6	+10.05	− 6	− 9	− 8.47
	z	[+ 6?]	−15	−15.46	0	0	0

It should be noted that RUETE and FICK used the centre of the eye as origin of coördinates, whereas VOLKMANN used the centre of rotation, which he took as being 1.29 mm beyond the centre of the eye. Accordingly in the original data and likewise also in ZOTH's tabulation, VOLKMANN's y-values should be diminished by 1.29 in order to compare them with those of the others. I have made this correction in ZOTH's figures, so that in the preceding table everything is referred to the centre of the eye.

The table below gives the positions of the axes of rotation, as calculated from the above values and compiled by ZOTH, together with the corresponding earlier results of FICK and RUETE.

[1] ZOTH, *Sitzungsber. der Wiener Akademie, Math.-naturw. Klasse.* 109 (3). 1900. Also, NAGELS *Handbuch der Physiologie.* III. S. 289.

Angles between the Positive Directions of the Axes of Rotation and the Axes of Coördinates

Muscle	x-axis (outward)		y-axis (to front)		z-axis (upward)	
	RUETE	VOLKMANN	RUETE	VOLKMANN	RUETE	VOLKMANN
R. externus....	90°	90°52′	90°	88°40′	180°	178°35′
R. internus....	90°	89°19′	90°	90°45′	0°	1° 1′
R. superior....	161°30′	150° 5′	109°30′	113°47′	90°	72°55′
R. inferior.....	19°	31°53′	71°	66°	90°	108°34′
O. superior....	51°	53°46′	141°	146°42′	84°30′	100°45′
O. inferior.....	127°	129°19′	37°	39°54′	90°	83°46′

A set of interesting data with respect to the parts surrounding the bulbus and their mechanical significance, chiefly as being contrivances for restricting the ocular movements, has been compiled by MOTAIS.[1] However, as they are mainly matters of anatomy, perhaps this is not the place to insert them.

13. Other forms of ophthalmotrope [see pages 118, 119] have been devised by LANDOLT, AUBERT, DONDERS, BROWNING, and BOWDITCH.[2] However, a much simpler method, which is perhaps also more useful for many purposes than such mechanical models, consists in employing schematic drawings, designed by various authors, for exhibiting the actions characteristic of the individual ocular muscles. One that is extensively used and particularly simple is that given by HERING (Fig. 20). It represents the path that would be traversed on a frontal plane by the point where the line of fixation meets this plane, when one of the muscles was isolated and

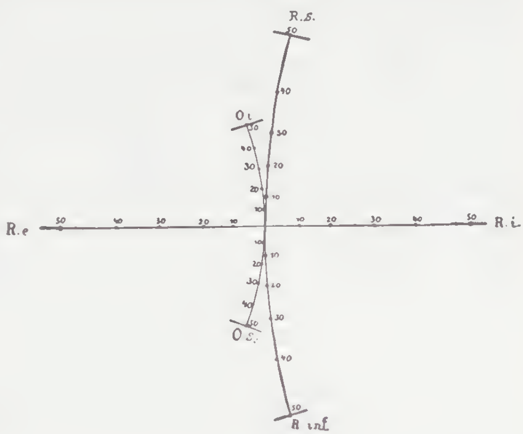

Fig. 20.

[1] MOTAIS, *Anatomie de l'appareil moteur de l'oeil.* 1887.

[2] LANDOLT, *Trans. of the Ophth. Society.* XII. (1894).—AUBERT *Zft. Instrumentenkunde.* VII. (1887).—DONDERS, *Archiv f. Ophth.* XVI. 1870.—BOWDITCH, *Journal of the Boston Society of med. Sc.* II. (1898).—BROWNING, *Archiv f. Augenheilkunde.* XI. (1881). S. 69.

acted by itself. The thick marks in the diagram indicate at the same time the positions of the projection of the after-image of a horizontal band which was fixated in the primary position, and enable us to tell therefore the magnitudes of the angles of torsion produced by the muscles.

Other diagrams for exhibiting the actions of the ocular muscles have been designed by WINTERNITZ,[1] ZOTH[2] and ELSCHNIG.[3]

Another demonstration apparatus that may be mentioned here is the model which I have used for a long time for demonstrating the projection of the after-image of an originally horizontal cross, when the eye is turned around a vertical, horizontal or oblique axis; and especially in the latter case the production of angles of torsion that are different from zero. It consists of a hollow brass sphere representing the eye. In its equator which lies in the frontal plane 8 short little tubular sockets are inserted 45° apart, two of them pointing vertically up and down, two horizontally right and left, and the others midway between these. For fastening the globe and for turning it around given axes, a fixed brass ring of circular form is supported on a post, the ring being perforated with holes, which are likewise 45° apart to correspond to the tubular projections. By inserting suitable pegs through two opposite openings in the corresponding sockets on the globe, the model of the eye can be turned around the axis thus determined; that is, just as desired, around a vertical or horizontal axis or around an axis extending diagonally from the upper corner on the left to the lower corner on the right or from the upper corner on the right to the lower corner on the left. If it is desired to fasten the globe, all that is necessary is to insert another peg. In order to visualize a projection of the after-images, a little tube is attached at the anterior pole of the globe, which contains a convex lens that can be shifted through several centimetres; and at the posterior pole there is a platinum wire cross, which can be rendered incandescent by a suitable current of electricity. The image of this cross is projected by the lens on a screen; and by turning the globe around its various axes, it is easy to realize how the cross is reproduced on the screen.[4] It is advisable to make rather considerable rotations, but in such cases it is never necessary to use a screen with a set of horizontal and vertical lines on it, because the oblique projections

[1] WINTERNITZ, *Wiener klin. Wochenschrift.* 1889.

[2] ZOTH, *Die Wirkungen der Augenmuskeln.* Wien 1897.

[3] ELSCHNIG, *Wiener klin. Wochenschrift.* 1902. No. 11.

[4] In turning the globe in this way the image falls on the screen at places that are somewhat farther away than the model, and in order to keep the image in focus it is necessary to shift the lens a little.

of the cross, and especially the difference between the part that was originally horizontal and the horizontal lines of the screen, that is, the angles of torsion, can be perceived well enough as it is. It will be seen that the optical conditions are exactly the same as those given in the experiments with after-images.

14. In conclusion, we must mention here some investigations relating to certain phases of the ocular movements which have not been considered in the text. The speed of these movements was investigated by LAMANSKI,[1] under HELMHOLTZ's supervision. This was done by means of an intermittent light of given period. When a light of this nature is in the field of vision, as the gaze passes from one mark to another, a series of separate after-images will be developed. The distance between them will be greater and the total number of them less, the faster the gaze travels. If a denotes the angular interval between two after-images and p the period of the intermittent light, the angular velocity of the eye will be a/p. For a we can substitute approximately s/n, where s denotes the entire length occupied by the after-images and n denotes their number. In his experiments LAMANSKI found velocities between 1.883 and 4.091 times 360° per second. On the average the velocity was rather more for horizontal motion than for vertical or oblique motion. Whether the head was erect or bent forward or backward, had no appreciable effect.

Other determinations of the speed with which the gaze travels have been made by BRÜCKNER[2] (who likewise used the method of after-images), by GUILLERY[3] (by another process in which rotating discs were used), and by HOWE.[4] BRÜCKNER found that the initial speed of the eye was decidedly greater when a very extensive movement was contemplated than when only a slight movement was intended.

In greater measure still, GUILLERY found that the speed of movement of the gaze depended on a series of conditions. According to his observations, in case of extensive movements the speed in the middle part of the path is greater than at the beginning or end. Moreover, in case of monocular observation, inward movements were executed more rapidly than those directed outward. Accordingly, movements which were controlled by both eyes were less rapid on the whole than the highest monocular values.

LAMANSKI noticed also in his experiments that when the gaze was

[1] LAMANSKI, PFLÜGERS *Archiv.* II. S. 418. 1869.

[2] BRÜCKNER, Über die Anfangsgeschwindigkeit der Augenbewegungen. PFLÜGERS *Archiv.* XC. 1902. S. 73.

[3] GUILLERY, PFLÜGERS *Archiv.* LXXIII. 1898. S. 87.

[4] HOWE, Über die Schnelligkeit der seitlichen Augenbewegungen. *Archiv für Augenheilk.* LI. 1904. S. 51.

diverted from the first fixation mark to another one (which had been seen excentrically at first), the route from the initial to the final position was not always the shortest. This can be perceived because the after-images do not lie in a straight line. In case of horizontal and vertical movements, he succeeded after some practice in getting the after-images in a straight line. On the other hand, when the movements were oblique, the after-images (as WUNDT[1] had previously stated) formed curved lines (concave inward for motions directed obliquely inward, and concave outward for motions directed obliquely outward). More extensive researches especially in regard to these phases of ocular movement were afterwards carried out also by HERTZ,[2] who investigated the after-image lines developed by a continuously visible light.

DELABARRE[3] and ORSCHANSKY[4] succeeded in getting a direct record of the ocular movements. Upon a cornea anesthetized with cocaine they placed a little glass or metallic cup which exactly fitted it. The motion of this cup could be transmitted to a recording device by cords; or the motion might be projected by means of a little mirror attached to it, and thus be made visible.[5]—K.

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§28. The Monocular Field of Vision

Ordinarily, we see with both eyes at the same time, turning them in the head first one way and then the other, and likewise from time to time changing the position in space not of the head only but of the

[1] WUNDT, *Beiträge zur Theorie der Sinneswahrnehmung*. 1862. S. 202.

[2] HERZ, PFLÜGERS *Archiv*. XLVIII. S. 385. 1891.

[3] DELABARRE, A method of recording eye-movements. *American Journal of Psychology*. IX. S. 572. 1897.

[4] ORSCHANSKY, Eine neue Methode die Augenbewegungen direkt zu untersuchen. *Zentralblatt f. Physiologie*. XIII. 1898. S. 785.

[5] ¶In addition to the works mentioned in this chapter, the following is a list of some more recent literature:

G. T. STEVENS, *A treatise on the motor apparatus of the eyes*. Philadelphia, 1906.—K. BAR´NY, Apparat zur Messung der Rollbewegungen des Auges. *Ber. IV. Kongress. f. exper. Psychol.*, 1911, p. 252.—A. HÖGYES, Über den Nervenmechanismus der assoziirten Augenbewegungen. *Monat. f. Ohrenhk.*, 46 (1913), 1353-1443 and 1554-1571.—M. W. LORING, An investigation of the law of eye-movements. *Psychol. Rev.*, 22 (1915), 254-270.—L. BURMESTER, Kinematisches Aufklärung der Bewegung des Auges. *Münch. Sitz.-Ber.*, 1918, pp. 171-202.—H. V. NEAL, The history of the eye muscles. *J. of Morph.*, 30 (1918), 433-453.—N. GRÜNBAUM, Représentations de la direction et mouvements des yeux. *Arch. néerl. de Physiol.*, 4 (1920), 216-223.—E. J. GEORGE, J. A. TOREN and J. W. LOWELL, Study of the ocular movements in the horizontal plane. *Amer. J. of Ophthalm.*, Ser. 3, 6 (1923), 833-838.—A. DUANE, The associated movements of the eyes, etc. *Amer. J. of Ophthalm.*, Ser. 3, 7 (1924), 16-26.—P. K. KROMAN, Movements of the human eye. *Acta Ophth.* 2 (1924), 54-75.—J. M. BANISTER, A series of papers on the ocular muscles. *Amer. J. of Physiol. Optics*, 5 (1924), 3-31, 154-169, 277-296 and 491-513. (J. P. C. S.)

whole body. Thus, we are in the habit of letting our eyes roam about, *fixating* first one point and then another of the object in front of us; that is, both eyes are turned so as to get the image of the point of fixation on the centres of the two retinas simultaneously. By thus using the eyes, we are enabled to obtain correct perceptions of the location of the visible object whose rays pursue rectilinear paths and enter the eye without having been deflected.

In fact, according to the laws by which the light is refracted by the ocular media, as explained in §10, it is easy to see that when we know the position of the body and head, together with the positions of the two eyes in the head, including, therefore, the positions of their nodal points, and when we know also the locations of the two retinas on which the images of the luminous point are formed, theoretically we should be able to determine uniquely the place where the luminous point really is. For then all that is necessary is to draw a straight line from the retinal image in each eye through the corresponding nodal point and prolong it. The two lines of direction can meet only in one point, and the luminous object must be at this place.

Incidentally, the accuracy of the determination of the actual location of the visual object in space will depend on how accurate the various data are which have been enumerated above as being necessary.

Thus suppose we have given:

1. The requisite sensations for supplying correct information as to the position of body and head with respect to some base chosen for making the measurements, for example, the floor on which we happen to be standing;
2. The requisite sensations for enabling us to estimate correctly the positions of the eyes in the head; and
3. Factors in the sensation (so-called *local signs*), whereby the stimulations of the retinal areas, where the light acts which comes from the object-point A, can be discriminated from the stimulations of all other places on the retina (We know nothing whatever as to the nature of these latter stimulations; and we infer that they must be of the same kind there just because we have the faculty of distinguishing luminous impressions on different parts of the retina.);

then we have the requisite data to enable us to find the unique location in space of the point A. If the point A were anywhere else, another aggregate of sensations would have to be excited by it. We know by experience too that as a rule we do actually learn to judge correctly by sight the place where the point of the object is. It is true, the

accuracy of this determination is variable and depends especially on how near the images of the point A in the two eyes are to the *fovea centralis*.

Accordingly, we shall have to inquire now how much the factors of the sensation above mentioned contribute by themselves to the accurate perception of the location of the object. There will be no need here of investigating further what sensations are concerned in judging of the position of the body with respect to the floor and of the head with respect to the body, as these are questions that belong to the physiology of the perceptions of the senses in general rather than to that of the sense of sight. Let us assume, therefore, that the position of the head with reference to the base used for the measurements in space is accurately known in each instance. Then all that remains to be ascertained is how much is contributed to our recognition of the location of the object, (1) by movements of the head, (2) by movements of the eyes in the head, (3) by vision with one eye, and (4) by vision with both eyes.

We shall begin by seeing what can be learned by using one eye only, leaving out all movements of the head. On the other hand, it will usually be assumed in this chapter that the eye is free to move in the head.

In the first place, it is evident that when we know the location and adjustment of one eye and the location of the retinal image of a luminous point, for which the eye is accommodated, we can draw a straight line from its image on the retina through the nodal point of the eye; and we know too that the luminous point must lie on this line in front of the eye. Necessarily, however, its position on this line remains unknown unless some other means is afforded to help us decide this question. Of course, it might occur to us to suppose that the accommodation of the eye would be of some service. If the eye were accommodated for the point as well as possible, perhaps the amount of effort required for this purpose or the size of the blur circle might enable us to tell something about the distance. In §30 we shall inquire as to what means are afforded by monocular vision for judgment of distance, and it will be seen then that accommodation is an exceedingly unsatisfactory means indeed for this purpose. And so, if we leave out the slight variations of distinctness of the image that can be produced by changing the accommodation, there is nothing else in the factors or details of the sensation which would afford any clue to the distance of the luminous point.

It was assumed above that the eye was accommodated exactly for the luminous point. Then, as was stated, its direction could be found by drawing the line of direction from the image on the retina through

the nodal point; or else some other ray may be traced from a point in the pupil to the image on the retina. If any such ray is correctly constructed by the laws of refraction, as explained in §10, so as to determine the path it had before it reached the eye, it will pass through the luminous point where it emanated. In this case, therefore, it would make no difference what ray going through the pupil was selected for finding the direction in which the luminous point was.

However, it does make a difference when the image on the retina is that of a luminous point for which the eye is not absolutely accurately accommodated. In a case of this kind the centre of the blur circle may be regarded as the place where the image on the retina is.[1] But, as stated in Vol. I, p. 124, the ray coming from the luminous point which ultimately goes through the centre of the blur circle passes through the centre of the pupil and is called a *line of sight* (*Visierlinie*). If the luminous point were moved to and fro along this line of sight, nothing in the sensation would be altered except that the blurred image of it would undergo slight variations of size one way or the other, which might be too small to notice even with very considerable variations of the distance.

It can also be shown, that even when the eye is accommodated for near vision, the position of the centre of the blur circle on the retina is not appreciably altered. The mathematical calculation will be given at the end of this chapter.

In order to obtain an idea of what can be perceived in the external world by one eye, without the aid of movements of the head and without taking account of differences of accommodation, the best illustrations are afforded when the objects of sight are very distant bodies. For when the objects are very far away, considerable movements of the head do not produce any variation of the image that could not be produced also by rotations of the eye alone. Indeed, in looking at infinitely distant objects it makes no difference whether the other eye is open or shut. For nothing of importance is added to the detail of sensation by using the second eye unless the line of sight drawn to it intersects that of the first eye at some measurable distance. When both lines are practically parallel and run side by side until they are lost to sight, we have no means of telling anything about the actual distance except that it must exceed a certain limit in that direction.

In considering very remote terrestrial objects, our previous knowledge of their actual forms, distances, colours, etc., is frequently of service to us in interpreting our field of view. If we wish to get rid

[1] We are speaking here simply of luminous points. That the case is different along the edges of luminous areas, has been explained in §21 in the theory of irradiation.

of all these aids in the way of previous recollections, we must choose an object adapted for this sort of investigation, such as the starry sky. There we can find objects of whose form, dimensions and distance we have no previous idea at all. In perceiving them there is no advantage in using both eyes or in any movements we can make; we can learn just as much about them with one eye which is kept steady.

Under these circumstances, objects which really are extended in space of three dimensions appear to us as having only two dimensions. The best we can do is to tell the direction of the line of sight for each separate visible point. A direction like this does not need to be located by three parameters, as is the case with a point; only two are necessary. Thus the position of a star will be given by two angles, either its longitude and latitude with respect to the pole and equator, or its right ascension and declination with respect to the ecliptic.

A magnitude of two dimensions in space constitutes a surface, the positions of each point on it being given by a pair of parameters.[1]

Hence, in monocular vision, on the assumption that the centre of rotation of the eye continues stationary, one dimension, namely, distance cannot be discerned; the consequence being that objects cease to look like bodies of three dimensions in space and appear as if they were distributed over a surface. This apparent superficial configuration of the objects of vision is called the *field of vision* (*Gesichtsfeld*). Thus, for instance, the stars appear to be scattered over the imaginary surface of the celestial vault.

I must beg the reader to notice that I was careful not to say that the objects appear to us to be distributed *in or on a surface*, but only *as if they were in a surface*, in superficial configuration, that is, in a configuration which is different with respect to two dimensions. As a matter of fact we do not necessarily think of a definite surface at a definite distance to which the stars and the distant mountains on the horizon are attached, although such expressions as the celestial vault and the crystalline spheres of the ancients are natural to a more childlike form of conception, where the tendency is to make everything

[1] ¶ Readers who are familiar with the mathematical terms employed here will not need to be reminded that whereas a single parameter is sufficient for determining the position of a point on a curve, it takes two parameters to locate a point on a surface. Thus, for example, all the points on a curve may be represented by three equations of the form:

$$x = f_1(t),\ y = f_2(t),\ z = f_3(t),$$

where t denotes a parameter which varies in some continuous manner; and hence any point on the curve may be found by means of these three equations. Similarly, a surface may be represented by three equations of the type:

$$x = f_1(u, v),\ y = f_2(u, v),\ z = f_3(u, v),$$

whereby each of the coördinates x, y, z of any point on the surface is expressed as a function of two parameters u and v, which, each independently of the other, may assume all possible values. (J.P.C.S.)

as realistic and concrete as possible. The fact that it was supposed necessary to assume some definite surface, usually spherical in form, as being the temporary field of view of each eye, has been responsible for many a difficulty in physiological optics. Any function of two variables may be represented on a surface. Thus in §20 colours of the same luminosity were represented on the colour chart according to certain conventions. In this case the two variables by which the colours differed from one another were hue and saturation. Suppose we start with a certain colour and pass through a continuous gradation of hues, returning finally to the original one (that is, suppose we draw a closed line on the colour chart); then all the colours will be divided in two entirely separate groups (one on the inside and the other on the outside of the closed line), and in order to pass continuously from a colour of one group to one of the other group, it will be necessary to go through one of the colours of the first group that is on the line separating the two. Now this latter circumstance is characteristic of any simple continuous surface. Every closed line drawn on it divides it in two portions, and we cannot pass from a point in one portion to a point in the other without crossing this line. It is just this analogy that enables us to form the idea of a system of colours, by representing them as being distributed over a surface. And this is all we do when we picture the objects as being projected on the imaginary surface of the field of view, leaving its location in space, however, completely indefinite.

Incidentally, it is easy to understand too that this conception of a superficial distribution of the objects in the field of view must continue to persist, even when along with it our sense of sight enables us to have perfectly exact and correct conceptions of the actual distribution of the objects in space. For there will always be this peculiarity about the conception, that, after letting the gaze traverse the field in a closed line, it cannot pass from a point within this enclosure to one outside without crossing the line. Having traversed the outline of a window with my eyes, I cannot pass from an object, which I see outside the window, to one on the walls of the room, without letting the line of fixation cross the edge of the window. This is the essential characteristic indication of a superficial configuration of the objects of vision; although, of course, we are well aware that in actual space innumerable lines may be drawn from those external points to the one on the wall of the room, which do not intersect the edge of the window at all.

The possibility of recalling the appearances of things by means of drawings and paintings on canvas is due to the very fact that in glancing over the objects in the field of view they were found to be

arranged as if on a surface. The painter who wishes to represent a landscape does not take the trouble to find out how far every point of the scene really is from the eye or from another point of the landscape, but simply whether the eye has to turn upward or downward and to the right or left from one point to the other, and what excursion his eye has to make. The flat picture is recognized as being similar to the corporeal object, provided the same movements of the eye have to be made to pass from one point in the picture to another as were required in looking at the corresponding points of the object one after the other.

It is obvious too that in this simple manner we can ascertain the *mode of arrangement* of the points on the apparent surface of the field of vision, without any measurements of dimension at first.

The easiest way of understanding what this means is to think of a picture drawn on a flat sheet of elastic rubber. We can stretch it afterwards any way we like, thereby altering all the linear connections between its various parts and the angles between the different lines in any arbitrary fashion. And yet, in spite of all the changes, every closed line drawn through the same series of points in the picture continues to enclose the same invariable set of other points in the picture and to exclude the other half; the sequence of points remaining the same in every continuous linear series of points in the picture, no matter how much the size and form of the various portions of such a line may be altered. The mode of arrangement of the points on a flat geographical map is likewise the same on a terrestrial globe, and yet the relative dimensions on the flat map cannot correspond exacty to the globe, the correspondence being less and less exact, the greater the portion of the earth's surface that is represented.

If there are two surfaces whose points correspond to each other in some definite way, we speak of the *arrangement of the points* on the two surfaces as being of the same sort (*gleichartig*), provided all series of points lying on a continuous line on the first surface have a corresponding series of points likewise lying on a continuous line on the other surface, and provided the points occur in the same sequence on both lines.

By letting the eye wander over the field, we perceive immediately the order of succession of object points in the field; and, therefore, in the first place the *arrangement* of the points in the field of view can be ascertained at once by letting the eye traverse it in this way.

Hereafter we shall investigate how the relative dimensions can be determined by ocular measurements, and to what extent. At present we shall simply say, in the first place, that, at least so far as adults are concerned, the eye determines the arrangement of the points in

the field of view not simply with respect to objects over which the gaze can roam, but we get also a definite, superficially arranged picture of those objects and stimuli whose location remains the same with respect to our retina and which move with our eye. This is true in regard to the after-images, the retinal vessels, the polarisation brushes and nearly all subjective phenomena generally. No matter how the eye moves, a certain definite point of any subjective image of this sort will always correspond to the point of fixation, and different parts of the image can never be brought to the centre of the retina one after the other. Consequently, simply by the impression made on the stationary retina by the stationary image, we are enabled also to ascertain the arrangement of the visible points in the field of view, without having to make movements each time to determine the sequence of the various points of the object.

In order to explain these facts, the assumption may be made, and indeed has been made, by those who adopt the *intuition* (*nativistischen*) *theory*, that we are born with a certain knowledge of the arrangement of the retinal points on the surface of the retina, and perhaps even of the extent of the intervals between them; whereby we are immediately enabled to perceive which points of the retinal image lie on a continuous line each in contact with the one next to it, and which points do not lie in this way. Of course, any such assumption as this puts an end to further discussion as to the origin of the superficial field of view.

On the other hand, it is evident that the faculty of recognizing and appreciating the arrangement of the objects in the field of view even without moving the eye, may also be acquired, as is assumed in the *empirical theory* of the perceptions of vision. For whenever the arrangement of the portions of a stationary object has been determined by ocular movements, as long as we happen to gaze steadily at one of its points, we shall get a stationary impression of its various parts on the retina; and so we can learn to see by experience how two points which by moving the eye have been recognized as being adjacent, will be represented in the stationary image in the eye. In other words, in terms of anatomy, we may learn to know by experience what are the local signs of the visual sensations corresponding to adjacent retinal fibres; and having learned this, we are enabled to tell, from the absence of variation in the impression made by an object that is stationary with respect to the eye, what is the arrangement of the points in the field of view.

Accordingly, in the following discussion we shall have to see whether, without the hypothesis of having been born with a knowledge of the arrangement of the retinal points, the facts can be explained by

the known capacities of the memory (*Sinnengedächtniss*). Naturally it is not possible to make direct experiments as to this matter on children just after they are born. And there are practically no data to be obtained from persons who were born blind and operated on, because in nearly all such cases these individuals were suffering from cataract, and although it is true they could see very little through the clouded crystalline lens, still they were able to tell the direction of the more intense light; and hence they were not entirely without some experiences as to the localization of their retinal impressions. Far more valuable in this connection than the experiences of persons who had been operated on for cataract would be the cases of congenital closure (or atresia) of the pupil, which had been cured by an artificial pupil formation. Some remarkable cases of this nature will be cited at the end of this chapter.

But not only do we perceive the arrangement of the object-points in the field of view, as mentioned above, but we discern also the *relative dimensions* of the lines and angles with some degree of accuracy. The artist who endeavours to reproduce the impression of material bodies by means of a flat picture must not be content simply with arranging the points of the object in the same sequence in his delineation, as they occur when the eye is allowed to roam over them; but he must strive too to include certain relative dimensions between the distances of the various points, so as to make the flat picture have the same appearance as the material object. And if a drawing which has been executed on a sheet of rubber is elongated by being stretched, its appearance to a spectator will be altered, no matter if the points on the surface do have the same sequence as before.

Before we can definitely discuss the facts concerning how our judgments of relative dimensions are formed, and can find out how they originate, it will be necessary to give some definitions of the surfaces on which the images of the field of vision are supposed to be projected.

As a rule, the term field of vision is used to describe the appearance of the visual objects in front of us, so long as we are not thinking of their distances away from us, but simply of their apparent superficial arrangement alongside one another; without specifying precisely whether the objects are to be considered by gazing steadily in one direction or by letting the eye roam about, perhaps too with contributory movements of head and body. However, in the following analysis of our perceptions it will be necessary to make a clear distinction between these various cases. The vague term *field of vision*

(*Gesichtsfeld*) may be retained, so long as no distinction of this sort has to be made between the passive eye and the mobile eye, or in case we have to think at the same time of what is perceived both by the mobile eye and by the passive eye; just as we use the word *sight* (*Gesicht*) with reference to this sense in all its manifold applications. But in the preceding chapter, the term *field of fixation* (*Blickfeld*) was introduced to denote the field traversed by the gaze of the mobile eye. Accordingly, the field of fixation was considered as being a surface rigidly connected with the head and moving therewith, in which a certain point, the so-called *point of fixation* (*Blickpunkt oder Fixationspunkt*), as viewed by one eye or by both eyes, is imaged in the *fovea centralis*. The vertical directions up and down and the horizontal directions right and left in the field of fixation are taken so as to agree with the corresponding directions of the head. One point in the field of fixation is distinguished as being the point of fixation of the corresponding eye in its primary position. This point is called the *principal point of fixation* or *primary point of fixation* (*Hauptblickpunkt, primären Fixationspunkt*). The point exactly opposite behind the observer's head, at the other end of the diameter drawn to the principal point of fixation of the field, is the so-called *occipital point* previously defined [page 79]. The horizontal direction in the head from right to left, so far as our present purpose is concerned, may be defined by the line joining the pivots of the two eyeballs. The plane passed through this line and the principal point of fixation is the *horizontal meridian plane* of the field of fixation or the *primary position of the plane of fixation*. The other meridian planes of the field of fixation all pass through the line joining the centre of rotation of the eye with the principal point of fixation. The lines of intersection of the meridian planes with the imaginary surface of the field of fixation are the *meridians* of this field. When both eyes are used, we cannot speak of meridian planes except in case of the horizontal plane, although perhaps we may speak of meridian lines; because, since the field of fixation may be considered as being so exceedingly far away, there will be no appreciable difference in direction between a plane which passes through a point in the field of fixation and the visual axis of one eye and a plane which passes through the same point and the visual axis of the other eye.

Thus, when the head is moved, stationary external objects will assume different places in the field of fixation. The same place in the field of fixation will be imaged at different places in succession on the retina when the eye is moved. On the other hand, fixation of the same spot in the field of fixation inevitably involves always the same position of the eye in the head and the same contractions or elongations of the

various ocular muscles; and so we may conjecture that each place in the field of fixation is more or less exactly indicated by the special feeling of innervation (and by other sensations, which may be present in the adjacent parts of the eye), which is characteristic of the given position of the eye in the head.

For purposes of geometrical division, the field of fixation may be regarded as a sphere of infinite radius, like the celestial dome, with its centre at the pivot of the eye. The location of a point seen in the field is obtained by drawing a straight line through it and the centre of rotation of the eye and producing it to meet the imaginary surface of the field of fixation. The place where it intersects this surface is the *geometrical place* of the point as seen in the field of fixation, which will frequently have to be distinguished from the *apparent place* in the field where the visual object is projected by the eyesight.

The field of fixation is concerned with the mobile eye. We must make a distinction between it and the so-called *visual globe* (*Sehfeld*) of the eye. This latter is regarded as moving with the eye so that the image of every point on it remains constantly at the same definite place on the retina. At the conclusion of this chapter it will be shown that this place cannot be materially changed by altering the accommodation of the eye. Thus the visual globe is, so to speak, nothing but the retina itself with all its images and special characteristics projected outside. Accordingly, after-images, the vascular system, the blind spot and the yellow spot will always be projected on the same places of this field. And hence every point of the visual globe is indicated in the sensation by those local signs which are emblematic of the sensations of the corresponding places on the retina; and it was expressly stated above that the only way we have of indicating and describing either to ourselves or to others the local characteristic of the sensation of any fibre of the optic nerve is by signifying the place on the visual globe which corresponds to it.

However, as the point of fixation changes, the position of the visual globe itself may be altered with respect to the field of fixation. In order to establish definite directions on the visual globe, we start with the eyeball in its primary position. Then the horizontal meridian plane of the field of fixation will intersect the visual globe in a line which will be called here its *horizontal meridian* or the *retinal horizon*.[1] The meridian planes of the visual globe all intersect in the *principal line of sight*, that is, in the line of sight which goes to the point of fixation and which we may think of as coinciding with the *line of fixation* drawn

[1] ¶ See page 43. (J.P.C.S.)

through the point of fixation and the centre of rotation of the eye; since the centre of the pupil (see Vol. I, p. 22), like the visual axis, lies a little to the nasal side of the eye. The location of any object of vision on the visual globe will be determined by the line of sight drawn through the given point of the object and produced to meet the surface of the globe.

For dividing the visual globe geometrically and scientifically, the best way is to consider it as being a spherical shell concentric with the field of fixation. It is true we shall see subsequently that the *apparent* positions of the points on the visual globe do not correspond to the geometrical construction. And, therefore, it is necessary to distinguish between a *geometrical* and an *apparent place* on the visual globe also. The apparent place is the place determined by the eyesight.

As the eye moves, the spherical shell of the visual globe will be shifted with respect to the field of fixation. The position of the former can be found from the laws of the ocular movements as developed in the preceding chapter, provided we know the place in the field of fixation where the point of fixation is, whose position is fixed on the visual globe. Think of the primary position of the point of fixation and its temporary position as being connected by the arc of a great circle; then, *provided the movements of the eye are in accordance with* LISTING's *law, the horizontal meridian of the field of fixation and the retinal horizon of the visual globe must make equal angles with this circle.*

When the visual globe is shifted with respect to the field of fixation, the geometrical positions of the projections of the various points of the object do not remain absolutely unchanged on the spherical surface which is common to the field of fixation and the visual globe. In order to find the position on the visual globe, straight lines must be drawn to the points of the object from the point of intersection of the lines of sight. Now as this latter point is about 3 mm beyond the cornea and 12.9 mm in front of the centre of rotation, its position will vary as the eye turns, and thus the directions of the lines of sight will be slightly altered. However, this variation is comparatively very unimportant for object-points which are not too close to the eye. Calculation shows that the apparent displacements of objects for ocular movements not exceeding 10° are less than the imperfections of the images when the eye is accommodated for distant vision; and so, as a rule, considering the lack of precision of accommodation, they are generally negligible. Such displacements are not appreciable unless the objects are very close at hand, and the movements of the eye are extensive. For instance, when a lead pencil, whose thickness is about equal to the diameter of the pupil, is held close in front of the eye, so as to hide a flame com-

pletely, by turning the eye considerably to one side, it will be possible to perceive the flame by indirect vision. The blurred image of the near pencil is shifted so much in this case by the lateral movement of the eye that the object cannot longer hide the flame. This method is occasionally serviceable in ascertaining what can be recognized in indirect vision, because under such circumstances the object is not situated so as to be seen directly at all.

Provided, therefore, all the objects are far away that can be seen at the same time without appreciable indistinctness when the eye is accommodated for distant vision, the displacements of their projections in the field of fixation will be so small as to be negligible; and the geometrical places of the given objects in this field may be considered as being independent of the movements of the eye.

With the above proviso, the field of fixation is the external projection of a constant retinal image, whereas the visual globe is the projection of the retina itself. The two fields are shifted with reference to each other by the movements of the eye, exactly in the same way as the retinal image of the external objects and the retina itself are shifted. In the following discussion I prefer to consider the two surfaces that are outside the eye rather than the retina and the retinal image, because the former are a more correct expression of our actual consciousness, and because by directly referring all places to the two spherical fields we avoid the ambiguity which is responsible for so much that is erroneous here; whereas when we speak of knowing the positions of objects by the places on the retina that are affected by them, we seem to imply that we are aware of the retina and know something about its dimensions and extent. Incidentally, with respect to all constructions made on the spherical surfaces, it does not matter at all how big we take the radii, except that when the radius is finite, instead of drawing the lines of sight, we must draw lines parallel to them through the centre of rotation of the eye. Thus we can even make the radii of the spherical surfaces negative, and so construct the parts of the surfaces behind the centre of rotation, where the retina and the retinal image are situated. Such a spherical surface drawn in the region of the actual retina may be called an *ideal retina*, with an *ideal retinal image* lying on it. However, we must not suppose that the dimensions of such a schematic retina correspond with the real retina except by way of a very rough approximation. The form of the real retina is ellipsoidal, and, besides, the retinal image of the external scene which is projected on this surface is always very much distorted by the asymmetries of the refracting mechanism. Moreover, so far as vision is concerned, I myself am disposed to think that neither the size, form

and position of the real retina nor the distortions of the image projected on it matter at all, so long as the image is sharply delineated all over, and provided neither the form of the retina nor that of the image is appreciably changed during the progress of the observation. In the natural consciousness of the spectator the retina has no existence whatever. Neither ordinary sensation nor even scientific experimentation enables us to obtain any experience as to the dimensions, position or form of the retina of the living eye, except such as may be obtained from the optical image as projected outside by the ocular media. As a general thing, however, as compared with the external world, the retina is completely inverted by the ocular media, and, so far as the former is concerned, the latter has no existence, as we might say, except as it appears in its optical image. Now the visual globe, as defined above, is the representative of this optical image.

If with the eye in a fixed position there are two luminous points present on the visual globe, the light coming from them will stimulate two different fibres of the optic nerve, and there will be two sensations, necessarily differentiated from each other by characteristic local signs, since it is possible to distinguish them in the sensation. But we know beforehand just as little about the places on the retina to which these local signs relate as we do about where the nerve fibres are that conduct them or whereabouts in the brain they are transmitted. At most we might be able to deduce some conclusions from scientific investigations as to the places on the retina; but with respect to the part of the question concerning the optic nerve and brain we are still at present perfectly at sea. We may possibly know from daily experience how the arm has to be reached forth in order to touch this object or that in order to hide it from the eye. And so such movements enable us to find out directly the directions of objects on the visual globe; and thus we learn to connect the special local signs of the sensation directly with the place in this field where the object belongs. This is likewise the explanation of how it is that we see objects erect, although their retinal images are upside down. The retinal images have nothing whatever to do with the localization of objects. They exist simply for the purpose of concentrating the rays of each point in the field of view on a single nerve fibre. There would be just as much sense in wondering why the letters on a printed page are not inverted from right to left, because the type from which it is printed is inverted.

Hence it is more correct to say, "We perceive (*empfinden*) the place where an object appears to lie on the visual globe," than to say, "We perceive the place on the retina where its image is." The only sense in which the latter expression is right is when we mean to imply that

certain characteristics of the sensation, that is, its local signs, are peculiar to those sensations that are transmitted to us by a certain definite locality on the retina; and in a scientific investigation we should have to know too how to characterize the local relations of the sensation by the place on the retina where the light falls. However, the expression always creates the misunderstanding that somehow in natural vision we must have had innate knowledge of the real existence and position of the place on the retina; and as far as I can see, there is no basis for this statement whatever.

It was stated above that this connection between the local differences of the sensation and the direction in the visual globe is so exceptional that we have no means at all of describing the local definiteness of our sensations in our consciousness or of communicating it to others except by specifying the place on the visual globe to which the sensation is referred.

Having paved the way with these definitions, we may proceed now to investigate how far our ability extends of estimating relative dimensions in the field of view, and what illusions we are liable to have here. Every accurate comparison between two spatial dimensions such as lines, angles or surfaces in the field of view, is made with the help of ocular movements. Let us inquire first what can be accomplished by such means; and then we can see afterwards how such estimates will be altered when movements of the eyes are not allowed. I select this order because estimates made by moving the eyes, being found to be more accurate, were apparently earlier in use.

Experiments were made by FECHNER[1] and VOLKMANN as to the accuracy in comparing nearly equal distances in the field of view. FECHNER adjusted a pair of dividers at distances of 10, 20, 30, 40 and 50 half Paris decimal lines, and then tried to adjust by his eye the points of another pair of dividers at the same distances. In the experiment the two pairs of dividers, which were concealed except for their tips, were placed near each other on a table in front of him at the distance of distinct vision from the eye (one Paris foot). The error was noted after making each adjustment. VOLKMANN suspended three vertical threads near each other with weights attached to them. Their horizontal distances apart could be varied. He tried to adjust them by the eye at equal intervals apart. These latter varied between 10 and 240 mm; and his eye was placed 80 cm away. Without taking account of the directions of the errors which were made in each set of experi-

[1] FECHNER, *Psychophysik*. Bd. I. S. 211-236. See also other experiments by HEGELMAYER in VIERORDTS *Archiv*. XI, 844-853.

ments carried out under the same conditions, he added them all together and divided the sum by the number of trials. Thus he found the *mean error*, which in these experiments was always nearly the same fraction of the total length used in making the comparison. The magnitude of this mean error as found by taking the average of all observations, expressed as a fraction of the total length of the line used, was:

In FECHNER's experiments............	1/62.1
In VOLKMANN's first experiments......	1/88.0
In VOLKMANN's later experiments......	1/101.1

Accordingly, in these observations the psycho-physical law, which was proposed by WEBER and generalized by FECHNER, was found to be obeyed. It will be recalled that we learned about this law when we were investigating the connection between the intensity of the sensation of light and its objective brightness [Vol. II, page 175], and that we found that the discriminable differences in the magnitudes of the sensations were proportional to their total magnitudes.

VOLKMANN and one of his pupils carried out other experiments in which much smaller distances were used, which had to be measured by micrometers. The intervals were the distances between three fine parallel silver wires, each of which was 11 mm long and 0.445 mm thick. The intervals between them, which varied from 0.2 to 1.4 mm, could be regulated by micrometer screws. The experiment consisted in trying to adjust the wires by the eye at equal intervals apart. The errors in this case were no longer found to diminish in proportion to the size of the interval, but tended to approach a lower limit; as might have been anticipated, since with such small intervals it is necessary to take into account the accuracy with which the eye can discriminate between the smallest parts of the field of view, which will depend on the fineness of the elements of the retina. The mean error Δ may be represented, however, as being the sum of two terms, one of which is constant, and the other proportional to the distance D between the wires; as expressed by the following formula:

$$\Delta = v + WD,$$

where v and W denote two constants. When the eye was 340 mm away, the values of these constants were found to be as follows:

	v in mm	W
VOLKMANN for horizontal intervals	0.008210	1/79.1
" for vertical intervals.............	0.007319	1/45.1
APPEL for horizontal intervals..............	0.005331	1/164.5
APPEL, afterwards for the same intervals....	0.008548	1/85.3

The first two values of W in these results indicate that there is much more uncertainty in the comparison of vertical distances than in that of horizontal distances. Incidentally, the same thing will be noticed if we take a sheet of paper which is ruled with horizontal and vertical lines, and try to bisect the intervals by the eye, and then measure them on a scale. The errors made in dividing the vertical intervals in half will generally be larger than in the case of the horizontal intervals. In the comparison of two distances or two straight lines, it is found that small differences will not be noticed unless the point of fixation is brought first to the middle of one line and then to the middle of the other, so that images of the two lines fall on the same parts of the retina in succession. When the point of fixation is kept fixed, we are apt to say that two distances are equal, although the moment the direction of gaze is varied in the manner above stated, we can recognize that they are not equal.[1]

The comparison of vertical and horizontal linear dimensions with each other is much more difficult. In this case we find a constant error, owing to the fact that we are disposed to regard vertical lines as being longer than horizontal lines of the same length. The best way to see this is to hold a piece of paper perpendicular to the line of vision and try to draw a square on it by the eye. The height of the square is invariably made too low. In my own case the error amounts to between 1/30 or 1/60, the average being about 1/40, of the length of the base. However, this fraction appears to vary very much for different eyes. Wundt[2] states that it is one-fifth.

Volkmann[3] made experiments also on the size of the errors made in estimating the ratio between two unequal distances. A line was adjusted in between two others at a distance from one of them which was one, two, three, four or five tenths of the total interval between the outside lines. The difference which was found between the average of all the adjustments for a given value of the ratio and the actually correct adjustment was what Volkmann called the *constant error*; the differences between the separate adjustments and the average of them all being the so-called *variable errors*. The constant errors indicated that the interval on the left-hand side was always made somewhat too large as compared with that on the right-hand side. When the interval to be divided was the length of a Paris line [2.2558 mm], the average values of the constant errors, expressed in thousandths of a line, as obtained from each set of 40 experiments, were as follows:

[1] In regard to this matter see Note 1 at the end of this chapter.—K.

[2] *Vorlesungen über Menschen- und Tierseele.* S. 255.

[3] *Berichte der Kön. Sächs. Ges.*, August 7, 1858.

Constant Errors (from 40 trials in each case)

Starting from	Required ratio								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
The left....	13.4	19.8	6.7	11.7	3.4	13.4	24.8	10.0	6.8
The right...	−10.8	− 9.3	−20.0	−12.0	− 6.2	− 4.5	− 9.5	−19.7	−19.4
Below......	+ 2.9	+ 2.9	−12.1	− 5.9	−13.5	− 2.2	+ 7.2	+ 5.1	+11.6
Above......	− 5.0	− 4.7	− 6.0	+ 3.9	+ 9.7	+13.6	−17.4	− 7.3	−10.8

In the two upper lines of this table the distance to be divided was horizontal; in the two lower lines it was vertical. The starting point was the end of the line from which the estimate was made.

The variable errors were all added together without taking account of their signs and then divided by the number of observations. The average results were nearly the same for complementary ratios. The subjoined table shows their average values as obtained from sets of 160 measurements in each case (except for 0.5, in which case there were only 80 measurements):

Average values of the variable errors

Interval to be divided	Required ratio				
	0.1 and 0.9	0.2 and 0.8	0.3 and 0.7	0.4 and 0.6	0.5
Horizontal.......	6.73	4.36	3.01	2.64	1.11
Vertical.........	7.09	9.01	9.95	8.61	7.98

In another set of experiments where the total distance to be measured was 100 mm, and the limits of the given distances were shown by three fine human hairs hanging down from the scale, while the errors found were actually larger, they were relatively a little smaller. The distances are given in tenths of a millimetre, so that the unit is again a thousandth part of the total length.

Constant Error

Starting from	Required Ratio								
	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
The left....	2.35	7.45	0.5	10.7	4.15	12.4	11.3	0.85	4.10
The right...	− 1.8	+ 0.6	−11.1	− 5.2	− 4.0	− 7.5	− 5.5	− 4.4	− 2.8

Average value of the variable error.

For the ratios 0.1 and 0.9 = 2.6
" " " 0.2 " 0.8 = 5.6
" " " 0.3 " 0.7 = 7.9
" " " 0.4 " 0.6 = 6.5
" " " 0.5 = 2.8

In order to be able to tell not only whether two equal distances are equal, but also what is the ratio between two unequal distances, the lengths should be estimated between the two ends of the given distance that is used as measure of the distance. On a plane this latter line will be straight. In the field of fixation, which is an apparently curved surface, straight lines cannot be drawn; and even in order to draw shortest lines on the surface, we should need to have a more accurate idea of the curvature of the surface of the field of fixation than has been defined. If the field of fixation is imagined as being a spherical surface with its centre at the centre of rotation of the eye, as is usually done for purposes of precise geometrical requirements, we might suppose that objective lines which were really straight lines in the external world, and which, as being the shortest lines, would be projected on the spherical field of fixation as arcs of great circles, would necessarily appear in the field of view as lines without curvature. But this is not so, except under certain conditions.

When we consider a straight line, like the edge of a ruler, say, and try to determine by the eye whether it is really straight or curved, we find, as a result of the illusion mentioned in the last chapter, that our judgment will depend on the direction of the eye in the head. When the ruler is held horizontal and too low down, the edge seems to be concave upward; when it is held up too high, it appears to be concave downward. We perceive at once that there is an optical illusion here, by turning the ruler around, so that the edge which was above is now below. Then an edge that was really concave downward would now be concave upward, and *vice versa*. But when the ruler is right and straight, the optical illusion persists. However, by holding it so that the middle of the edge corresponds to the primary position, it will appear to be straight, provided it really is so. Now there certainly is a natural tendency for us to choose the primary position when a question of this sort has to be decided by the eye; and yet there is not much guarantee of our maintaining this adjustment. On the other hand, my experience is that I can recognize tolerably slight curvatures of rulers in the primary position, provided I turn the ruler over, and look first at one surface and then at the other. In this way, with an ivory ruler 200 mm long, which was convex, and whose curvature in the middle was bent out only 0.35 mm from the straight line, its radius of curvature, therefore, being about 14 m, I found I was able to recognize the curvature correctly by my eye; and also in case of another ruler which was concave and a half millimetre out in the middle. However, accurate determinations of this sort cannot be made without the aid of movements of the eye.

Moreover, we can tell with much precision whether straight lines are parallel to each other or not. In order to do this, we let the eye move to and fro along one of them or midway between them; and then we can tell pretty accurately whether they are just as far apart at one end as at the other, or whether they are farther apart. Similarly, also, we can tell with a relatively high degree of certainty that two angles are equal whose sides are parallel, because a small deviation of the sides from parallelism can be readily noticed; and hence we can infer that the angles are not equal. In E. MACH's experiments[1] it was found that the estimate of parallelism was more accurate for horizontal and vertical lines than it was for oblique lines. On the other hand, the comparison of equal angles whose sides are not parallel is not very sure, but is liable to constant errors that are fairly regular.[2]

Comparatively the simplest problem of this kind is to tell whether an angle is equal to its adjacent angle, that is, whether it is a right angle. Suppose two straight lines intersect each other at right angles, one being vertical and the other horizontal; to the right eye of most persons the upper angle on the right and the lower angle on the left will appear obtuse, and the other two angles acute. It is exactly opposite when the figure is viewed with the other eye. In making these tests care should be taken to adjust each eye in turn perpendicularly to the plane of the diagram and to focus the point where the lines cross. On the other hand, in trying to draw a vertical line to meet a given horizontal line, the upper end of it will deviate about a degree to the right when we use the left eye in drawing the line. Thus, the diagram in Fig. 21 represents what looks to my right eye as a rectangular cross made by the lines *ab* and *cd*; whereas the line-segments γ and δ show the position of the really correct vertical line. When I look at this figure with my left eye, the upper end of *cd* appears to me, on the contrary, to be too much inclined to the right.

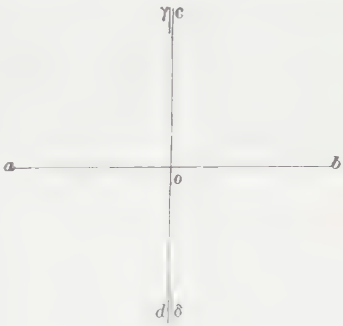

Fig. 21.

The amount of error made in estimating a right angle depends on the inclination of the sides of the angle to the retinal horizon. I see right angles correctly with my right eye when the upper end of one of

[1] *Sitzungsber. d. K. K. Akad. zu Wien.* 1861. Bd. XLIII, 215-224.

[2] ¶See E. GELLHORN, Über den Parallelitätseindruck. PFLÜGERS *Arch.*, 199 (1923), 278-289. (J. P. C. S.)

the legs is about 18° to the left of the vertical; and with the left eye, when this leg is about just as much to the right of the vertical. On the other hand, the difference is most when the legs of the angle are turned 45° away from the position above mentioned, in which case the angles lying to the right and left look about like angles of 92°, and those lying above and below like angles of 88°.

When one of the sides is horizontal, angles of 91.2° and 88.8° look to my eyes like right angles. In VOLKMANN's case[1] these angles were 91.1° and 90.6° for his left and right eyes, respectively. However, in his experiments he did not use a cross, but tried to place a single line first horizontal and then vertical, making 60 trials in each case.

I find also that surprisingly large errors are made when we take an angle of from 30° to 45° with one of its sides horizontal, and try to draw by the eye a third line through the vertex of the given angle, nearer the vertical, so as to make another angle equal to the first. We regularly make this angle much too large. In case the first angle is 30°, I am apt to make the second more than 34°, no matter which eye I use, or whether the vertex of the angle points to the right or to the left. But when the figure was turned around until the line last drawn was horizontal, it could be detected that the angle was too big.

Here too the fact may be mentioned that the angle of a correct equilateral triangle, which is opposite the horizontal base, invariably appears to be smaller than the angles at the base.[2]

If it is asked how it is possible for us to make comparisons between dimensions in space which belong to different parts of the visual globe, my own experiments mentioned above already indicate a method by which it can be done, in case the said dimensions are so situated that their images can be produced one after the other on the same part of the retina; preferably at the centre of the retina, so that their corresponding points fall on the same points of the retina in succession. As a matter of fact, this is the method used, for instance, in comparing the lengths of two parallel straight lines A and B by the eye. We look first at the middle of A, then at the middle of B, then again at A, and so on, and try to see whether we get exactly the same impression in both cases; that is, whether the same points on the retina are affected to the same extent by the images of the two lines. Evidently, here we do not need to know anything about the form and length of the image on the retina. The retina is like a pair of compasses whose points are placed on the ends of each of the two lines in succession, so as to see whether they are of the same length or not; but we do not have to

[1] *Physiologische Untersuchungen im Gebiete der Optik.* Leipzig 1864. Heft 2. S. 224, 225.

[2] With reference to this, see Note 2 at the end of this chapter.—K.

know anything about the distance between the points of the compasses or the form of the instrument, except that the adjustment is the same in both cases.

However, there is a difference between the two comparisons as made with the retina and as made with the compasses. The line connecting the points of the compasses can be turned in every direction. But, in consequence of the laws of ocular movements, it is not possible to do this in case of the line joining a pair of points on the retina; unless we are willing to resort to excessive movements of the head, which, since they involve more effort, cannot be long-continued or varied so often or so quickly. Even if this could be done, it would usually involve a fundamental alteration of the point of view where the eye was located in space, and that would modify the whole perspective outlook. Suppose a, b and α, β are two pairs of points in the field of view whose distances apart are to be compared; and suppose I look at a first, thus causing its image to be produced in the *fovea centralis* at A, while the image of b is formed at the point B somewhere else on the retina. Then if I turn my eye and look at α, so that its image will be in the fovea, for this new position of the visual axis the retinal point B will have a perfectly definite position, which I cannot vary arbitrarily without moving my entire head; and the line $\alpha\beta$ must have a perfectly definite direction in the field of view, in order for the image of the point β to be at B.

If a, b, α and β are all close enough to the principal point of fixation, for the portion of the field that contains them to be considered as being flat, the images of the lines ab and $\alpha\beta$ cannot be formed in succession on the same points of the retina unless the two lines are parallel. This is just the reason why it is possible to make an accurate comparison between the lengths of two parallel lines, whereas large errors are made in comparing the lengths of two lines when they are not parallel, even though they are close together.

Similarly, as stated above, we can readily tell whether two lines are parallel by observing that they are equally far apart everywhere; or whether two angles are equal by the fact that their sides are parallel.

Now if we have to decide whether a certain line in the field that passes through the principal point of fixation is a straight line, we can let the eye glide along it and cause the images of its various parts all to fall one after the other along the same line on the retina. In the previous chapter we saw that when an after-image was developed of a piece of a line passing through the principal point of fixation, and the gaze made to travel along the meridian where the linear element was, the after-image would remain constantly in that meridian. In those

experiments the after-images indicated the projections in the field of those places on the retina which had received the impression of the linear object; and the consequence of the experiment just mentioned is that all parts of any such meridian may be imaged in succession along the same row of points on the retina.

Thus, as the eye traverses such a meridian of the visual globe, the corresponding line of the retinal image will be shifted along the corresponding line of the retina itself, since they are both continually coincident and congruent; and the visual globe will be shifted in front of the eye with respect to the field of fixation in such fashion, that while the given meridian of the visual globe will be displaced along that of the field of fixation, it will always continue to coincide with it.

The same lines in the field of fixation whose images are shifted along themselves are likewise the *direction-circles* (*Direktionskreise oder Richtkreise*) mentioned in the previous chapter (page 79), which all pass through the *occipital point* of the field of fixation. It was proved there that if, in gazing at a point on one of these direction-circles, a linear after-image was congruent with its own direction, it would also be congruent with it at all other points. As the after-image is attached to the retina, this proves that the images of the portions of a direction-circle will continually lie on the same retinal line, when the eye is made to travel along one of these circles.

It was likewise noted at the same place above that an after-image of short length was congruent with the other direction-circles which all had a common tangent at the occipital point.

By virtue of these properties, the direction-circles have a peculiar importance for the eye. The straight line on a plane is distinguished from all other lines by the fact that every piece of it is congruent with every other piece, no matter how they may be superposed. The circle is the only other line besides the straight line that possesses this property of being congruent in every part with every other part, so that it can be shifted along itself. But two circular arcs of the same length and curvature have to be superposed on each other in a definite way in order to be congruent. Their two ends can be placed together, and yet the lines themselves will not necessarily coincide. It is this peculiar characteristic of straight lines which gives them so much importance as being measures of length. For we cannot use any line for this purpose, unless it is uniquely determined when its extremities are given, and unless every part of it can be made to coincide with every other part.

Now in the field of fixation there is only one species of lines which require only a direct act of sensation for us to tell whether they can

be shifted along themselves and are therefore congruent with themselves all over. As shown by the preceding investigation, on the assumption of LISTING's law, these lines are the *direction-circles.* It is true that there may be also other circles in the field of fixation which must be admitted to possess this same property, but we cannot prove it except by measurements and deductions, not by a direct act of sensation.

In case an eye does not obey LISTING's law in its movements, it will not necessarily have lines in it that can be shifted along themselves, when the eye traverses their entire length. But in every instance lines can be drawn, whose elements can be imaged in succession on the same linear element of the retina going through the *fovea centralis.* These lines will be called the *direction-lines* (*Richtlinien*) of the field of fixation. It is only when we assume LISTING's law of ocular movements that *all* these direction-lines of the field of fixation can be shifted along themselves, their after-images appearing always unchanged as the gaze travels along them. This is an essential characteristic of ocular movements that obey LISTING's law.

Straight lines of objective space appear as great circles in the spherical field of view. Great circles do not coincide with the direction-circles unless they go through the principal point of fixation (primary position of the line of fixation). In this case short pieces of them, as described in the experiments above, will appear as straight lines, but otherwise they will be curved, the apparent curvature being opposite to the real curvature of the direction-circles.

The direction-circles of direction-lines on the surface of the field of fixation must indeed be similar to the straight lines which are the lines of constant direction in the plane. We can use a short ruler to draw a line in a plane of any desired length simply by drawing a line at first as long as the ruler and then shifting the ruler along the line, and thus continually extending the line farther and farther. If the ruler is exactly straight, we obtain a straight line by this process; but if it is itself a little curved, we get a circle. In the field of view, instead of having a ruler that can be shifted, we have the central place where vision is most distinct, provided with a linear visual impression that may sometimes be augmented up to the after-image. We shift the gaze along this line, thereby shifting the line itself and indicating to ourselves the continuation of this direction. On a plane this process can be performed just as well with any rectilinear or curved ruler, but in the field of view only one single kind of line is possible for each direction of the eye and of the movement, such that it admits of being shifted continuously in its own direction.

Thus we see how certain measurements in the field of fixation are possible by virtue of the ocular movements and their fixed law. However, as was stated above, we find that even when the eye is perfectly stationary it is possible in indirect vision to make certain metrical estimates in indirect vision on the visual globe. Of course, they are far less positive than those made by direct vision with the mobile eye, for the very reason that indirect vision is not very accurate anyhow. But that we do have some capacity of this sort, is shown most strikingly by subjective phenomena which cannot generally be observed except by indirect vision. An instance of this kind is the vascular figure. We are enabled to draw this figure, and to perceive how it is distorted by varying the direction of the illumination, and we have somehow a definite superficial idea of it, although we cannot alter its position on the retina by moving the eye so as to look at each portion of it separately. Similarly, when the field is instantaneously illuminated by a flash of lightning of too brief duration for any appreciable movement of the eye to be made in it, it appears that we are enabled to judge correctly in the main of the forms of the objects that are presented to our vision in this way.

However, in this mode of vision also the judgment of the eye is liable to peculiar illusions, which are important mainly because they seem to give some indications as to the way in which we arrive at estimates of the field of indirect vision.

In the first place, those illusions mentioned above in regard to the comparison of angles whose sides were not parallel and of lines extending in different directions belong here, because, as we know by our own observation, movements of the eye do not and cannot contribute anything towards improving our judgment in these cases. The aforesaid illusions too are just as apt to occur when the eye gazes steadily at one point as when it wanders about.

But there is also another system of illusions, which have never been described, so far as I know, and which are connected with the lines on the visual globe, which apparently have no curvature, and with the apparent size of the peripheral parts of this field. Straight lines drawn on a plane are likewise the shortest lines and those that exhibit no curvature either to one side or to the other. But on the sphere they appear as great circles whose radii are perpendicular to the spherical surface, exhibiting no curvature on the surface of the sphere itself. On the other hand, all circles that are smaller than a great circle appear to be concave toward the side where the smaller portion of the sphere is, and convex toward the opposite side.

We may ask now, What are the uncurved lines on the visual globe?

Are they, as might probably be conjectured at first, the great circles of the imaginary spherical field? We can easily show that they are not always so.

Suppose we repeat the experiment with three stars which was described above [p. 67], but this time keeping the gaze fixed, whereas before the eye moved back and forth from one star to the nest. We must try to find three bright stars in the sky that are as nearly as possible on the arc of a great circle; which can be determined accurately enough by sighting the three stars over a piece of stretched thread. The stars should be chosen as far apart as possible, and they should be bright enough to be easily recognized even in indirect vision and to be distinguished from the smaller stars in their vicinity. Having selected the stars properly, look directly at the middle one. They will appear to form a straight line, or if they do not lie exactly on the arc of a great circle, the direction of the deviation and its approximate amount can be told correctly. But as soon as the point of fixation is shifted to some distance on one side or the other of the row of stars, then immediately and very distinctly the line will appear concave toward this point, the concavity being more and more pronounced, the farther the point of fixation is from the row of stars. This shows that, when the eye looks steadily in one direction, a great circle of the celestial sphere will not appear to be without curvature unless it passes through the point of fixation; otherwise, it will appear concave toward that point. A further consequence is that lines on the celestial sphere which, in the peripheral parts of the field, are said to be without curvature, must really be convex toward the point of fixation.

Of course, in the case of terrestrial objects the judgment formed by the eye is apt to be influenced by previous knowledge of the object as acquired by actual measurements, and yet even here we have the same illusion.

The best way to do is to bend far over and look down on the top of a large table, because under such circumstances it is not likely that any recognizable straight lines, toward which the gaze might be directed, will be in the field of view. Now look steadily at a point on the top of the table, and then try to arrange three bits of paper or some other bright objects along a straight line at some distance away from the point of fixation. Invariably, as soon as we look at the pieces of paper themselves, we find that they have been placed on an arc that is convex toward the previous point of fixation.

If a long strip of paper, with parallel edges about three inches apart, is laid on top of the same table, it will be noticed, on looking at the middle of it, that by indirect vision it appears to be narrower

at the ends than in the middle, and that it is apparently bounded by two arcs with their concavities toward each other.

In short stretches of straight lines the apparent curvature is generally not noticed, because we are disposed to regard and interpret them as being straight lines on material objects rather than as being great circles in the field of view.

Whereas great circles, which do not pass through the point of fixation itself, appear to be concave toward this point, on the contrary, circles which are parallel to a great circle going through the point of fixation appear to be convex with reference to that point. In order to test this, a strip of paper from three to five inches wide may be bent into the form of a semi-cylinder, and the eye placed on its axis. In looking at the middle of the strip, it seems to get wider toward the two ends and to be bounded by two arcs with their convexities turned toward each other. The lateral parts of the strip, being at the same distance from the eye as the middle portion, subtend geometrically the same visual angles as the middle portion, and yet on the visual globe these angles are apparently larger for the sides than they are for the middle.

Suppose that the point of fixation is on the horizon; and that there is a point above it at the height h, through which it is desired to draw a horizontal line which will appear uncurved in indirect vision. The great circle, which crosses the horizon at equal distances to the right and left and passes at the distance h below the observer's occipital point, will appear to be concave downward. A circle parallel to the horizon, which is really horizontal all over, and which is at the distance h *above* the occipital point, will not correspond to the requirement either, but convex downward. Since the first of these circles is concave, and the second convex, downward, the line which will appear uncurved must lie in between them; and if it is a circle, it must pass above or below the occipital point at a distance from it less than h. As we can think of direction-circles in the field of fixation which go through the occipital point itself, suppose we try them.

Accordingly, I have made a plane chart showing the projections of the direction-circles in the field of fixation which have the same directions as the vertical and horizontal lines going through the point of fixation. The projections are found to be hyperbolas in this case. In order to bring them out as distinctly as possible, even where they are seen indirectly, I have exhibited the fields of the pattern formed by the curves in black and white like the squares of a chess-board; as represented in Fig. 22 on a scale of three-sixteenths. The line A, reduced to the same scale, indicates the distance of the observer's eye,

which must be placed directly opposite the centre of the chart. He is supposed to gaze steadily at the centre. The original chart was hung on the wall of the room with its centre on a level with my eyes. A draughtsman's forty-five-degree right-triangle, the two sides of which were of the same length (20 cm) as the desired distance of the eye, was used for measuring. The distance was regulated by placing one of the legs of this triangle on the chart, while the vertex of the opposite angle rested against the outside angle of the eye.

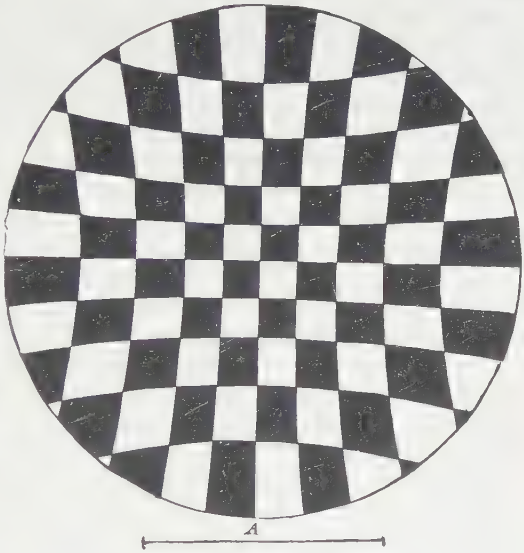

Fig. 22.

Now, indeed, under these circumstances, the direction-circles in the field of fixation projected as hyperbolas[1] will appear as straight lines, or at least as being uncurved lines on the surface of the visual globe. The several vertical and horizontal rows of black and white blocks will appear to be straight from one end to the other, and of equal width, as long as the centre of the figure is the steady point of fixation. But, of course, the curvature of the outer rows can be perceived by looking out toward them. In this case a peculiar illusion takes place. Thus, when I let my eye wander, I see the drawing curved like a flat bowl, the curvature of the hyperbolas seeming to be a curvature of the surface, and the lines in this curved surface being apparently great circles (or shortest lines). To some extent the distinction between direct and indirect vision is made to disappear by the conception we get in this way. Along the directions in the field of view itself the hyperbolas are apparently not curved, but the field itself appears curved.

[1] The equation of these hyperbolas is given by equation (5e), and the following equations in the previous chapter. Their distances apart, measured along the central horizontal and vertical lines, were chosen so as to make them correspond to equal visual angles

Accordingly, in this observation we must take care to keep the gaze steadily fastened on the centre of the figure. In case we are unable to dispel quickly the notion of its real form, the illusion will be aided by holding a lens close in front of the eye with its focal plane in the plane of the figure. It is true the peripheral portions of the figure will be somewhat distorted by the glass. When the rays are very oblique, the refraction of the lens tends to enhance the curvature of the hyperbolas. But the larger central portion of the pattern will be seen through the lens as if it were exceedingly far away, which is conducive to getting rid of the idea of its real material form.

The illusion succeeds best when we look steadily at the centre of the figure until an intense after-image is developed; and then turn toward a bright window and contemplate it with closed eyelids.

I continued these experiments by putting my eye at first more than 20 cm from the figure, in which case the hyperbolas on the right and left and above and below appeared to be curved; and then I gradually drew nearer until they became straight for my vision. Then I measured the distance of my eye with the triangle mentioned above. If I came still closer, the hyperbolas began to be curved apparently in the opposite direction from that in which they really were curved. And nearly always I found that the distance between my eye and the figure was 20 cm, when I looked at the horizontal lines and tried to see them straight; and it was practically the same for the central vertical rows also. On the other hand, as to the remoter vertical rows, particularly those on the outer side of the eye, I was disposed to select a position somewhat nearer the figure. At the distance of 20 cm, for which the pattern was designed, their actual curvature did not seem to disappear entirely.

Moreover, when the head was tilted so that the lines of the figure fell on oblique meridians of the retina, the phenomena were the same.

Accordingly, the conclusion is that, so far as the uncertainty of indirect vision and corresponding estimates by the eye will allow us to tell, the direction-lines in the field of fixation, as they would appear on the visual-globe if the principal point of fixation were constant, are the lines that are apparently uncurved; that is, they are the apparently shortest lines on the visual globe.

This special form of the shortest lines involves other consequences also with respect to the apparent form of the visual globe and the apparent dimensions of objects, as was previously remarked. Consider the horizontal meridian of the visual globe, and imagine a direction-line drawn horizontally 10° above the centre of it. The latter will coincide with that meridian at a place 180° behind the observer's head,

where it is tangent to it. But at a distance of 90° on the borders of the field the perpendicular distance of the direction-line will still be only 5° from the horizontal meridian; and since the two circles are apparently parallel lines on the visual globe, the perpendicular distance between them at the periphery, which is only 5°, looks just as large as it does in the centre, where it is 10°. And in the same way also at other places on the edge of the visual globe the dimensions of images parallel to this edge will appear to be comparatively too large.

The same thing is shown also in the following experiments. Stand at a place where there is a white door in a dark wall off to one side about 90° from the point of fixation, or where there is a dark tree outlined against the sky; and then see how high it looks in indirect vision. Now turn the eye and head directly toward the object, and it will appear to be much lower, whereas, on the contrary, its width will seem to be much greater. In the same way mountains on the edge of the field of view seem to be higher and steeper than they do in direct vision.

Again, place a piece of white paper on a dark floor, and look straight ahead horizontally, so that the paper is on the lower edge of the field. It will look relatively too wide from right to left, and will apparently contract the instant we look directly at it.

Thus, while the arcs parallel to the visual globe appear to be magnified, the peripheral portions of the lines running radially seem to be somewhat diminished. The hyperbolas in Fig. 22 are so constructed that as seen from the distance A the vertices of the horizontal hyperbolas and those of the vertical hyperbolas are each separated by the same visual angle of 10°. Hence, if the hyperbolas look like straight lines, the black and white fields should all be apparently equal squares. But this is not the case. The squares that are far above or below the centre are apparently not as high as they are wide. My experience is that in the case of the squares on the right and left, the lack of sufficient width is perhaps not quite so plain. But anyhow this comparison between the magnitudes of objects in direct and indirect vision is very imperfect.

A circular piece of coloured cardboard held against a contrasting background will appear, therefore, like an elliptical disc on the upper or lower edge of the visual globe, with its axis major horizontal. On the right or left edge of the field it will look less distinctly elliptical, with its axis major vertical.

Since the lateral portions of the visual globe look to us somewhat too high and too small, there is a tendency to consider them as being nearer and as being situated obliquely with respect to the visual axis.

Whenever we turn to look toward them, they seem to recede and to become more perpendicular to the line of fixation. This is an illusion that is very usual with me when I am looking at distant objects on the horizon or in the sky. Then the visual globe does not seem to me like a sphere with my eye at its centre, but it appears to be more concave than a sphere would be. Still I do not wish to be understood as implying that, when the eye is kept fixed, the monocular visual globe appears to have a decided form corresponding to any definite surface at all.

Indeed, the leading characteristics of the perception just described may be summed up in the following geometrical figure. In the first place, I think of the field of fixation as being a hollow sphere with the eye at its centre; and suppose that radii are drawn out from the centre to the various points of the object (so-called direction-lines of vision) and produced to meet the surface of the sphere. The image of the object as projected on this spherical envelope will be formed at the places where these lines meet the surface of the sphere. The object is supposed to be removed, and nothing but these images of it substituted on the surface of the spherical field of fixation. The eye gazes at the principal point of fixation. Opposite it is the occipital point. Then I say the eye beholds the object in the visual globe apparently with the same configuration as it would see it by correct geometrical projection if the images on the sphere were viewed from its occipital point. Or I can also say, the eye sees the objects in the field of view as they would look in a stereographic projection from the occipital point as centre, when the latter was viewed from this point. It is the same kind of projection as is always used in representing the terrestrial hemisphere on a map.

In fact, the direction-circles which appear to have no curvature on the visual sphere will all lie in planes passing through the occipital point, and, therefore, as seen from that place, they must be projected as rectilinear. Tangential dimensions along the periphery of the visual globe must appear to be relatively larger than stretches that are parallel to them in the centre of the field, because the former are nearer the eye than the latter. Besides, the visual globe of each eye, which in the geometrical sense embraces a horizontal angle of about 180°, seems indeed to be much narrower than this. For the farthest objects on the right and left which can be recognized in indirect vision, and which are connected by a straight line passing through the eye, nevertheless always appear to be situated in front of us, as if the direction lines of vision drawn to them made an obtuse or perhaps even a right angle with each other. Especially, on looking up at the

sky, where there are no terrestrial objects on the visual globe of known positions and dimensions, the bright field in front of us appears to have an angular diameter of about 90° horizontally, and even less than that vertically, where the eyebrows and cheeks tend to contract the field somewhat. We have the impression of looking at the external world from a certain depth in the head.

The geometrical picture as above described must be regarded merely as such. It includes the main features of the apparent configuration in the visual globe, but not all of them. The apparent contraction of dimensions near the periphery that extend radially from the principal point of fixation is particularly noticeable at the lower and upper edges of the field, but it is not represented in that picture. Equal radial segments would appear rather to be equally long all over the field, being measured by equal peripheral angles for the eye placed at the posterior point of the sphere, just as they are measured by equal central angles for the eye at the centre. For we must remember that equal angles inscribed in the sphere correspond to equal angles at the centre.

Moreover, in this mode of representation no account is taken of the apparent deviation of the vertical meridian and of the relation between vertical and horizontal dimensions.

The question to be considered next is, How do we come to have this method of gauging the visual globe?

According to the *intuition theory*, we were endowed with it from birth by means of certain organic contrivances, and hence it would be idle to seek for any further explanation from the phenomena of vision.

But the *empirical theory* will have to endeavour to find such an explanation. Without needing to know (as we saw in the preceding chapter) how the impression is localized, the law of the ocular movements was formulated as the result of the endeavour to show that the changes of impression produced by the movement of the eye were dependent on this movement and not on changes of the external object. In reality, as was stated above, the explanation of the eyesight may be developed to some extent along with the law of the movements of the eye, without keeping them so entirely separate and proceeding so methodically step by step, as we have been obliged to do here for the sake of clearness. The result will be practically the same in either case.

It was explained at the beginning of this chapter how we can ascertain, in the first place, by the aid of the movements of the eye, the sequence in which the objects and the retinal points corresponding to them, that are characterized by local signs, are arranged, on the

surface of the field of view in case of the former, and on the surface of the retina in case of the latter. All that remained to be done was to find out the origin of the definite dimensional relations.

Then we saw how by means of the law of ocular movements we could find out about certain so-called *direction-lines* in the field of fixation, which have the same direction throughout their whole extent and can be perceived as capable of being shifted along themselves.

Now when we perceive any object in indirect vision, and thus have received a limited impression of it on a peripheral part of the retina, and then turn the eye so as to look straight at it, we get afterwards an impression of the same object with the same apparent size on the centre of the retina; and thus we can gradually learn by experience when a certain peripheral impression is the same in quality and size as a central impression. As far as its accuracy extends, this renders it possible to learn to judge of objects by their form and apparent size even in indirect vision.

But besides the size and form, a comparison is made also between the direction of the object, first, as seen indirectly, and then as seen directly, with that of the first object that was seen directly; and thus we perceive which lines of the two objects are imaged on the same meridian of the retina. Undoubtedly, this comparison of position will necessarily prove to be somewhat different, according as we proceed from the primary position of the eye or from a secondary position, although LISTING's law in case of the emmetropic eye makes the sum of these differences as small as possible. But, taking the average of all cases, the result of the comparison will be the same as if the first object were in the mean position, that is, had been fixated in the primary position. Besides, it has already been expressly stated that the primary position is generally assumed by the eye as being the most convenient and most satisfactory for the orientation; and that we try to avoid movements involving rotation around the line of fixation. Thus we may learn by experience the directions in the peripheral portions of the visual globe that agree with the lines drawn through the point of fixation; and as a rule this agreement will decide the question, when the point of fixation is also the principal point of fixation; that is, *all the elements of one and the same direction-line on the visual globe will apparently be in the same direction, and all direction-lines that are tangent to the same meridian of the field at the occipital point will be in the same direction.*

However, this determination of lines having the same direction is in conflict with the determinations of apparent size as made by comparing the appearances of an object in direct vision and in indirect

vision. Lines that have the same direction, according to our definition of this concept, cannot intersect each other, for if they did, they would not appear to have the same direction at those places. They seem to us rather as being really parallel and at the same distance apart everywhere. However, as we saw above, a limitation is imposed by the fact that peripheral portions that are directed tangentially appear relatively too large.

The fact that in these comparisons we attach more importance to the agreement in the direction of lines than to the sizes of the objects is possibly because, when figures are vague and blurred, as they are to a great extent out toward the periphery of the visual globe, linear directions can be perceived fairly well and accurately, although the form and dimensions of the object are still far from being accurately perceived. When a fine black line is viewed under conditions where the accommodation cannot be used, and it looks like a blurred band of shadow, it would be idle to try to measure its width and almost as hard to determine its length; but still its direction can be compared quite accurately with that of a thread which is seen sharply in focus, by adjusting the latter parallel to the edge of the shadow or even just in the middle of it. Now the images in the lateral portions of the visual globe make about the same subjective impression, although for an entirely different reason, as images which are very much blurred on account of poor accommodation. Hence, the assumption seems to me admissible (and I believe it is verified by direct observation), that there is comparatively much more certainty in determining the directions of lines in the peripheral parts of the field than there is in determining the dimensions of the objects there. In my own case at least, it is much harder to decide what position I should take in order for the widths of the outer squares in the chess-board pattern in Fig. 22 to appear to be the same as those of the central ones, than it is to determine when the lines appear to be straight.

The reason why the direction-lines are apparently still a little curved at the extreme borders of the chess-board figure is because, in starting from the primary position, these places cannot be reached without turning the eye more sideways than we are in the habit of doing. In order to be able to reach them without extraordinary effort, the line of fixation for the centre of the chart needs to have been turned toward the opposite side. But for such a position of the eye the direction-lines of the visual globe at the given place on the periphery would really be less curved than the hyperbolas.

Owing to the limited extent of the central parts of the visual globe, where the vision is distinct, the curvature of the spherical surface

and of the direction-lines on it may be disregarded; and in this portion of the field direction-lines which go the same way may be considered as parallel straight lines. Here, too, the comparison of the form, size and position of objects must be in agreement when we view them first indirectly, and then directly. Here, therefore, it will also be possible to make a more accurate comparison between lengths viewed indirectly whereas the comparisons between such lengths in the peripheral parts of the field of view are much more uncertain and liable to error. Lengths which do not go the same way cannot be directly compared, however, even in the centre of the field, except by turning either the head or the object. This sort of comparison is necessarily far more imperfect than that made by turning the eye alone.

The facts stated above also show that, as a matter of fact, those lines and angles that have similar positions, and can therefore be made to coincide with the same retinal points, can be easily and satisfactorily compared with each other in size; whereas in comparing the relative dimensions of such figures as do not have similar positions, we find not only a considerable uncertainty but also certain uniformly constant errors. Of course, to a certain extent, we learn also to compare lines and angles which are not in similar positions, as, for example, the sides or angles of a square or of an equilateral triangle; either by having the objects before us and turning them around so as to see them in different positions, or by turning the head. But neither of these resources is so often available or can be so regularly repeated as the simple movements of the eye; and so naturally we are still very deficient in skill when it comes to comparing objects which are in dissimilar positions.

In case of an uncertain perception, our judgment is apt to be led astray by other causes that affect it. We shall see that the illusion as to the size of the right angle has an entirely special connection with binocular vision, and so in different individuals with normal vision it occurs to about the same extent. On the other hand the illusion which causes vertical lines to look longer than horizontal ones is found to be very different for different persons. In my own case I find my judgment here is very variable and very uncertain. A circumstance that may have something to do with it is that most figures, which are such that either our position changes with respect to them or their position can be changed with respect to us, so that the images of their differently oriented lines and angles can be focused in succession on the same parts of the retina, are figures which can be drawn on the floor or on flat surfaces like books that can be held in the hand with

their lower end nearer the eye than the upper end. The reason for holding them in this way will be explained in the theory of the horopter. But, as a matter of fact, for such positions of the lines, vertical lines appear always to be foreshortened, so that we have a tendency to consider them as being longer than their apparent size warrants.

Incidentally, it is obvious also that as soon as it has been settled (no matter why), that a certain meridian is vertical, and that a certain linear ratio between vertical and horizontal lines is equal to unity, the apparent position of every other point on the visual globe will likewise be determinate.

Let us restrict the discussion to the central part of the visual globe, which may be considered as approximately plane. Then the geometrical position of the point may be supposed to be defined by rectangular coördinates. In Fig. 23 let AB be the horizontal corresponding to the retinal horizon; and let AC be a vertical line, the point of fixation being at A.

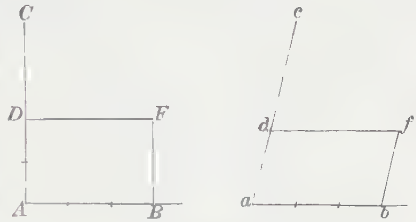

Fig. 23.

On the visual globe ab represents the position corresponding to the retinal horizon, and ac that corresponding to the vertical meridian. Suppose that the point F on the geometrical visual globe is two units of length from the axis AB, and three units from the axis AC. Lay off on ab three units of length equal to those of AB, and on ac the line ad which is *apparently just as long* as two units of AC; and complete the parallelogram $abfd$. Then f will be the apparent position of F, for by the construction all the various lines and angles in the two figures must be apparently equal to each other.

Thus, according to the proposed theory, as is actually also the case, the apparent positions of points in the central part of the visual globe, where vision is distinct, and where the field can be regarded as being plane, can be deduced from the geometrical theory, provided we transfer the points from a rectangular system of coördinates to an oblique system with axes in a different relation. But we know from analytic geometry that in such cases a definite direction of the axes of a rectangular system can always be found for making the transformation by merely shortening or lengthening the coördinates parallel to one of the axes in a definite ratio. The angles and connections between the axes that are to be used in these transformations have been already given above.

I must add here that the actual relations, as here described, do not agree with two other theories which have been proposed for the ocular mensuration of the visual globe. Some physiologists have accepted J. MÜLLER's theory, that the retina has the faculty of perceiving its own dimensions in space. In this case the tangential lines near the periphery would not have to be apparently too large, as they are, but rather apparently too small, since, as shown by the cross section of the eye, as represented in Fig. 2 of Vol. I, the retina is considerably narrower toward its posterior edge in the *ora serrata*(*gg*) than a hemisphere would be which was described around the nodal point. It is not easy to tell how it would be with the radial dimensions on this assumption, because the refraction of the rays at such oblique incidence and the position of the retinal image cannot be exactly determined.

Another theory, which has been used for explaining the ocular mensuration of the visual globe, was derived by several physiologists from E. H. WEBER's experiments on the sensation circles of the skin and the retina, although I must say it hardly seems to me to be that author's meaning.[1] According to it, the smallest perceptible differences of space were to be used as units for measuring areas. The only way to perceive a difference of space between two impressions (as was explained in Vol. II, p. 31) is when in between two stimulated elements of surface there is one that is not stimulated or that is differently stimulated, which can be perceived. The dimensions of the least discriminable elements of surface are very different for different parts of the retina, as has been proved not only by WEBER, but by AUBERT and FÖRSTER, and are very different also at different places on the skin; and therefore the distance between the stimulated points would have to be taken very differently at different places, in order to distinguish them as separate points. Thus when the two points of a pair of dividers are placed on a spot of the skin where their distance apart is less than the least perceptible distance, the impressions are fused together, and we have the impression that only one point is pricking us. If the points of the dividers are applied to a place where the discrimination between them is only vague, there certainly is a tendency to regard them as being nearer together than they really are. Lastly, when the points are applied to a place where the discrimination is very delicate, and where it is easy to recognize that the points are separated, it is my experience at any rate that the real distance between them is correctly estimated. Thus, for example,

[1] E. H. WEBER, Über den Raumsinn und die Empfindungskreise in der Haut und im Auge. *Berichte der Sächs. Ges.* 1852. S. 85-164.

in my own case, when I apply the points of a pair of dividers, which are four "lines" apart, to the end of my tongue or to the tip of one of my fingers or to my lips, the interval appears to be the same in each instance; and yet a distance of half a "line" is perceptible on the tongue, whereas the perceptible intervals on the finger-tips and lips are only one and two "lines," respectively. On the other hand, on the chin, and below it where discrimination of the given interval of four "lines" is difficult and uncertain, the points seem to me, when I am able to distinguish them at all, as being perhaps somewhat nearer together than they really are; in accordance with the general law of sensation, that distinctly perceptible differences are apparently larger than those that are vaguely perceived. But yet on my throat, provided I am still able to distinguish the interval at all, it never does seem to me as small, as when the actual interval is just half a "line" or a whole "line" and applied to the tip of the tongue. Thus the smallest perceptible magnitudes are not by any means apparently the same at different places on the skin, but are apparently different.

It is the same way with the retina. Consider two small black dots, each of diameter 2 mm, separated by an interval of 2 mm. If, viewing them indirectly, I try to find a place where they just begin to be visible, they do not seem at all nearer to each other there than they really are; and even when the interval is apparently greatest, they do not seem to be as close as two points do that are just on the border of differentiation when their images are focused in the centre of the retina.

Consequently, we have no right to extend Weber's theory of circles of sensation, by ascribing the same apparent size to these circles everywhere, and employing them as elementary units of measurements of space. So far as the eye is concerned, the consequence of this assumption would indeed be that the entire periphery of the visual globe would necessarily appear to be relatively much smaller in all its dimensions than objects of equal angular diameter in the centre of the field. On the contrary, we have seen that the tangential directions are apparently magnified, while the radial directions, at least at the upper and lower edges of the field, are certainly diminished in appearance.

There is no conflict here whatever with the fact that in estimating very small distances by the eye, which cannot be determined with sufficient accuracy by the eyesight aided by the ocular movements, the circles of sensation are utilized, as above stated. Incidentally, in connection with the phenomena of the blind spot to be considered presently, we shall have occasion to refer to these questions again.

In addition to the general illusions as to the relative dimensions of the visual globe, which are dependent on the law of the ocular

movements and on the way in which we learn about this field, as they have been described here, there is a series of illusions which depend on the special peculiarities of the figures under consideration, and which are likewise interesting, because they enable us to see more or less clearly the motives that guide us in estimating magnitudes and forms on the visual globe.

Most of these phenomena can be explained by the rule that was given in the case of contrast effects, namely, that in all perceptions of the senses distinctly perceptible differences appear to be larger than differences of the same objective size which are only vaguely perceived. One of the first consequences of this rule is that a graduated division of a dimension in space is easily supposed to be larger than one that is not thus divided, because the direct perception of the divisions enables us to see that the given magnitude contains so many divisions of such size more distinctly than we can do when they are not thus perceptibly marked off. Thus in case of the line shown in Fig. 24, there is no difficulty in supposing that the segment *ab* is just the same length as *bc*, although as a matter of fact *ab* is longer. A series of measurements of this type of illusion was made by A. KUNDT.[1] Five steel points A, B, C, D, E protruded from behind a screen, their distances apart being as follows: $AB = 20.2$ mm, $BC = 40.2$ mm, $AE = 241.9$ mm. The point D was adjusted in the middle by the eye. If it really were in the middle, the distance CD would have to be 60.55 mm. But for 120 trials made by one observer this distance was on the average put at 57.87 mm; that is, the apparent middle point was 2.68 mm nearer the side where the points A, B, C were situated. In the case of another observer for the same number of trials, the difference on the average was 3.95 mm. In all the tests the distance of the point D from the nodal point of the eye was 338 mm.[2]

Fig. 24.

It should be noted that in these experiments in bisecting a line there is a tendency for the right eye to make the right half too long, and for the left eye to make the left half too long. The first observer made the half corresponding to the eye that he used 2.24 mm longer than the other half; and the second observer made it 4.77 mm longer.

In the above experiments the distances under comparison could be made to fall on the same points of the retina. The illusions are much more striking when the distances have different directions.

[1] POGGENDORFFS *Annalen*. CXX. S. 118.

[2] See W. G. SMITH, D. KENNEDY-FRASER, and W. NICOLSON, The influence of margins on the process of bisection; additional experiments, etc. *Brit. J. of Psychol.*, 5 (1912), [illegible] — F. [illegible], Untersuchungen uber psychologisch-physiologische Bisektionsfehler. *Zft. f. Instrumentenk.*, 44 (1924), 61-78 and 155-172. (J. P. C. S.)

Consider A and B in Fig. 25. The two areas shown by sets of parallel lines are correctly drawn squares. According to the illusion above mentioned, both should appear to be taller than they are wide. This is decidedly the case with A; whereas B looks too wide.

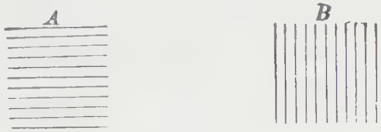

Fig. 25.

The same thing is true with regard to angles. Thus, in Fig. 26 the angles 1, 2, 3, 4 are all right angles; but, apparently, 1 and 2 are acute, and 3 and 4 obtuse. The illusion is stronger still if we look at the figure with the right eye only. On the other hand, owing to the deviation of the vertical meridian mentioned above, 1 and 2 should appear to be obtuse when viewed with the left eye; whereas they actually appear to be just about right angles, as they really are. If the figure is turned so that 2 and 3 are down below, then, on the contrary, 1 and 2 will look decidedly acute to the left eye, but correct to the right eye. Thus the divided angles invariably appear to be comparatively larger than they would appear if they were not divided.

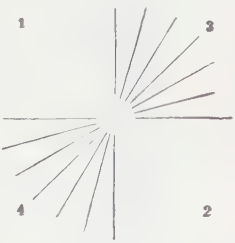

Fig. 26.

Two equilateral triangles are represented in Fig. 27. A is divided horizontally, and is apparently much too high; as would be the case also without the lines of division. In B, on the other hand, the angle at the right corner of the base seems larger than the angle on the left, and the vertex of the triangle seems to be shifted too much to the right. There are numerous illustrations of the same effect in everyday life. An empty room looks smaller than one that is furnished; and a wall covered with a paper pattern looks larger than one painted uniformly in one colour. Ladies' frocks with cross stripes on them make the figure look taller. A familiar parlour game consists in asking each person present to indicate on the wall how high from the floor a gentleman's top hat is. Usually, he will make it half as high again.

Fig. 27.

A fact observed by Bravais[1] probably belongs here also. He reports, that when an observer is on the ocean at a certain distance

[1] Fechner, *Zentralblatt*, 374–379; 558–561.

from a very irregular coast, and tries to sketch it as it looks to the eye, a mathematical comparison will show that, while he has represented the horizontal linear dimensions in their proper relations, the vertical angles have been estimated on a scale twice too large. This illusion, to which we are subject unconsciously in estimates of this kind, is not individual, as might be supposed. On the contrary, numerous observations show how prevalent it is with everybody. Various optical illusions that have recently become familiar are similar to the above.

Consider A in Fig. 28. Apparently, it is not d that is the continuation of the straight line a, as it really is, but rather f situated a little below it. This illusion is still more striking when the figure is drawn on a smaller scale, as at B, where the two pieces of the fine line are

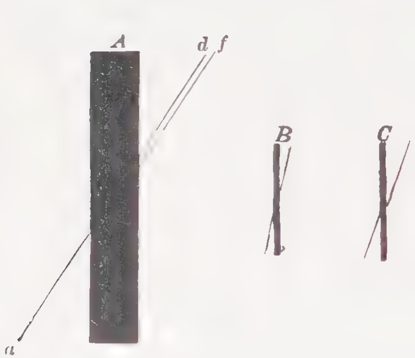

Fig. 28.

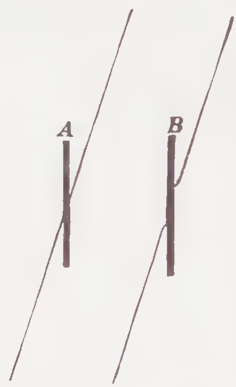

Fig. 29.

really prolongations of each other, although they do not appear to be so; and at C, where they appear to be prolongations, but are not so. If figures like A are drawn without the line d and viewed from distances that are farther and farther away (the accommodation of the eye being improved by glasses, if necessary), their apparent sizes will become less and less, and it will be found that f has to be drawn lower and lower to make it look like the prolongation of a, the farther away the figure is and the smaller its apparent size.

If the fine lines are made long, as in A of Fig. 29, it will be noticed that they seem to be bent in in the vicinity of the broader black line (as I have indicated rather too much in B), so that the farther ends of the fine line appear quite correctly to be prolongations of each other. If it were not for those kinks near the places where the heavy black line crosses the thin one, we should not get the appearance of the continuation of the thin line.

Now these are precisely the phenomena that must be produced by irradiation in this case; and it is hard to decide how much of the effect is due simply to this cause, and how much of it is the result of illusions partly of the kind here mentioned, and partly of the kind still to be described. It was explained in Volume II, pages 191-192, that there is irradiation also of black lines on a white ground. Near the vertex of the two acute angles the blur circles of the two black lines overlap and reinforce each other. Thus the maximum of darkness in the retinal image of the fine line is shifted nearer the broad band and appears to be bent toward it. But in the case of figures of the same kind drawn on a larger scale, such as *A* in Fig. 28, irradiation can hardly be the sole explanation.

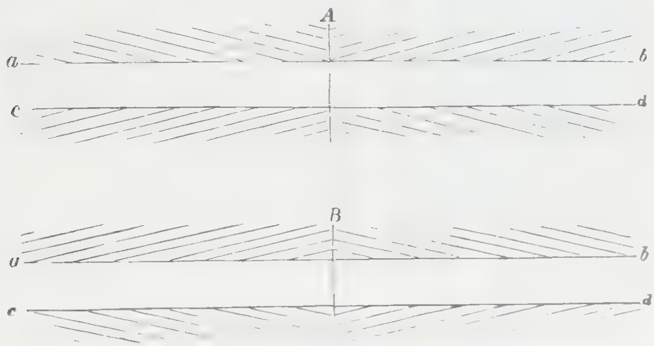

Fig. 30.

The illustrations shown in Fig. 30 are due to HERING. The lines *ab* and *cd* are parallel straight lines in both *A* and *B*. But in *A* they are apparently bent outwards, and in *B* inwards.

But the most striking diagram of all is that shown in Fig. 31, published by ZÖLLNER. The vertical black bands in this figure are all parallel, but they look convergent and divergent. Apparently, they always deviate from the vertical in the opposite direction from that of the short oblique lines that cross them. Here the two halves of each oblique line are shifted with respect to each other in the same way as the two halves of the fine line in Fig. 28. If the figure is turned so that the broad vertical bands appear to be about 45° from the horizontal, the apparent convergence is more noticeable still; on the other hand, the apparent shifting of the halves of the short lines that are then horizontal and vertical is less marked. Thus on the whole there is less change of direction in the vertical and horizontal lines than in those that cross the field of view obliquely.

The illusions last described may be considered as new illustrations of the rule above given, that as a general thing acute angles, composed of smaller angles distinctly marked, seem to be relatively too large when they are compared with obtuse angles or right angles that are not thus divided. Now if the apparent magnification of an acute angle is such that its two sides are apparently bent outward, the illusions represented in Figs. 28, 30 and 31 must be produced. In Fig. 28 the light lines would apparently turn around the points where they enter the thick bands, and then their two halves would not be prolongations of each other. In Fig. 30 the halves of each of the two straight lines are apparently always shifted so as to increase the acute angles between them and the oblique lines. Apparently, it is the same way with the vertical bands in Fig. 31.

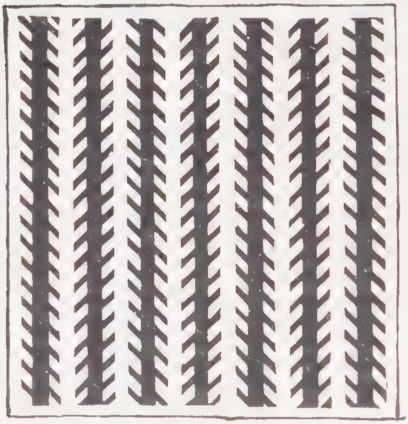

Fig. 31.

But in the cases of Figs. 30 and 31, under ordinary conditions, the causes assigned are responsible for only a small part of the effect; and the greater part of it is due to ocular movements, as I have discovered. These illusions disappear altogether, or only faint traces of them are left, when I look steadily at a point in the figure, as I should have to do to develop an after-image; and when I succeed in getting a well defined after-image, as can be done best with Zöllner's pattern (Fig. 31), there is no trace of the illusion any longer perceptible in it.

In Fig. 28 movement of the eye has no distinct influence on heightening the illusion. On the contrary, the illusion disappears, provided my eye moves along the light line. Conversely, on the other hand, the illusion in case of Fig. 30 disappears comparatively easily as the result of fixation; but not so readily in case of Fig. 31. Yet even in case of this last figure, I can get rid of it by looking steadily at it and not considering the black bands on a white ground as the object, but trying to imagine the white intervals as being branches with little leaves, lying on a black ground. But then the moment my gaze begins to wander over the pattern, the illusion recurs in its full strength.

With these figures also the illusion can be entirely or almost entirely avoided by first covering the drawing with a piece of opaque

paper, and holding the point of a needle steadily above it as a point of fixation; and then, while looking straight at this needle, removing the paper between it and the drawing. It is possible to tell, by the clearness of the after-image formed in this way, whether the point of the needle has been sharply fixated or not.

The surest and easiest method of getting rid of the effect of ocular movements is by illumination by an electric spark, because this lasts such an exceedingly short time that the eye cannot make any appreciable movement. For this purpose I used a hollow box *ABCD*, Fig. 32, painted black inside. Two holes were bored in opposite sides of the box, at the same distance apart as the distance between the eyes,[1] one pair at *f* in the anterior wall, and the other pair at *g* in the posterior wall. The observer looks through the holes at *f*, the drawing being fastened inside in front of *g*. A hole in the chart was pierced with a needle, and this hole was visible and could be fixated even without the electric spark in the otherwise perfectly dark box. The box, from which the bottom was removed, rested on a table. When the pattern had to be changed, the box was turned over and the chart taken out. The room was made moderately dark, so that while the observer could still see and manipulate the electrical apparatus, nothing was visible inside the box except the hole made by the needle. The wires for conducting the current are shown at *h* and *i*. The place where contact was made and broken is shown at *k*. A strip of cardboard is represented at *l*; it was white on the side next the spark, so that while it prevented the light of the latter from getting to the observer's eye, it reflected this light on to the drawing. The sparks were produced from the secondary spiral of a large RUHMKORFF induction coil connected with the terminals of a Leyden jar. The contact in the primary coil was made or broken by hand.[2]

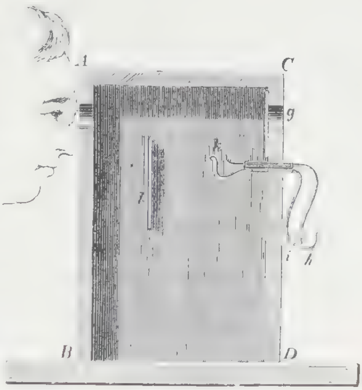

Fig. 32.

[1] The reason for having a pair of holes in each side was because the apparatus was intended to be used especially for stereoscopic experiments also.

[2] In case a sufficiently powerful electrical apparatus is not available, the *tachistoscope* made by VOLKMANN (*Leipziger Sitzungsber.* 1850, pp. 90-98) will answer the purpose. A falling screen opens one of the apertures or both of them, for an instant, enabling the observer to look through.

With electrical illumination the illusion in Fig. 28 was found to be just the same; but it disappeared completely in case of the diagrams in Fig. 30, and, while it did not always disappear completely in case of Fig. 31, it was much less evident when it did occur, and more doubtful than otherwise. The electric spark illuminated the objects quite enough to enable their forms to be plainly discerned.

Thus there are two different phenomena to be explained: First, the fact that the illusion is not so strong when movements of the eyes are avoided, and, second, the fact that the illusion is heightened by movements of the eye. As to the first point, I think the law of contrast is a sufficient explanation; that is, the law according to which a distinctly perceptible difference looks bigger than one that is not so distinct. The thing that is most distinctly perceptible in indirect vision is the agreement in direction between similar dimensions in space. The deviation of the side of an acute or obtuse angle from the direction of the other side is easier to perceive at the vertex than its deviation from the perpendicular to the other side, when this perpendicular is not drawn. And so the difference of an angle from 0° or from 180° appears to be too large as compared with the difference of an angle from 90°; an acute angle therefore being too large, an obtuse angle too small. This apparent magnification of the angle being distributed between the two sides, the illusion is produced of apparent displacements and changes of direction of the sides. Apparent displacements of lines in which they continue parallel to their real directions are hard to correct; and that is why the illusion in Fig. 28, comparatively speaking, persists so stubbornly. On the other hand, changes of direction are easier to perceive by looking at the figure more carefully provided we can succeed in producing apparent dissimilarity between lines that are similar in direction; and perhaps it would not be possible to fail to see the similarity between the lines in Figs. 30 and 31, which appear to be changed, were it not for the large number of lines that cross them obliquely and make them look unlike each other.[1]

We have still to see what is the effect of movement on the apparent direction of lines seen by the eyes. It is easy to show that it does have an effect even on the appearance of simple straight lines, when the direction of the motion makes an acute angle with that of the line. There is a prevailing tendency in moving our eyes to follow the direction of the more conspicuous lines in the field of view; and therefore in these experiments it is necessary to guide the point of fixation and to keep it steadily in mind, by continually focusing the eyes on

[1] As to this whole class of optical illusions, see Note 3 at the end of the chapter.—K.

the sharp point of a needle, while the latter is caused to pass over the drawing.

Draw a long straight line A on a sheet of paper, and move the mark of fixation along a second straight line B which crosses the first line at a very small angle. It is not necessary for the second line to be drawn on the paper; still it does no harm to make it actually visible. If the tip of the needle is followed by the eye, the straight line A will appear to move on the paper either toward it or away from it, depending on whether the mark approaches the line or recedes from it. Under these circumstances the image of the line A is shifted on the retina not only parallel to itself but also at right angles. The former movement will scarcely be noticed at all, provided the line is long and has no specially conspicuous mark on it; whereas the other movement perpendicular to its length is all the more distinct.

In this case even the direction of the line A is apparently altered, the angle between it and the line B, along which the point of the needle travels, being apparently magnified. The best way to see this is by drawing a straight line ab (Fig. 33), and placing one point of a pair

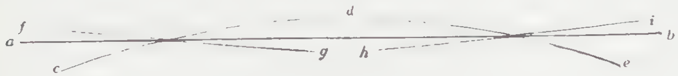

Fig. 33.

of dividers on the paper so that the other point can move back and forth along the arc cde. On following this moving point with the eye, the line ab will appear to move downward, while the point of the pair of dividers proceeds from c to d, and upward, when it goes from d to e. At the same time the entire line ab apparently has a direction like fg, while the eye, following the moving point, traverses cd; and a direction like hi, when the motion is between d and e. In going through the highest part of the arc in the movement from c to e, the direction of the line ab will be distinctly changed.

When the point of a needle is made to traverse Zöllner's pattern (Fig. 31) horizontally from right to left, its motion being followed by the eye, the figure seems to be in the strangest state of unrest. The first, third, and fifth black bands ascend, while the second, fourth and sixth descend; or it is just the opposite, when the direction of the motion is reversed. The ascending portions in this case are apparently not parallel to the descending portions, but are inclined in opposite ways not only to each other, but also to the plane of the drawing; the ascending portions being inclined with their upper ends opposite to the direction in which the point of the needle is travelling, and the descend-

ing portions with their upper ends in the same direction as that of the motion; and hence the characteristic illusion produced by this figure is manifested in particularly striking fashion by this apparent motion.

In order to see the apparent motion quite plainly, the point of the needle should be made to move with a certain average velocity, neither too great nor too small, the gaze being kept steadily on it. If this method does not succeed, the point of the needle can be kept fixed with the eye steadfastly focused upon it, and the drawing itself moved behind it. The cause of the apparent motion is evidently the same as in the experiment with the single straight line above described. We approach the oblique cross lines in an inclined direction, and they appear therefore to move, and in so doing they take with them, so to speak, the vertical black bands with which they are fused. Now if the vertical band we are approaching exhibits a vertical movement upward, the appearance is similar to what it would be if we approached it, not perpendicularly, but at an acute angle whose vertex was pointed downward; and, conversely, in case of the descending bands, the apparent motion is the same as if we approached them at an acute angle whose vertex was pointed upward. But since the direction of the actual motion of the eye is the same for all the bands, the latter appear to be inclined opposite to the line of motion of the eye; the ascending bands with their upper ends opposite the direction of this motion, and the descending bands following it; as represented in Fig. 34, where *ab* indicates the direction of the movement of the eye, and *cc*, *dd*, *ee* and *ff* are the apparent positions of the vertical bands, their divergence being exaggerated. The arrows on these latter lines show the directions in which lines so situated would appear to move, as the eye travels in the direction of the horizontal arrow.

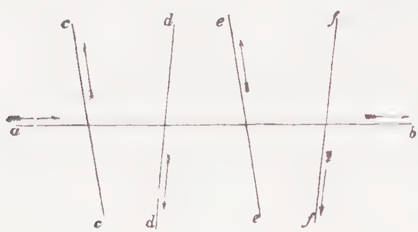

Fig. 34.

If the fixation mark followed by the eye is gradually made to move more slowly, the apparent motion will also get slower and less easy to see. Still by watching closely it can be perceived, and at the same time I find that the apparent divergence of the vertical bands is less definitely shown. Without the aid of a moving point to guide the eye, neither the apparent motion of the bands nor their apparent divergence comes out so beautifully as it does with this contrivance. Probably this

is because we are not able to make the eye move so uniformly and so straight over a drawing that has such prominent systems of lines. Incidentally, since the illusion as to the direction of the bands increases and decreases along with the illusion as to their motion, there is no doubt in my mind that the enhancement of the illusion produced by ordinary movements of the eye may be explained in the same way.

If the point of the needle used for the point of fixation of the eye is moved over the drawing parallel to the vertical bands, not only is the illusion not enhanced, but it is diminished in fact, if not utterly abolished. In this case the vertical bands are shown to be parallel direction-lines in the field of fixation, because their retinal images are shifted along themselves.

Incidentally, this effect, which the apparent movement of the vertical bands has on the apparent size of the angle between them, and the influence of the direction of movement of the eye, can be illustrated perfectly with an actual body in motion. Place a divided scale horizontally on a sheet of paper. Close to it put one point of a pair of wide-open dividers, and move the other point to and fro just above the edge of the scale. Then it will move exactly at right angles to the direction of the scale. If now the scale is moved also to and fro in its own direction, the line along which the point of the pair of dividers travels will no longer appear to be perpendicular to the direction of the scale, but very much inclined to it; as indeed it would really be represented by a system of coördinates which was rigidly attached to the scale; and yet, with reference to an absolutely fixed system of coördinates, the movement continues to be perpendicular to the edge of the scale. Incidentally, the change of the angle in this case is much more considerable than in ZOLLNER's pattern (Fig. 31), because in the latter the apparent change of position can never proceed so far as to make the shifted bands come together or in fact cross each other, because this would be too much at variance with the image in indirect vision.

HERING's diagrams (Fig. 30) give the same relations, only not in so marked a degree. Up and down movements of the eye tend to enhance the illusion in these figures, whereas lateral movements impair it.[1]

Doubtless, it may seem strange that causes which are apparently so different are allowed to act together to produce the same illusions. But when it is recalled that, according to the view adopted here, our

[1] Concerning the significance of the ocular movements for the perception of space-relations, see the extended discussion of this subject in Note 1 at the end of §29.—K.

information as to measurements on the visual globe obtained by indirect vision is dependent on the memory of former experiences obtained by movements, and that similar new impressions recur when the eye is moved, it follows that the two causes are not so different as they seem to be in the explanation. They are different simply in the same way as memory is different from the present apperception of analogous relations.

By virtue of these relations, a kind of *contrast* exists for the directions of lines and for distances, the effect being similar to that for luminosities and colours as described in §24. When the directions are nearly the same, the differences are apparently magnified. The effect of letting one line be crossed by another one at an angle or by a number of such lines, seems to be that the first line is bent away from the intersecting line or lines. On YOUNG's theory, the phenomena of contrast of luminosities and colours could be explained as being due to a comparison between stimulations of the fibres, which, while they were different in degree, were the same in quality. If we could think of the local signs of the fibres of the retina as being sensations of any two qualities which corresponded to two directions of co-ordinates, and whose intensity varied continually over the surface, contrasts of direction might be referred to precisely the same peculiarities of discrimination of intensity of sensation as contrasts of colour. However, having succeeded in tracing the influence of the ocular movements on directly visible phenomena, we may leave this other hypothesis alone for the present. Incidentally, ZÖLLNER, in describing the illusion in case of Fig. 31, tried too to connect it with the ocular movements. On the other hand, E HERING's explanation seems to be out of the question. He thinks that we judge the distance between two points by the rectilinear distance between their retinal images. Consequently, according to him, small intervals will generally appear relatively larger than large individual intervals, because for small arcs the difference between the arc and the chord, which is the measure of the distance between the ends of the arc, is relatively less than it is for large arcs. For just the same reason small angles should invariably be seen relatively too small as compared with their larger adjacent angles. On the same principle, A. KUNDT also tried to establish an extensive theory of these phenomena, and performed measurements, as stated above, which are intended to support it.[1] His method consisted in determining by the eye the length of an undivided line which was equal to that of a divided line. For lines of a certain length observation and calculation are found to agree fairly well; but for shorter

[1] POGGENDORFFS *Annalen*. CXX. 1863. 118–158.

lines, the difference between the two is nearly twice as much as it should be by the proposed method of explanation. Thus Mr. KUNDT found:

Visual angle for the distances to be compared		Errors observed	Errors computed
II	20° 14′	4.40	4.62
I	19° 41′	3.31	4.47
III	12° 47′	1.48	0.84

It should be noted that the illusions persist even when the figures are so tiny that the objects are almost at the limit of distinct vision; and that with such minute objects any difference between arc and chord ceases to be noticeable. KUNDT himself found, for example, that his Fig. 4 showed the illusion 9 feet away, in which case there is no longer any difference between the given arcs and angles even as far out as to the fifth place of decimals.

Accordingly, my position is that the method of explanation used by HERING and KUNDT does not even express the facts correctly. If it were meant to be regarded as an explanation of the actual causes of the phenomena, it would be necessary to extend the assumptions of the intuition theory by supposing that we were born with a knowledge of our retina, not only of the arrangement in space of the sensitive points on it, but in fact also as to how it was curved.

It should be added, in conclusion, that in certain cases binocular apperception of material space tends to interfere with the comparison of distances in the field of view. This goes to prove that our natural vision is vision of corporeal things. I can tell with much certainty whether my index finger is thicker or thinner than a gas-pipe on the opposite side of the room, although there is an enormous difference in the apparent sizes of the two objects. On the other hand, I am not at all sure whether my finger, held at a certain distance in front of my eye, has the same apparent size as a book on the other side of the room, or is as big as the moon, say: supposing that the two objects to be compared are not brought near together in the field. Rather in my own case, there is a very strong tendency to consider the angle subtended by my finger as being much smaller than that of the book or of the moon, unless I can bring the two quite near together or make them cover each other in the field of view.[1]

Connected with the above in my opinion is the fact, as shown by KUNDT's experiments, that, in trying to bisect a horizontal line, or-

[1] Concerning the origin of the absolute impression of size and its connection with distance and angular size, see Note 9 at end of §30, and also the discussions in the Appendix at the end of this volume.—K.

dinarily the right half is made too large by the right eye or the left half by the left eye. In case of a line 100 mm long, viewed from a distance of 226 mm, as an average of 40 trials, the middle of the line was put 50.33 mm from the left end by the left eye, and only 49.845 mm from the same end by the other eye. Incidentally, these deviations from the real middle of the line, amounting to 0.33 and 0.155 mm, are much smaller than the deviations of the individual observations from the average, the mean errors in this case being 0.50 and 0.66. Thus it was only by a large number of trials that the deviation in question was made manifest.

I think this deviation may be due to the fact that in looking at a bisected line with both eyes we are in the habit of holding it in front of the middle of the face symmetrically with respect to the head, and hence we are accustomed to consider the right half as larger with the right eye, and the left half with the left eye.

Before concluding this description of the visual globe, something must be said about its borders and the gaps in it. It embraces in its extent all the points in the space around us which can send light through the pupil that has a chance of reaching the sensitive parts of the retina. The visual globe does not comprise those parts of space, and especially, therefore, those parts behind us, which are so situated that no light from them can ever get to the retina in the normal way. Thus the surface of the visual globe corresponds to the image of the retina projected outward, and its borders to the borders of the retina. We are conscious of this limitation and aware that we have no visual perception of objects behind us. By giving heed to the field of indirect vision, we can tell what objects are just visible on the edge of the visual globe, and what objects are not visible, at least as far as it is possible to do so, considering how very vague vision is on the extreme periphery of the retina. Here it is to be noted that there is an essential difference between the geometrical continuation of the portion of the visual globe which can never be seen at all, and the visible part of the field, which may not be visible sometimes simply from lack of illumination. Even when all external light is extinguished, there is still before our eyes a definitely limited dark field. But under such circumstances we never dream of thinking that we see the space behind us as dark; we are simply conscious of not seeing it at all. The sensation of darkness is the sensation of the state of rest; or, if we prefer to say so, it is the lack of sensation in parts of the nervous mechanism of vision, which might be stimulated, if a stimulus acted there. The corresponding perception we have is the idea of portions of space in front of us which

send no light to the eye. This involves, therefore, a definite, although negative, statement as to the objective condition of these portions of space. But there is no organ of sense corresponding to the non-visible portions of space that could note and distinguish its own state of rest. Concerning them nothing whatever is expressed in the perception, beyond the fact that we know nothing about them, neither whether they are bright nor whether they are dark. It is well to make a distinction between these two conditions.

However, within the outside border of the visual globe, there is a gap corresponding to the insensitive place on the retina where the optic nerve comes into the eye, and where nothing is visible. The position and extent of this locality were defined in the first part of §18; and it was shown there that this spot was really insensitive to light. The question to be considered now is, How does the corresponding place on the visual globe appear to us?

Under ordinary circumstances, we are not in a position to notice that there is such a gap in the visual globe or to concentrate our mind on the nature of its appearance. Nor is this so merely when the apperception of objects that happen to be in this gap is supplied by the perceptions of the other open eye; or, supposing this other eye is closed, is supplied by movements of the one eye that is open. In this latter case the gap continually moves about in the field, so that such objects as were not in sight at one instant may be perceived at another instant. But even when the gaze is kept steadfastly fixed, we do not notice this gap when the part of the visual globe around it is represented by a uniformly illuminated coloured ground. In this case it seems to us rather as if this entire portion of the field were all of the same colour without any break. Under these circumstances, of course, it does not matter what sort of non-visible objects really are situated in the gap. They just disappear, as was explained above. The truth is we seldom use indirect vision anyhow to find out about the form, size and arrangement of objects visible in that way. Its main service consists in supplying us with a sort of rough sketch of the vicinity of the point of fixation where our attention is directed, and at the same time to divert our attention to any new or extraordinary phenomenon that may arise out toward the periphery of the field. Consequently, under ordinary circumstances, the attention is never bestowed on a portion of the field, such as the blind spot, where nothing ever happens either striking or otherwise. Indeed, I have known cultivated and informed people, even physicians, who were never convinced of the disappearance of small objects in this area. If we are accustomed to making experiments in physiological optics and are skilled in perceiving

objects by indirect vision, at first it will be only those larger objects which stand out conspicuously by virtue of brightness or colouring or movement, that can divert the attention enough for us to recognize how they are arranged without our having to change the point of fixation. But in indirect vision we are unable to turn our attention to some definite place where there is no peculiar sensation at all, such as the gap in the visual globe when it happens to be projected on a ground of uniform colour.

However, I must note in this connection that lately I have begun to see the blind spot when one eye was opened opposite an extended white surface, and also in case of slight movements of the eye or just when accommodation was beginning. Under these conditions it appeared like a shadowy spot, and when I pointed my finger at it, the tip of it would disappear. This is a subjective phenomenon connected with those described in Vol. II, page 10. It soon vanishes if the eye is kept open and stationary. Accordingly, it is merely an apparent, and not a real, exception to what has been stated; because in this case the subjective stimulation of the visual globe is not uniform, but the vicinity of the blind spot is distinguished by special phenomena that are calculated to rivet the attention on this place. And yet at other times it may be that when I am looking at a bright field I am completely unable to say, without any previous trial, whereabouts on the visual globe the blind spot is.

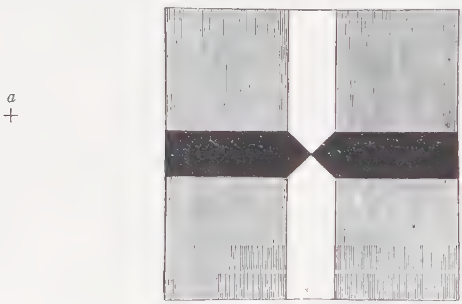

Fig. 35.

It is a different matter, at least with an observer who has had some practice in indirect vision, when there are certain tokens on the visual globe that serve to draw the attention directly toward this gap in it. A very convenient thing for this purpose, for instance, is a cross in which the vertical and horizontal arms are distinguished from each other by colour or brightness. Both arms of the cross should also be different from the background; and the place where they intersect should be such as can be completely concealed by the blind spot. A cross of this kind is represented in Fig. 35. The mark a is the point of fixation. The drawing is intended to be viewed at a distance of 16 cm. To prove that the place where the arms cross disappears entirely, cover it with a

coloured wafer, and when this has vanished, hold the eye perfectly steady and try to see whether the black or the white arm of the cross lies on top at the point of fixation. VOLKMANN[1] and most other observers, who have tried this experiment, thought they saw sometimes one arm of the cross, sometimes the other, lying above, but more frequently[2] the horizontal arm, probably because the horizontal diameter of the gap is less than the vertical. But if the horizontal arm is made shorter and shorter, the colour of the vertical arm finally prevails. I also thought at first that I saw it in this way, but now that I have had more experience and much practice in indirect vision, *I am perfectly certain that in this experiment I cannot perceive the place where the arms cross.* AUBERT also, who is one of the best trained observers in indirect vision, agrees with me here. His statement is as follows:

"Although I have had great experience in indirect vision, and have frequently repeated the experiment as described by WEBER and VOLKMANN and recently by WITTICH, I am bound to state publicly that I am not able to reach any decision as to how the field of view is completed at this place. Whether the place where the arms cross when one of them is yellow and the other blue, appears in one colour or the other, when this place falls on the blind spot, I am still unable to say, although I have repeated the experiment a hundred times. Nor can I tell whether two parallel lines are compressed together in the middle, or whether a circular line, either wide or narrow, completes the circle, or not."[3]

It is more difficult to direct the attention to the gap when it is traversed simply by a straight-edge without any break in it. Take a sheet of black paper one edge of which forms a vertical straight line, and gradually insert it from the outer side of the visual globe over a sheet of white paper, on which there is a point of fixation for the eye; until a part of the vertical edge falls in the gap. In this case most observers fancy that they see this edge all along without any break in it; but here also I have lately been convinced that I was able to tell when and where I cease to perceive part of the line. As the sheet of black paper is shoved inwards toward the point of fixation, I can tell the exact instant when the two visible ends of the edge come together. On drawing the sheet of black paper back toward the temporal side of the blind spot, it is much harder to be sure about the precise moment when the same thing occurs, because indirect vision is already much

[1] *Berichte der Kön. Sächs. Ges. d. Wissenschaften.* 30. April 1853. S. 40.

[2] v. WITTICH, Studien uber den blinden Fleck. *Archiv fur Ophthalmologie.* 1863. IX 3. S. 1 31.

[3] AUBERT, *Physiologie der Netzhaut.* Breslau 1865. S. 257–258.

more imperfect over on this side. A curious thing about it, though it is characteristic of the existence of the phenomenon, is that nowhere do I ever see a gap between the white and black fields, although I am aware that there is one part of the edge of the black paper that I cannot see, and that there is nothing inserted between the black and the white; and yet I cannot say where or how the contour is formed. I cannot even say that white and black are blended and confused together there, for the grey in this blending would be something definitely perceptible. All I can do is to compare it with the impression we get in trying to fixate and recognize faint objects in semi-darkness, when the individual parts of the picture are wiped out by after-images.

It is very much easier to see the gap when, instead of falling on a straight line in the field, it falls on part of a circle or on the edge of a circular area. In this case I can tell fairly well how much of the circle is lacking.

If there is a large number of tiny objects of various sorts in the field in front of me, I am enabled immediately to tell where the blind spot is by a certain vagueness and indistinctness which are characteristic of it. For instance, this is the case in looking at a mass of foliage or embroidered tapestry or a sheet of printed paper.

Consequently I must assert that on the whole there is no sensation at all corresponding to the blind spot, and especially that no sensations whatever are transferred from the surrounding neighbourhood to the gap in the visual globe, but that it can be demonstrated, by careful observation and by using the proper means for attracting the attention to the blind spot, that sensation is lacking there. Nothing bright or coloured or dark is to be seen in the gap in the field. What we see there is literally nothing—a nothing that is not even a hole and not even manifested by being the edge of the visible. For if the gap in the visible field were itself visible, it would have to show some quality of visibility, but it does not. It is only negatively that we can discover its existence, and by observing the last objects that can still be seen. Then when we discover that these latter do not touch each other in space, we begin to suspect that there is a gap and to form some notion about its size and position in space. But as this involves localizing the visual impression and as that is something which, according to our point of view, can be acquired only by experience, the discovery of this gap must be, as a matter of fact, the result of a judgment. It is not a direct sensation.

Incidentally, the case is entirely similar with respect to the larger gap situated back of the head; except that the existence of the latter is more familiar to us than that of the blind spot, because under no

circumstances have we ever had any way of filling up that gap, whereas the gap due to the blind spot is ordinarily compensated to a sufficient extent by the perceptions of the other eye and by the ocular movements, so that it is not a sensible deficiency. Even the borders of the visual globe can only be determined by the negative process of seeking to ascertain which objects are just visible in indirect vision, and which are not. On the other hand, in the case of a uniform background, as, for instance, when the eye is turned inward, and a sheet of translucent paper illuminated from behind is held in front of it, none of the parts of the face next the outer angle of the eye can be seen any longer, and then it is absolutely impossible to say where this bright surface ends, and where non-vision begins. But if there were some dark or coloured spot on the paper, we should be able to tell immediately the direction in which we saw it. And so even here the non-visible is not separated from the visible by a distinct border.

The case is different when our ideas of objects are the results of sensations. There cannot be any hole in objective space and the objects it contains corresponding to the gap in the visual globe. Thus, we are practically in the position of a person looking at a spotted picture or one perforated with holes, and trying to obtain from it an idea of what the painter intended to portray. If there happens to be a spot on some subordinate part of the picture where the continuation is perfectly obvious, the spectator will probably scarcely notice it, or at any rate will not be hampered by it at all in his conception of the object. So far as this is concerned, he may consider the spot as not even being there. Thus if the spot were to fall on a uniformly coloured surface or on a surface with the same pattern over it, the spectator will at once supply the gap by his imagination and fill it up with the colour of the background. There would have to be some very special reason to make him suppose that the colouring or pattern at that place was originally different. And, likewise, without the slightest hesitation or doubt, he supplies the deficiency where the spot happens to cover a small part of a rectilinear edge or of the circumference of a circle. It is only when the spot happens to be on important points in the picture or on places whose significance is not obvious, that the spectator's attention will be arrested by it, and he will have difficulty in completing his perceptual image of the objects depicted.

The above comparison may help to make the matter tolerably clear; especially by supposing that the painting is full of interesting detail and that the spot falls out to one side where there is nothing important at all, and has nothing about it in the way of colour or

brightness to distract the attention. Then possibly it would not be detected at all, as is ordinarily the case with the gap in the visual globe. Where the comparison fails is that the spot on the painting is something visible, which may easily rivet the attention entirely when once it has been attracted to it; whereas the break in the field of view does not have the quality of something visible, and it is entirely contrary to our custom and practice to turn our attention to anything in the field of indirect vision except to individual, positive and conspicuous phenomena. In both cases our idea of the objects is formed as well as possible from the positive details that are present in the sensation; but in case of the gap in the visual globe it is much more difficult to note the lack of material for apperception than in case of the spot on the painting. In this connection, therefore, VOLKMANN is correct in saying that we complete the gap in the visual globe by an act of the imagination; only it should be added that the complete evidence of the sense-apperception is not furnished by this act of imagination, although it is certainly more difficult to tell here than it is in other similar cases that there is a lack of sensation material. One of the most beautiful illustrations of this completion by the power of imagination is given by VOLKMANN: if the gap is allowed to fall on the page of a book, we fancy we see it completed by words, which, of course, cannot be read. But, undoubtedly, this completion only lasts apparently until we discover, on closer attention, that we perceive nothing at that place. The activity of the power of imagination, therefore, by no means extends so far as to simulate the absent visual sensation and to take the place of it.

The next thing we have to find out here is how correct measurements of space as made by the eye prove to be for points that happen to be near the gap in the field. The statements of different observers on this subject are found to be much at variance. Some of them, notably v. WITTICH, for example, find that objects that are very close to this place are apparently drawn toward it and have a tendency to fill it out. Others, like E. H WEBER, VOLKMANN and myself, observe the surrounding parts in their correct positions, except for the distortions that are characteristic of the outer parts of the field of view anyhow. Finally, some observers, like FUNKE, find that it varies, and that they see it sometimes one way, and sometimes the other, under somewhat different conditions.

The differences are brought out very clearly in the following experiment devised by VOLKMANN. Nine letters A, B, C, etc. are arranged as shown in Fig. 36. When the right eye is placed 20 cm away and focused on the little cross at k, the central letter E will fall

in the gap. Under these circumstances, the size of the gap for my eye is shown by the dotted circle around E. By placing a small red wafer on E, and shifting it, first in one direction, and then in another, until it just begins to be visible, it is possible to tell how big the gap is, and also whether no one of the other letters is concealed by it. A similar pattern made of wafers of different colours, instead of the letters, also answers the purpose very well. In the case of Fig. 36, both VOLKMANN and I see the eight letters $A\ B\ C\ F\ I\ H\ G\ D$ as forming the sides of a square, standing, therefore, in straight lines, as they really do, the centre of the square being empty. On the other hand, v. WITTICH, instead of seeing the straight sides of a square, observes four arcs convex towards the centre, ABC, CFI, IHG, GDA. They appeared convex to FUNKE[1] also, provided no other straight lines were in the neighbourhood with which they could be compared. On the contrary, when a vertical line was drawn through k or between k and ADG, or even when the vertical row CFI was covered by a piece of white paper, the sides of the figure seemed to FUNKE to be straight, as they looked to VOLKMANN.

Fig. 36.

According to v. WITTICH, a straight line, with its middle in the gap, will appear to be shortened; but it does not appear so to E. H. WEBER, or VOLKMANN or myself. A circular area, almost, but not entirely, covered by the blind spot, its edge being visible all around, looks to me just as large as a similar surface just as far to the nasal side of the point of fixation. Incidentally, I agree with WEBER and VOLKMANN in thinking that I see the whole area of the same colour as that of the edge, although only a little of the edge lies outside the gap. Indeed, if the disc is cut out of a piece of finely printed paper, I fancy that I see it covered all over with letters, until my attention is directed exactly on it, and then I perceive that I do not distinguish anything in the centre.

FUNKE states that when the gap falls on a sheet of printed paper, and he has noted two prominent letters on either side of it, they are

[1] *Berichte der naturforschenden Gesellschaft zu Freiburg i. Br.* Bd. III. Heft 3. S. 12, 13.

apparently nearer together than before. Here also I see the letters at their right distance.

The explanation of these contradictions may be perhaps that WEBER's circles of sensation are also of some aid to us in forming our estimates of dimensions of space on the visual globe. We learn about the latter mainly by movements of the eye. But the circles of sensation may have something to do with these judgments, particularly in case of small objects which are close together, and for which the other kind of estimate probably affords less satisfactory data. Whether two black points on opposite sides of the point of fixation are equidistant from it or not, cannot be decided as accurately as when they are both near together on the same side, with a white spot of the background visible in between them. In the latter case there is no doubt as to which one is nearer the point of fixation, and which one is farther from it.

In the other parts of the visual globe the two methods of determination are necessarily in accordance with each other. But in the region of the blind spot there are no impressions such as we should expect to have between those at the edge of the gap, and such as should be the sensible sign of their separation in space. On the other hand, by means of movements of the eye we can still have correct experiences as to the actual places where the edges of the gap are, and thus perceive that they are separated. Hence it is possible that different observers may be in the habit of heeding one of these considerations more than the other, and thus their estimates may be different. And even in the case of one and the same observer secondary causes may be responsible for this or that judgment.

It has already been stated that generally the gap in each eye in ordinary binocular vision is filled out by what is seen by the other eye in that place on the visual globe. But there are likewise some exceptions to this rule, as has been shown by VOLKMANN. Suppose we use the letters a and α to designate the blind spots of the two eyes; and b and β to designate the regions surrounding a and α, respectively. Moreover, let A designate the place in the field of view corresponding to the two places a and α, and B the region around A. Then the following experiments may easily be performed:

1. Close one eye and look at a sheet of white paper with the other eye. The sensations obtained will be:

   Nothing on a; White on b;
   Dark on α; Dark on β;

   and what is supposed to be seen will be:

   White on A; White on B.

2. Look at a sheet of white paper with both eyes, at the same time holding a piece of blue glass in front of one eye. Then the sensations obtained will be:

Nothing on a;	White on b;
Blue on α;	Blue on β;

and what is supposed to be seen will be:

Blue-white on A;	Blue-white on B.

3. The experiment proves to be similar when we look with both eyes through pieces of glass of different colours, in which case the two colours will appear to be superposed on the visual globe, not uniformly, but mixed in varying proportions. Even then A will not be distinguished from the rest of the field in any way whatever.

In the previous cases where the place α was just as much illuminated as β, the gap in the field was supposed to be seen in the same colour as that of the ground. The special result of this was that the place A on the visual globe, which aroused no sensation at all in one eye, and the sensation of black or blue in the other eye, appeared to be white or blue-white.

4. Now look at a sheet of black paper with a white cross on it corresponding to the gap a. The sensations obtained will be:

Nothing on a;	Black on b;
White on α;	Black on β;

and what we see will be:

White on A;	Black on B.

When a piece of blue glass is held in front of the second eye, of course, blue occurs all over, instead of white.

5. Look at a white field with a black spot on it, corresponding to the gap a. The sensations obtained will be:

Nothing on a;	White on b;
Black on α;	White on β;

and what we see will be:

Black on A;	White on B.

6. Having maintained the same fixation for a little while as in the preceding experiment, look at another place on the white surface, and then there will appear a more brilliant white after-image of the black spot, which likewise corresponds to the place where the gap is. Thus even the faint difference between the somewhat more brilliant white of the after-image and the somewhat duller white of the ground will be sufficient to define the visual impression of the gap. The result is also that apparent contradictions with 3. may occur.

7. If the conditions in the previous experiment are modified by putting a piece of green glass in front of the eye ab and a piece of red glass in front of the eye $\alpha\beta$, and then focusing so that the black spot corresponds to the gap a, the spot will appear to be black-green almost as if it were being viewed through the green glass with the gap a. But, as a matter of fact, this is a contrast colour in the other eye due to α being against the red ground β. If the same fixation is maintained for a little while, and then the eye turned to look at another part of the paper, the place A in the field of view will look pure red, apparently as seen by the eye $\alpha\beta$ alone. But in this case it is the bright red after-image of the previously seen black, by which α distinguishes itself from β and thus determines the impression.

From these latter experiments it would appear, therefore, that the impression on α determines the total image, at least in the case when α is distinctly differentiated from β by brightness and colour. Yet even in such cases α is not the sole determining factor.

8. Look at a piece of pale grey paper with a white wafer on it corresponding to the gap α; and place a piece of red glass in front of the closed eye $\alpha\beta$, and then open this eye. The sensation obtained then will be:

Nothing on a;	Grey on b;
Red on α;	Dull red on β;

What seems to be seen is:

Red-white on A;	Grey-red on B.

The red on α, when the eye ab is closed, is decidedly more saturated than it is at A, when the eye ab is open, although a gets no impression. A corresponding effect is obtained with glasses of other colours. The difference was found to be still more distinct when a red wafer was placed alongside the white one, which as seen through the red glass looked just like the white one. However, until the eye behind the red glass is opened, the red wafer must be covered by a screen of the same colour as the ground, so that it will not develop any after-image tending to dilute the red and make it grey by contrast.

In this last case the influence of the grey ground at b, which causes us to see a whitish, is imperceptible. These phenomena can all be explained by the following law: *In binocular vision the place A on the visual globe which corresponds to the gap is supposed to be just as much brighter or darker than the ground B as it really is brighter or darker as seen by the other eye* ($\alpha\beta$). The common colouration of the visual globe at α and β is not transferred to the gap in the field of the other eye, but

perhaps the difference between α and β is regarded as existing also for a and b. We shall see similar relations again when we come to consider the theory of binocular contrast.

There is some difficulty about explaining those subjective phenomena which occur just at the place where the optic nerve enters the eye; such as the sheaves of light when the eye is being moved quickly and the bright or dark round areas in case of electrical stimulation. The only way of explaining them is by supposing that under such circumstances the parts of the retina are affected immediately around the optic nerve. The simple explanation of the electrical effects may be that the sinewy mass of the optic nerve lying behind the sclera is such a poor conductor that it is difficult for the current to flow through the parts of the retina directly in front of it, and therefore there is a contrast between them and the rest of the field of view. An ascending current, which renders the field luminous, makes the place look dark where the optic nerve enters and where the conductivity is bad; whereas a descending current, which makes the field dark and reddish yellow, causes this place to appear luminous and blue.

It is impossible to prove the correctness of this explanation of the luminous sheaves in the case of rapid movement of the eye; but perhaps it can be proved with respect to the corresponding dark spots that are seen when the eyes are turned far to one side toward a uniformly illuminated field. If they are turned to the left, a dark spot will be seen by the right eye off to the right in the field of view, with its right edge very well defined, whereas the left edge toward the centre of the field is extremely vague. This is the place where the gap lies in the field; for when the point of a lead pencil is inserted in front of this inner edge of the dark spot, it will disappear, without doing so, however, in the rest of the dark spot.

On the other hand, when the left eye is turned to the left, the dark spot in front of it will be seen lying between the point of fixation of this eye and the blind spot. Thus when the two eyes are turned to the left, the retina of each eye is made more insensitive on the left of the optic nerve (the dark spot in the field of view being turned to the right). This is the side where the trunk of the nerve is bent toward the sclera. It is probably arched in a little thus tending to distort the retina. Thus it may be shown that these dark spots do not correspond to the actual place where the optic nerve enters the eye, but lie to one side of it. Perhaps, just as in the case of pressure images, the luminous phenomena in the dark field will occupy the same place here. By directing my attention specially toward it, I believe I have been able to detect that the point of one of the sheaves extended as far as the point of fixation, as one of the dark spots does. Accordingly, the data given

in Vol. II, pages 9 and 10, as to the place of this spot, should be amended.[1]

In looking at two points in the field of view situated at different distances, so that the eye cannot be exactly accommodated for both of them at the same time, at least one of the points will appear blurred. The cone of rays by which this blurred image is formed is limited by the pupillary opening, the ray which goes through the centre of the pupil being the axis of the cone. Therefore, if the centres of the blur circles of two points at different distances coincide on the same retinal point a, or if a punctual image of one of the points is at the centre of the blurred image of the other point, those rays coming from the two points which go through the centre of the pupil must coincide all along; or that ray, which goes through both points, must afterwards pass through the centre of the pupil.

The centre of the pupil is in the interior of the optical system of the eye, the cornea being in front of it, the crystalline lens beyond it. Thus the rays undergo a refraction before arriving at the centre of the pupil, and then they are deflected again after having passed through it.

Rays emanating from the real centre of the pupil are refracted by the cornea as if they had come from the image of the centre of the pupil as formed by the cornea. Conversely, rays, which outside the eye are convergent toward the image of the centre of the pupil, will pass through the centre of the pupil itself.

The image of the centre of the pupil as produced by refraction in the cornea is, therefore, the same as the so-called *point of intersection of the lines of sight*.[2] When two luminous points are on a straight line passing through this point, the centres of their blur circles will coincide on the retina.

For the schematic eye as given in Vol. I, page 152, I have likewise calculated the distance from the cornea of the point of intersection of the lines of sight (in millimetres), as follows:

	In far vision	In near vision
1. Distance of centre of pupil....	3.6	3.2
2. Distance of point of intersection of lines of sight..............	3.036	2.661
3. Distance of latter point from centre of pupil...............	0.564	0.539

[1] ¶Some more recent literature on the subject of the blind spot is as follows:

C. E. Ferree and G. Rand, The spatial values of the visual field immediately surrounding the blind spot and the question of the associative filling in the blind spot. *Amer. J. of Physiol.*, 29 (1912), 398-417. H Werner, Untersuchungen über den blinden Fleck. Pflügers *Arch.*, 153 (1913), 475-490. F. Rossler, Der blinde Fleck in schielenden Augen. *Arch. f. Ophthalm.*, 105 (1921), 48-103.—F. Nussbaum, Über die Raumwerte in der Umgebung des blinden Flecks. *Arch. f. Augenh.*, 87 (1921), 142-151. (J. P. C. S.)

[2] ¶ The same as the centre of the entrance-pupil of the eye. (J.P.C.S.)

The vertex of the visual angle is determined in another way when the eye is being continually accommodated for the observed objects, because, as the accommodation varies, the nodal points themselves will be shifted. Under such circumstances, the simplest way of finding the vertex of this angle is as follows.

In Fig. 37 suppose that A designates the required vertex of the visual angle. Let DA and CA represent two straight lines intersecting at A and making equal angles there with the optical axis EA, all three lines being in the same plane. Two objects, as represented by the arrows, with their extremities lying in the lines DA and CA, are required to produce retinal images FG of equal size, when the eye is properly accommodated for the extremities of the objects under consideration. Suppose that B_0 and B_1 are the images of the point A when the eye is accommodated for far vision and for near vision, respectively. Regarding the lines DA and CA as being incident rays, we note that, after arriving in the vitreous humor, they will diverge from B_0 or from B_1 so as to proceed to F and G.

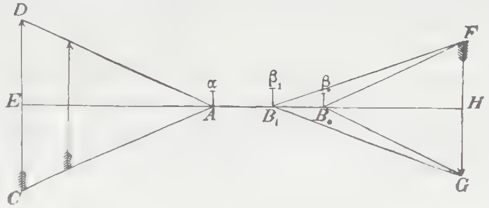

Fig. 37.

Imagine a small object α perpendicular to the axis at A; its optical images at B_0 and B_1 being β_0 and β_1, respectively. Then by equation (7d), in Vol. I, p. 74, we obtain the following relation between the angles DAC, FB_0G, FB_1G and these images:

$$n_1\, \alpha \tan\frac{DAC}{2} = n_2\, \beta_0 \tan\frac{FB_0G}{2}$$

$$= n_2\, \beta_1 \tan\frac{FB_1G}{2},$$

where n_1, n_2 denote the indices of refraction of air and vitreous humor, respectively. But since

$$\tan\frac{FB_0G}{2} = \frac{FH}{HB_0},$$

$$\tan\frac{FB_1G}{2} = \frac{FH}{HB_1},$$

it follows that

$$\beta_0 : \beta_1 = HB_0 : HB_1 .$$

Accordingly, the required position of the vertex of the visual angle is given by the fact that when a small (virtual) object is placed there at right angles to the axis, the size of its image increases for changes of accommodation of the eye in the same ratio as the distance of the image from the retina.

If the average values of the optical constants of the schematic eye, as given in Vol. I, p. 152, for far vision and near vision, are used here to calculate the position of this point A, its distance from the cornea will be found to be 2.942 mm; so that it is almost exactly coincident with the place of intersection of the lines of sight of the unaccommodated eye, whose distance from the cornea was found to be 3.036 mm. For practical applications, therefore, the two points may be regarded as coincident; especially as such small differences as those found here are negligible as compared with the inaccuracies of our present knowledge of the optical constants of the eye.

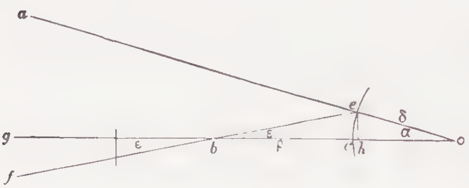

Fig. 38.

Accordingly, so far as the size of the visual angle of the passive eye is concerned, it does not matter whether the eye is accommodated for the objects to be observed or whether it is accommodated for infinity.

The difference between the angle made by two lines drawn from two points of the object to the nodal point of the eye and the angle made by drawing lines from the same two points to the centre of rotation of the eye, is what LISTING[1] called *the parallax between the apparent positions of the object in direct and indirect vision.* In employing this term, I take the vertex of the first angle at *the point of intersection of the lines of sight,*[2] because in indirect vision two luminous points have the same positions when they lie on the same line of sight.

This parallax is equal to zero when the objects are infinitely distant; because for infinitely distant objects the sides of the two angles to be compared are mutually parallel. When one of the objects is infinitely far away, the parallax tells us how much the nearer object appears to be shifted on the infinite background of space when the eye is focused on it.

In order to make a comparison in this relatively simplest case between the above mentioned parallax and the faults of accommodation, consider the diagram in Fig. 38, where the centre of rotation of

[1] *Beitrag zur physiologischen Optik.* Göttingen 1845. S. 14–16.

[2] The lines of sight, in LISTING's nomenclature, are lines [of fixation] drawn from the object to the centre of rotation of the eye.

the eye is designated by o, and where $oc = oe = \sigma$ represents the distance of this point of intersection of the lines of sight. The farther object is supposed to lie in the direction oa, while the nearer one is at b. When the eye is focused on b, it appears to lie in the direction bg, and to coincide with the part of the infinite background in this direction. But if the eye is turned to look in the direction oa, the point of intersection of the lines of sight will be at e, and then the point b will appear to be in the direction ef. Hence, the angle $ebc = fbg = \epsilon$ is the parallax between direct and indirect vision. Denoting the distance bc by ρ, we may write:

$$\tan \epsilon = \frac{eh}{hb} = \frac{\sigma \sin \alpha}{\rho + \sigma(1 - \cos\alpha)}.$$

According to equation (1b), Vol. I, page 131, the diameter (p) of the blur circles of b, when the eye is accommodated for infinity, is

$$p = \frac{P \cdot H}{\rho},$$

where P denotes the diameter of the pupil as seen through the crystalline lens, and H denotes the distance of the anterior focal point from the point of intersection of the lines of sight. If η denotes the angle subtended by the radius of the blur circle as projected on the infinitely distant background, and f denotes the distance of the nodal point of the cornea from the posterior focal point, then

$$\tan \eta = \frac{p}{2f} = \frac{P \cdot H}{2\rho \cdot f}.$$

If the distance σ (10.5 mm) can be neglected in the expression for $\tan \epsilon$ as compared with ρ (the distance of the object), then

$$\tan \epsilon = \frac{\sigma \sin \alpha}{\rho}.$$

Hence η will be greater than ϵ, as long as

$$\frac{PH}{2\sigma f} > \sin \alpha .$$

But according to the previous data for the unaccommodated eye:

$$F = 15.869 \text{ mm},$$
$$f = 15.007 \text{ mm},$$
$$\sigma = 10.521 \text{ mm}.$$

P may vary between 3 and 6 mm. Corresponding to the first value,

$$\alpha < 8.40°;$$

and to the second value,

$$\alpha < 17.33°.$$

As long as the movement of the eye is not greater than this value of the angle α, the displacement in passing from indirect to direct vision is not more than the radius of the blur circle in the image of the nearer point.

When we consider here how exceedingly indistinct indirect vision is at a distance of 8° from the point of fixation, we can understand why it is; because, when a very brilliant point disappears behind the edge of a dark screen, it is exceptional to be able to perceive the change of the image due to ocular movements.

I am inserting here two reports that have an important bearing on the theory of the comprehension of visual phenomena. They are observations made by CHESELDEN and WARDROP on two persons born blind, whose vision was only restored later in life by an operation. CHESELDEN operated on a boy, 13 years old who had a very pronounced congenital opacity of the crystalline lens (grey cataract, as it is called).

Concerning his ability to distinguish forms, CHESELDEN'S report is as follows:[1]

"When he first saw, he was so far from making any judgment about distances that he thought all objects whatever touched his eyes (as he expressed it) as what he felt did his skin; and thought no objects so agreeable as those which were smooth and regular, though he could form no judgment of their shape, or guess what it was in any object that was pleasing to him. He knew not the shape of any thing, nor any one thing from another, however different in shape, or magnitude, but upon being told what things were, whose form he before knew from feeling, he would carefully observe, that he might know them again; but having too many objects to learn at once, he forgot many of them: and (as he said) at first he learned to know, and again forgot a thousand things in a day. One particular only (though it may appear trifling) I will relate: Having often forgot which was the cat, and which the dog, he was ashamed to ask, but catching the cat (which he knew by feeling) he was observed to look at her steadfastly, and then setting her down, said, "so puss, I shall know you another time." He was very much surprized, that those things which he had liked best, did not appear most agreeable to his eyes, expecting those persons would appear most beautiful that he loved most, and such things to be most agreeable to his sight that were so to his taste. We thought he soon knew what pictures represented, which were shewed to him, but we found afterwards we were mistaken: for about two months after he was couched he discovered at once, they represented solid bodies; when to that time he considered them only as party-coloured planes, or surfaces diversified with variety of paint; but even then he was no less surprized, expecting the pictures would feel like the things they represented,

[1] *Phil. Trans.* XXXV. 1728. p. 447.—SMITH'S *Compleat System of Opticks*. Remarks, page 27.

¶CHESELDEN'S account is quoted by SMITH in his *Opticks*, Book I, Chapter V, §133; and the editor has inserted it here in the text just as it is given there. (J.P.C.S.)

and was amazed when he found those parts, which by their light and shadow appeared now round and uneven, felt only flat like the rest: and asked which was the lying sense, feeling or seeing?

"Being shewn his father's picture in a locket at his mother's watch, and told what it was, he acknowledged a likeness, but was vastly surprized; asking how it could be, that a large face could be expressed in so little room; saying, it should have seemed as impossible to him, as to put a bushel of any thing into a pint.

"At first he could bear but very little light, and the things he saw, he thought extreamly large; but upon seeing things larger, those first seen he conceived less, never being able to imagine any lines beyond the bounds he saw: the room he was in, he said, he knew to be but part of the house, yet he could not conceive that the whole house could look bigger. Before he was couched, he expected little advantage from seeing, worth undergoing an operation for, except reading and writing; for he said, he thought he could have no more pleasure in walking abroad than he had in the garden, which he could do safely and readily. And even blindness he observed, had this advantage, that he could go any where in the dark much better than those who can see; and after he had seen, he did not soon lose this quality, nor desire a light to go about the house in the night. He said every new object was a new delight, and the pleasure was so great, that he wanted ways to express it; but his gratitude to his operator he could not conceal, never seeing him for some time without tears of joy in his eyes, and other marks of affection: and if he did not happen to come at any time when he was expected, he would be so grieved, that he could not forbear crying at the disappointment. A year after his first seeing, being carried upon *Epsom* Downs, and observing a large prospect, he was exceedingly delighted with it, and called it a new kind of seeing. And now being lately couched of his other eye, he says, that objects at first appeared large to this eye, but not so large as they did at first to the other: and looking upon the same object with both eyes, he thought it looked about twice as large as with the first couched eye only, but not double, that we can any ways discover."

It should be noted, that although the crystalline lens in this case was still so opaque, the blind boy was always able to learn how he had to move his eyes to get the brightest impression from the sun, that is, by looking at the sun. And so he could not be considered as not having had any training in judging where objects were by looking toward them. Indeed, it is not likely *per se* that the lens diffused the light so perfectly uniformly every time in all directions, that the parts of the retina, near the place where the rays should have been focused, were not ultimately somewhat more highly illuminated than the rest of the surface of the retina. If this were the case, a certain degree of localization on the visual globe, however imperfect and inexact, might even have been developed; as was noticed also by J. WARE[1] in a similar case. He found that children with cataract were still able to recognize not only the colours of objects brought close

[1] JAMES WARE, Case of a young Gentleman, who recovered his Sight when seven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks. *Phil. Trans.*, XCI, 1801. pp. 382-396.

to the eye, but even their distance to some extent. A boy seven years old, on whom Ware operated, was from the beginning much cleverer and more certain than Cheselden's patient. Still in the case described above, it is very interesting to see the distinct importance of *learning* about the visual perceptions.

Still more remarkable in many ways is the case of a lady reported by Wardrop,[1] who was born blind, probably with opacity of the crystalline lens in both eyes. When she was six months old, an operation was performed on her in Paris, resulting in the complete loss of the right eye; whereas the pupil of the other eye was so overgrown that there was no longer any trace of it to be seen except some streaks of yellow lymph which were deposited in an irregular manner over the central part of the iris. Consequently, she was much blinder than persons with cataract usually are, and could hardly tell more about the light and its direction than a normal person can do with his eyes shut. She was able to distinguish between a very light and a very dark chamber, but without having the power to perceive even the situation of the window through which the light entered. But in sunshine or in bright moonlight she knew the direction from whence the light emanated.

On January 26, 1826 an attempt was made to cut through the exudations that closed the pupil, but it was not successful. Thereupon, on February 8, a section was made through the iris, enabling light to penetrate freely into the eye. But beyond this opening there was still some opaque matter. During the moderate inflammation that ensued, the patient was very sensitive to light. It was noticed that she frequently tried to see her hands. Finally, on February 17, the opening in the iris was enlarged, and the masses of opaque matter behind it were removed, enabling vision to be free at last. Here I shall quote the substance of Wardrop's report as follows:

"The operation being performed at my house, she returned home in a carriage, with her eye covered only with a loose piece of silk, and the first thing she noticed was a hackney coach passing, when she exclaimed. 'What is that large thing that has passed by us?' In the course of the evening she requested her brother to show her his watch, concerning which she expressed much curiosity, and she looked at it a considerable time, holding it close to her eye. She was asked what she saw, and she said there was a dark and a bright side; she pointed to the hour of 12, and smiled. Her brother asked her if she saw any thing more? she replied, 'Yes,' and pointed to the hour of 6, and to the hands of the watch. She then looked at the chain and seals, and observed that one of the seals was bright, which was the case, being a solid piece of rock crystal. The following day I asked her to look again at the

[1] James Wardrop, Case of a lady born blind, who received sight at an advanced age by the formation of an artificial pupil. *Phil. Trans.*, Vol. 116. 1826. pp. 529–540.

watch, which she refused to do, saying, that the light was offensive to her eye, and that she felt very stupid; meaning that she was much confused by the visible world thus for the first time opened to her. On the third day she observed the doors on the opposite side of the street, and asked if they were red, but they were in fact of an oak colour. In the evening she looked at her brother's face, and said that she saw his nose; he asked her to touch it, which she did; he then slipped a handkerchief over his face, and asked her to look again, when she playfully pulled it off, and asked, 'What is that?'

"On the sixth day she told us that she saw better than she had done on any preceding day; 'but I cannot tell what I do see; I am quite stupid.' She seemed indeed bewildered from not being able to combine the knowledge acquired by the senses of touch and sight, and felt disappointed in not having the power of distinguishing at once by her eye, objects which she could so readily distinguish from one another by feeling them.

"On the seventh day she took notice of the mistress of the house in which she lodged, and observed that she was tall. She asked what the colour of her gown was? to which she was answered, that it was blue: 'so is that thing on your head,' she then observed; which was the case: 'and your handkerchief, that is a different colour;' which was also correct. She added, 'I see you pretty well, I think.' The teacups and saucers underwent an examination: 'what are they like?' her brother asked her. 'I don't know,' she replied; 'they look very queer to me; but I can tell what they are in a minute when I touch them.' She distinguished an orange on the chimney-piece, but could form no notion of what it was till she touched it. She seemed now to have become more cheerful, and entertained greater expectation of comfort from her admission into the visible world; and she was very sanguine that she would find her newly acquired faculty of more use to her when she returned home, where everything was familiar to her.

"On the eighth day, she asked her brother, when at dinner, 'what he was helping himself to?' and when she was told it was a glass of port wine, she replied, 'port wine is dark, and looks to me very ugly.' She observed, when candles were brought into the room, her brother's face in the mirror, as well as that of a lady who was present; she also walked, for the first time without assistance, from her chair to a sopha which was on the opposite side of the room, and back again to the chair. When at tea, she took notice of the tray, observed the shining of the japan work, and asked 'what the colour was round the edge?' she was told that it was yellow; upon which she remarked, 'I will know that again.'

"On the ninth day she came down stairs to breakfast in great spirits: she said to her brother, 'I see you very well to-day,' and came up to him, and shook hands. She also observed a ticket on a window of a house on the opposite side of the street ('a lodging to let'); and her brother, to convince himself of her seeing it, took her to the window three several times, and to his surprise and gratification, she pointed it out to him distinctly on each trial.

"She spent a great part of the eleventh day looking out of the window, and spoke very little.

"On the twelfth day she was advised to walk out, which recommendation pleased her much. Mr. —— called on her, and she told him she felt quite happy. Her brother walked out with her as her guide, and took her twice round the piazzas of Covent-garden. She appeared much surprised, but apparently delighted; the clear blue sky first attracted her notice, and she said, 'it is the prettiest thing I have ever seen yet, and equally pretty every time I turn round and look at it.' She distinguished the street from the foot pavement distinctly, and stepped from one to the other like a person accustomed to the use of her eyes. Her great curiosity, and the manner in which

she stared at the variety of objects, and pointed to them, exciting the observation of many bystanders, her brother soon conducted her home, much against her will.

"On the thirteenth day nothing particular took place till tea-time, when she observed that there was a different tea-tray, and that it was not a pretty one, but had a dark border; which was a correct description. Her brother asked her to look in the mirror, and tell him if she saw his face in it? to which she answered, evidently disconcerted, 'I see my own; let me go away.'

"She drove in a carriage, on the fourteenth day, four miles on the Wandsworth road; admired most the sky and the fields, noticed the trees, and likewise the river Thames as she crossed Vauxhall bridge. At this time it was bright sunshine, and she said something dazzled her when she looked on the water.

"On the fifteenth day, being Sunday, she walked to a chapel at some distance, and now evidently saw more distinctly, but appeared more confused than when her sight was less perfect. The people passing on the pavement startled her; and once when a gentleman was going past her, who had a white waistcoat and a blue coat with yellow buttons, which the sunshine brought full in her view, she started so as to draw her brother, who was walking with her, off the pavement. She distinguished the clergyman moving his hands in the pulpit, and observed that he held something in them; this was a white handkerchief

"She went in a coach, on the sixteenth day, to pay a visit in a distant part of the town, and appeared much entertained with the bustle in the streets. On asking her how she saw on that day? she answered, 'I see a great deal, if I could only tell what I do see; but surely I am very stupid.'

"Nothing particular took place on the seventeenth day; and when her brother asked her how she was? she replied, 'I am well, and see better; but don't tease me with too many questions, till I have learned a little better how to make use of my eye. All that I can say is, that I am sure, from what I do see, a great change has taken place; but I cannot describe what I feel.'

"Eighteen days after the last operation had been performed, I attempted to ascertain by a few experiments her precise notions of the colour, size, forms, position, motions and distances of external objects. As she could only see with one eye, nothing could be ascertained respecting the question of double vision. She evidently saw the difference of colours; that is, she received and was sensible of different impressions from different colours. When pieces of paper one and a half inch square, differently coloured, were presented to her, she not only distinguished them at once from one another, but gave a decided preference to some colours, liking yellow most, and then pale pink. It may be here mentioned, that when desirious of examining an object, she had considerable difficulty in directing her eye to it, and finding out its position, moving her hand as well as her eye in various directions, as a person when blind-folded, or in the dark, gropes with his hands for what he wishes to touch. She also distinguished a large from a small object, when they were both held up before her for comparison. She said she saw different forms in various objects which were shown to her. On asking what she meant by different forms, such as long, round and square, and desiring her to draw with her finger these forms on her other hand, and then presenting to her eye the respective forms, she pointed to them exactly: she not only distinguished small from large objects, but knew what was meant by above and below; to prove which, a figure drawn with ink was placed before her eye, having one end broad, and the other narrow, and she saw the positions as they really were, and not inverted. She could also perceive motions; for when a glass of water was placed on the table before her, on approaching her hand near it,

it was moved quickly to a greater distance, upon which she immediately said, 'You move it; you take it away.'

"She seemed to have the greatest difficulty in finding out the distance of any object; for when an object was held close to her eye, she would search for it by stretching her hand far beyond its position, while on other occasions she groped close to her own face, for a thing far removed from her.

"She learned with facility the names of the different colours, and two days after the coloured papers had been shown to her, on coming into a room the colour of which was crimson, she observed that it was red. She also observed some pictures hanging on the red wall of the room in which she was sitting, distinguishing several small figures in them, but not knowing what they represented, and admiring the gilt frames. On the same day, she walked round the pond in the centre of St. James square and was pleased with the glistening of the sun's rays on the water, as well as with the blue sky and green shrubs, the colours of which she named correctly.

"It may be here observed, that she had yet acquired by the use of her sight but very little knowledge of any forms, and was unable to apply the information gained by this new sense, and to compare it with what she had been accustomed to acquire by her sense of touch. When, therefore, the experiment was made of giving her a silver pencil case and a large key to examine with her hands; she discriminated and knew each distinctly; but when they were placed on the table, side by side, though she distinguished each with her eye, yet she could not tell which was the pencil case and which was the key.

"Nothing farther occurred in the history of this lady's case worthy of notice till the twenty-fifth day after the operation. On that day she drove in a carriage for an hour in the Regent's Park, and on her way there seemed more amused than usual, and asked more questions about the objects surrounding her, such as 'What is that?' it is a soldier, she was answered; 'and that, see! see!' these were candles of various colours at a tallow chandler's window. 'Who is that, that has passed us just now?' it was a person on horseback: 'but what is that on the pavement, red?' it was some ladies who wore red shawls. On going into the Park, she was asked what she saw particularly, or if she could guess what any of the objects were. 'Oh yes,' she replied, 'there is the sky, that is the grass; yonder is water, and two white things', which were two swans. On coming home along Piccadilly, the jewellers' shops seemed to surprise her much, and her expressions made those around her laugh heartily.

"From this period till the time of her leaving London on the 31st of March, being forty-two days after the operation, she continued almost daily to gain more information of the visible world, but she had yet much to learn. She had acquired a pretty accurate notion of colours and their different shades and names; and when she came to pay me a farewell visit, she then wore a gown, the first of her own choice, with the light purple colour of which she seemed highly gratified, as well as with her cap, which was ornamented with red ribbons. She had not yet acquired any thing like an accurate knowledge of distance or of forms, and up to this period she continued to be very much confused with every object at which she looked. Neither was she yet able, without considerable difficulty and numerous fruitless trials, to direct her eye to an object; so that when she attempted to look at any thing, she turned her head in various directions, until her eye caught the object of which it was in search. She still entertained however the same hope which she expressed soon after the operation, that when she got home her knowledge of external things would be more accurate and intelligible, and that when she came to look at those objects which had been so long familiar to her touch, the confusion which the multiplicity of external objects now caused, would in a great measure subside."

Such is Wardrop's account. In this report it should be recalled that for several days before the last operation, when, of course, power of vision had not yet been completely restored, the patient tried to see her hands, having therefore learned perhaps to perceive them in the field of view and to follow their movements with her eyes. Even before this she had probably learned to turn her eyes toward the sun, that is, she may have succeeded in directing her gaze to a certain extent and in obtaining some idea as to the direction from which the light came that affected her eyes. The optical images in her eye must have been fairly good, because from her carriage in the middle of the street she could recognize the figures and hands of a watch, a rental sign on a window opposite, wax candles, and jewelry in a shop window. The first objects she learned to distinguish were things in motion, especially human figures, and objects that were conspicuous in colour, such as reddish doors, oranges, and women's coloured frocks. Incidentally, it is very noticeable how much sooner little babies learn to recognize human forms and faces, and to follow them with their eyes, than they do in the case of other objects. Naturally, forms of persons are more interesting and attractive than other things, and are distinctly different from the other objects in the field of view owing to the kind of motions they make. These motions have too a connected character; and the face, appearing as a pale reddish spot with its two brilliant eyes, is always a place in this image that can easily be recognized again, even by anybody who has only seen it a few times.

So far as distinguishing forms is concerned, which is the principal consideration for us here, it is obvious that in a case of this kind the main difficulty is necessarily in learning to understand the varying perspective projections of material objects. For, of course, a blind person knows nothing whatever about the possibility of such a projection. But various phases of the record indicate that the lady was not able to recognize even such forms as were not focused by perspective projection, for example, things like the key and the pencil-holder. The surface of the key, with its ring and tag, must have been represented on the retina in the same form as it feels to the touch. And so if there were an innate power in the retina of recognizing the forms of images there, according to the intuition theory, the key on the ring would necessarily have been recognized. Besides, there is the inability, frequently mentioned above, of finding the place where an object is with the eye and the hand, when the object is seen indirectly. If the directions of the lines connecting a peripheral image on the retina with the central image were known already by innate apperception, there might not have been any very great difficulty about

guiding the eye along the connecting line, following the series of images on it, until the desired point was reached.

On the other hand, apparently there is no doubt that eighteen days after her operation this lady knew how to distinguish simple forms. If the eye is allowed to run along the circumference of a circle and the edge of a rectangle and of a square, doubtless, the power will soon be acquired of recognizing, under similar circumstances, the difference between a rectilinear contour and a curved one, and of knowing what a corner is, and whether the eye is made to move mainly up and down or to the right and left, etc.; that is, of knowing whatever is needed for the recognition of such figures as those mentioned. All that is necessary for this purpose is to guide the eye along a continuous contour line, which is easier, of course, than to turn it toward a distant object in the periphery of the field of view. Moreover, the recognition of her brother's nose as being a projection in the reddish spot that constituted his face in the field of view may be similarly explained. The watch she examined the first evening was held in her hand, and could therefore be recognized also by touch. She did not describe the figures and hands as what they were, but simply observed that they were places marked on the face of the watch, not being able to tell what they were by touching them, because the crystal prevented that. She could point to these parts, by moving the image of her finger, which she already knew, to where the image of the dark objects were.

On the other hand, the rapidity with which the patient learned to see some things was apparently too great for us to assume that the local signs of the retinal points are disconnected and unsystematic signs, whose connection with the adjacent retinal points can only be acquired by experience. But if the local signs are themselves magnitudes varying continuously over the field of the retina, then in advance, without experience, adjacent points of the retina would have the characteristic in the sensation of being adjacent. It is only when this is the case that the impression of an illuminated area of the retina can be considered the same as the illumination of a continuous surface on the visual globe, unless previous experience has shown that the local signs belong to connected nerve-terminals of the stimulated fibres of the retina, and are not distributed over the field as if they were separate points.[1]

[1] Other cases: Grant in Voigts *Mag.* IV. 1. S. 21.—Hofbauer, *Beiträge* II. 2. S. 249.—Ware, *Phil. Trans.* 1801, p. 332.—Home, *Phil. Trans.* 1807. I. p. 834; *Bibl. Britann.* XXXVII, p. 85. 1808.—Trinchinetti in *Arch. des sc. phys. et nat. de Genève.* VI. 336; and *Giorn d. ist. Lomb.* fasc. 46 e 47.

[As to more recent examples of persons learning how to see after having been operated on, see Appendix at end of this volume.—K.]

Historical. Among the so-called "sensationalist" philosophers of the eighteenth century there was very active discussion as to whether the knowledge of measurements of the field of view was innate or acquired. MOLYNEUX raised the question whether a person, who was born blind, and who had learned by touch to tell the difference between a cube and a sphere, would also be able immediately to tell them apart by sight when vision was acquired. Both MOLYNEUX and LOCKE[1] answered it in the negative. While JURIN[2] concurred with them, he remarked that if the person who was born blind were to view cube and sphere from different directions, the sphere would always give him the same images, but the cube would give different images, and so he might possibly distinguish them in this way. Perhaps until the end of the eighteenth century, as long as attention was directed to this question, the prevailing opinion was that all knowledge of form in the visual perceptions was dependent on experience and comparison with the sense of touch. Under the influence of KANT's theory, that space is an innate form of our apperception, JOHANNES MÜLLER[3] advocated the opposite view. His idea was that feeling and seeing depend on the same fundamental apperceptions of the extension of our own organs in space. Thus, he starts with the assumption that we come into the world with an innate knowledge of the dimensions in space of the sensitive portions of the retina and of their arrangement, and that by this means the original measurements of the visible superficial image are given directly in the sensation. But external vision, judgment of distance and material form of objects are supplied by experience. What MÜLLER means by "external vision" is the perception of objects as being external to our own body. Certain portions of the body are seen always or are constantly recurring as depicted on the field of the retina, and we realize that they belong to us and can be directly moved by the will. The rest that we see is variable, and so we see it as being outside the body, and not part of it. Afterwards we learn to combine in the idea the two localizations by means of the sense of touch of the skin and the sense of sight. Yet MÜLLER realizes that this must seem queer from his point of view; and he compares it with the perceptions that may occur as the result of the action of the sense of touch along with that of looking at a reflected image of ourselves (as in shaving). With regard to the problem of erect vision by an inverted retinal image, MÜLLER maintains that everything is really upside-down to us, and the reason there is no contradiction is simply because our own bodies and the places on it indicated by the sense of touch are all apparently inverted also. Strictly speaking, therefore, according to this view, the images are not projected into the external world by our imagination, but the space of which there is apperception is an inner space in which the perceptions of things as being elsewhere are inserted. UEBERWEG[4] developed this part of MÜLLER's theory still farther; but HERING[5] extends this so-called apperception-space to a space of three dimensions, supplementing it by certain peculiar hypotheses, so as to enable the third dimension of this space to originate in the apperception. We shall speak of them in subsequent chapters. In his chapter on monocular stereoscopy, HERING also adheres throughout to the view that the retina sees itself in its space-relations

[1] *Essay concerning human understanding.* B. II. Ch. 9. §8. See also BERKELEY, *New Theory of Vision* 1709. §79.

[2] SMITH, *Opticks, Remarks*, p. 27. Also PRIESTLEY, *Geschichte der Optik*. II. 512 (German ed.).

[3] *Zur vergleichenden Physiologie des Gesichtssinns.* Leipzig 1826.—*Handbuch der Physiologie des Menschen.* Coblenz 1840. Bd. II. S. 362.

[4] *Zeitschrift für rationelle Medizin.* R. 3. Bd. V. S. 268–282.

[5] *Beiträge zur Physiologie.* Leipzig 1864.

so perfectly that even the distances of points on it are estimated by the chords instead of by the arcs. This view, which is not needed to explain the illusions of vision that it is intended to explain, has been alluded to above. Apparently, it is in direct conflict with the assumption made in §§118, 124 of HERING's same book, where it is stated that a plane is the apparent place of points seen by both retinas concordantly and identically.

A direct knowledge of distances on the retina as the basis of the distribution of the visible points on the visual globe is likewise fundamentally involved in those theories which assume that the images are intuitively projected directly outwards in certain definite directions. PORTERFIELD[1] and BARTELS[2] made this projection take place along the normals to the retina; but VOLKMANN[3] used the so-called lines of direction that pass through the posterior nodal point. Thus on either of these assumptions, estimation of angular distances on the visual globe is the result of intuitive impulses. TOURTUAL's views were of a similar nature.[4] VOLKMANN afterwards was still more specific in his ideas, inasmuch as he believed that the apparent size of the visual angle in the field was dependent on the number of the various sensitive nervous elements that were disposed over the corresponding extent of the retina.[5] This conception is the basis of a great many recent works on the physiology of the eye. For example, it is employed especially by RECKLINGHAUSEN[6] in giving the explanation of the deviation of the apparently vertical meridian and of other optical illusions, where he attempts to prove the possibility of corresponding distortions in the retinal image.

Meanwhile, physiologists began to espouse the earlier and contrary view, which maintained that all judgments of space were due to experiences. On the philosophical side the way was prepared by HERBART's method of regarding the perceptions of the senses. As the result of his metaphysical principle of the unity of the mind (*Seele*), he was led to explain all ideas as being qualitative processes, occurring successively in time and not existing side by side. Consequently, all apperception of space was necessarily derived from motion, and the local differences of sensation were bound to be qualitative. It was LOTZE in particular, who tried to apply these views to the actual facts in the sense-perceptions. On the physiological side, he was supported first by MEISSNER[7] and CZERMAK[8] in their researches with respect to the sense of touch. In physiological optics attention was first turned in this direction by the study of the ocular movements. One of the first steps was made by BRÜCKE, whose views in regard to the influence of movements of the eyes in stereoscopic vision will be discussed in the subsequent chapters. I myself have presented the matter from this standpoint in a popular lecture.[9] W. WUNDT[10] deserves the credit of having made the first more complete attempt to deduce the formation of the visual globe from experiences of

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[1] *On the eye.* II. 285.

[2] *Beiträge zur Physiologie des Gesichtssinns.* Berlin 1834.

[3] *Beiträge zur Physiologie des Gesichtssinns.* Leipzig 1836.

[4] *Die Sinne des Menschen.* Münster 1827.

[5] *Berichte der Kgl. Sächs. Ges. der Wissenschaften.* 30. April 1853.

[6] *Archiv für Ophthalmologie.* V. 2. S. 127.—POGGENDORFFS *Annalen.* CX, 65–92.

[7] *Beiträge zur Anatomie und Physiologie der Haut.* Leipzig 1852. –*Zeitschrift für rationelle Medizin.* R. 2. Bd. IV. S. 260.

[8] *Sitzungsberichte der K. K. Akademie der Wiss. zu Wien.* 1855. XV. 466, and XVII. 577.—MOLESCHOTTS *Untersuchungen zur Naturlehre des Menschen.* I. 183.

[9] *Über das Sehen des Menschen.* Leipzig 1855.

[10] *Beiträge zur Theorie der Sinneswahrnehmung.* Leipzig and Heidelberg 1862. Reprint from *Zeitschr. für rat. Medizin.* 1858–1862.

motion. The very existence of this important problem had been practically forgotten entirely. According to WUNDT, the local signs here were the qualitative variations of sensation in different parts of the retina, such as were observed by PURKINJE, AUBERT and SCHELSKE, and have been described in Vol. II, pp. 154, 155. In the above treatment of the subject I have not used this theory, because I do not see, for instance, how the impression of black in the centre of the field can be locally distinguished from that of red in the periphery, if there is no other sign there for perceiving the local difference except the qualitative difference that makes red look red in the centre of the field and black on the margin of it. Judgment of distances on the visual globe is deduced by WUNDT from the feeling of muscular effort required to make the eye travel over them. Experience shows that there is no certainty about judgment of muscular efforts, unless their effects are constantly compared with the visual images; and therefore I have begun with possible experiences as to the congruence of equal lines extending in the same direction. In my opinion this assumption is practically established by the experience that exact and sure comparisons can be made between lines in the same direction, whereas they cannot be made when they are not in the same direction. It is true that this does not preclude the possibility that the sense of muscular effort as claimed by WUNDT may also be a contributory cause.

Investigations of the accuracy of the eyesight were first started as the result of E. H. WEBER's law,[1] which FECHNER[2] afterwards spoke of as a psycho-physical law, and according to which the least perceptible differences are proportional to the entire magnitude of the sensation. Besides the two authors above named, VOLKMANN[3] should be specially mentioned also as having made an extended series of careful measurements. The effect of the time that elapses between two comparisons of this kind was investigated by F. HEGELMAYER.[4]

The constant errors in the comparison between horizontal and vertical distances were first noticed by A. FICK[5]; and the constant deviation of the apparently vertical meridian by RECKLINGHAUSEN.[6] The latter observed also the apparent curvature of straight lines in the peripheral parts of the field. ZÖLLNER[7] called attention to the visual illusions in line-drawings; and the consequences of this discovery were investigated further by HERING,[8] A. KUNDT,[9] and AUBERT.[10]

The earlier history and literature concerning investigations in connection with the blind spot, having to do mainly with demonstrating the fact itself and with the physiological explanation of blindness, has been given in Vol. II, pp. 42-44. The study of the question as to how the gap was filled out in the conception of the field was begun by E. H. WEBER's investigations.[11] These

[1] Über den Tastsinn and das Gemeingefühl. S. 559 in WAGNERS *physiologischem Wörterbuch.—Programmata collecta*, Fasc. III. 1851.—*Berichte der Sächs. Ges.* 1852. S. 85 ff.

[2] *Elemente der Psychophysik.* Leipzig 1860. Bd. I. S. 211–236.

[3] *Berichte der Sächs. Ges.* 1858. S. 140.—*Physiologische Untersuchungen im Gebiete der Optik.* Leipzig 1863. Heft I. S. 117–139.

[4] VIERORDTS *Archiv* XI S. 844-853.

[5] *De errore quodam optico asymmetria bulbi effecto.* Marburg 1851. See also *Zeitschrift für rationelle Medizin.* R. 2. Bd. II. S. 83.

[6] See contributions cited above.

[7] POGGENDORFFS *Annalen.* CX. S. 500–523.

[8] *Beiträge zur Physiologie.* Leipzig 1861. Heft I. S. 65-80.

[9] POGG. *Annalen* CXX. S. 118.

[10] *Physiologie der Netzhaut.* Breslau 1865. S. 269–271.

[11] Über den Raumsinn und die Empfindungskreise in der Haut und im Auge. *Verh. der Sächs. Ges.* 1852. S. 138.

were supported by A. Fick, P. du Bois Reymond,[1] and Volkmann,[2] who noticed that the localization was practically always correct in the case of objects seen in the field around the gap, and gave a psychological explanation of the way the gap was filled out. On the other hand, Wittich[3] published his observation of false localizations, and Funke[4] called attention to the possibility and occurrence of individual discrepancies in this connection.

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— Locke, *Essay concerning human understanding.* B II. Ch. 9. §8.

1738. Smith, *Optiks. Remarks.* p. 27.

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1849. Waller, Sur un cas, où la vue altérée faisait voir les objets plus petits que nature. *Institut.* XVII. No. 787, p. 39.

1851. E. H. Weber, *Programmata collecta.* Fasc. III.—Über den Tastsinn und das Gemeingefühl. S. 559 in R. Wagners *Wörterbuch der Physiologie.*

— Fick, *De errore quodam optico asymmetria bulbi effecto.* Marburg. See also *Zeitschr. für ration. Medizin.* 2. Bd. II. S. 83.

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— A. Fick and P. du Bois Reymond, Über die unempfindliche Stelle der Netzhaut im menschlichen Auge. Müllers *Archiv für Anat. und Physiol.* 1853. S. 396–407; Fechner, *Zentralblatt.* 1854. S. 57–72.

— A. W. Volkmann, Über einige Gesichtsphänomene, welche mit dem Vorhandensein eines unempfindlichen Fleckes im Auge zusammenhängen. *Berichte der Sächs. Ges.* 1853. S. 27–50.—Fechner, *Zentralblatt.* 1854. S. 57–72.

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[1] Müllers *Archiv für Anat.* 1853. S. 396.

[2] *Berichte der Königl. Sächs. Ges.* 30. April 1853. S. 40.

[3] *Archiv für Ophthalmologie.* IX, 3. 1863. S. 1–31.

[4] *Berichte der Naturforschenden Gesellschaft zu Freiburg i. Br.* Bd. III. Heft 3. pp. 12, 13.

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— Ueberweg, Zur Theorie der Richtung des Sehens. *Zeitschr. für ration. Medizin.* 3, Bd. V. S. 268–282.
1859. F. v. Recklinghausen, Netzhautfunktionen. *Archiv. für Ophthalmologie.* V, 2. S. 127–179.—Poggendorffs *Ann.* CX. 65–92.
— Hegelmayer, Über Sinnengedächtnis. Vierordts *Archiv.* XI. S. 844–853.
1860. F. Zöllner, Über eine neue Art von Pseudoskopie. Pogg. *Ann.* CX, 500–525. —*Cosmos.* XVIII. 289–290.—*Zeitschr. für Naturw.* XVI. 60–63.
1861. E. Hering, *Beiträge zur Physiologie.* Leipzig. Heft 1. S. 65–80.
— E. Mach, Über das Sehen von Lagen und Winkeln durch die Bewegung des Auges *Wien. Ber.* XLIII, 2. S. 215–224.
— F. Zöllner, Über die Abhangigkeit der pseudoskopischen Ablenkung paralleler Linien von dem Neigungswinkel der sie durchschneidenden Querlinien. Pogg. *Ann.* CXIV. 587–591.
1861. E. Bacaloglo, Über die von Herrn Zöllner beschriebene Pseudoskopie. Pogg. *Ann.* CXIII, 333–336.—*Zeitschr. für Naturw.* XVIII, 445.
1862. Wundt, *Beiträge zur Theorie der Sinneswahrnehmungen.* Leipzig and Heidelberg 1862. Reprint from *Zeitschr. für ration. Medizin* 1858–1862.
1863. A. W. Volkmann, *Physiologische Untersuchungen im Gebiete der Optik.* Leipzig 1863. Heft I. 139 180.
1864. v. Wittich, Studien über den blinden Fleck. *Archiv. für Ophthalmologie.* IX, 3. S. 9 46.
— O. Funke, Zur Lehre vom blinden Fleck. *Berichte der naturf. Ges. zu Freiburg im Breisgau.* Bd. III. Heft 3.
1865. Aubert, *Physiologie der Netzhaut.* Breslau 1865. S. 296–271.

Notes by v. Kries on §28

1. Among more recent investigations of the accuracy of the eyesight in the sense here considered [see page 170], may be mentioned the experiments of Binet,[1] of Binet and Henri,[2] and of Giesing[3] (who were particularly interested in ascertaining the efficiency of school children in this respect), and also those of Richter and Wamser[4] (which are concerned partly with adults and partly with school children).

With reference to what is said in the text as to how we go to work unconsciously in comparing linear magnitudes, especially parallel lines,

[1] *Revue philosophique*, 1890.
[2] *Revue scientifique*, 1894.
[3] *Zeitschrift für Psychologie.* XXXIX. 1905. S. 42.
[4] *Zeitschrift für Psychologie.* XXXV. 1904. S. 321.

the question may be raised as to how the eyesight works when the conditions are at all different from those specified. In this connection MÜNSTERBERG's experiments[1] are of interest. He showed that when ocular movements were not involved, and when therefore the basis of the comparison was simply the size of the retinal images, as a matter of fact, the accuracy was considerably affected. For example, the variable error in his focusings increased from 2.1 percent when his eyes were free to move to 4.3 percent when they were kept steady.

On the other hand, I have carried out experiments[2] in which it was not possible to estimate by the size of the retinal images, and where the only factor that could be used to determine the size of the impression was the extent of the excursions of the eye. It is true, the task in this case was not a comparison in the proper sense, but consisted in adjusting from memory an interval of a given length (50 mm). The method of doing it was to move the point of a needle between two sights whose distance apart could be varied, the exercise being to make the distance traversed by the needle equal to 50 mm. When the extremities of the path are not marked, the only thing to determine the size of the impression under these circumstances is the excursion of the eye in following the moving object. The variable error of the settings, which by the ordinary method was 1.78 percent, was found to have increased on the average to 3.26 percent, that is, nearly double as much.

Thus it is evident why the best results are obtained under ordinary conditions when both factors, namely, size of retinal images and ocular movements, act together.[3]

2. Much recent work on the ocular estimate of angles [see p. 174] has been done by JACKSON, GUILLERY, and BIEHLER.[4]

There are two fundamentally different kinds of problems to be distinguished from each other in this case. One is the absolute recognition of perfectly definite mathematical forms (the best examples being the right angle and the straight angle, that is, rectilinearity), and the other is the comparison of two angles of any size.

[1] MÜNSTERBERG, *Beiträge zur experimentellen Psychologie.* II. 1889. S. 164.

[2] v. KRIES, Beiträge zur Lehre vom Augenmass. *Beiträge zur Psychologie und Physiologie der Sinnesorgane,* HELMHOLTZ-*Festschrift* 1894.

[3] Strictly speaking, these experiments of mine really belong to the next chapter, where the effect of ocular movements on the perception of directions is considered. But since these directions are just the things that are involved in the measurement of the monocular field of view, it is proper to mention the experiments here also.

[4] J. JACKSON, On the judgment of angles and position of lines. *Amer. Journal of Psychology.* V, 2. 1893. S. 241.—GUILLERY, PFLÜGERS *Archiv.* LXXV. 1899. S. 466.—W. BIEHLER, *Beiträge zur Lehre vom Augenmass für Winkel.* Dissert. Freiburg 1896.

As to the first problem, GUILLERY found that a bend in a straight line could be detected, when the angle between the two adjacent portions amounted to only 23 minutes.

BIEHLER tried to make a right angle between two lines when one of them (the fixed side) was adjusted at different inclinations to the vertical. The following table gives the constant and variable errors found in this case (the vertical position being denoted by 0°, and clockwise deviations being reckoned positive).

Position of the given line	Constant error	Variable error
0°	−0°19′	0°13′
30°	+2° 8′	0°36′
45°	+4°18′	0°39′
70°	+2°57′	0°23′
90°	+1°54′	0°12′
110°	−0°42′	0°20′
135°	−3°32′	0°26′
150°	−2°28′	0°17′

Aside from the differences from the average as expressed by the constant errors, it is noteworthy that there was least certainty about the adjustment, and the greatest variable errors were obtained, for the oblique positions of 45° and 135°.

In the second type of exercises, BIEHLER tried to test the actual comparison of angular dimensions (without being aided by parallelism of the sides). His method consisted in trying to copy angles of various sizes, when the copy was turned about 45° with respect to the original. It was found that this exercise could be accomplished best when the angles to be drawn were in the neighbourhood of 90° or 180°. On the other hand, both the constant and the variable errors were greatest with angles around 60° and 140°.

3. The illusions of the eyesight, especially those that are observed (independently of their distance-relations) in certain peculiar kinds of drawings on a flat surface [see p. 198], so far as they involve comparatively simple and definite mathematical forms (and are not, say, representations of complicated objects like living figures, etc.) are usually referred to nowadays as *geometric-optical illusions*. Of recent years a vast amount of study has been bestowed on them, and they have been the subject of much theoretical discussion. It is entirely out of the question to try to give here even an approximately complete account of all this work. Although it is a subject of unusual interest

from the standpoint of psychology, that aspect of it also cannot be considered in this place. Thus, having to limit the discussion to certain portions of the matter, we shall simply complete what has been already given in the text by describing several illusions which are particularly interesting or remarkable; at the same time indicating the main principles employed in explaining them. And, in conclusion, some general observations will be made concerning the entire matter.

First of all, with respect to the examples given in the text, Zöllner's illusion [Fig. 31] in particular has been the subject of a great number of investigations concerning its extent and the conditions of its production. In this connection the first thing to be said is, that (contrary to Helmholtz's experience) Hering[1] finds that the illusion persists in the after-image; and if that is the case, ocular movements would not be a controlling factor in its production.

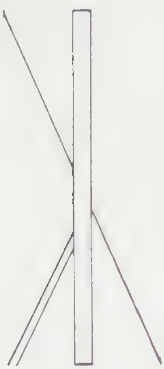

Fig. 39.

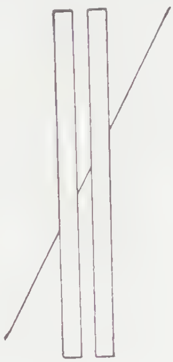

Fig. 40.

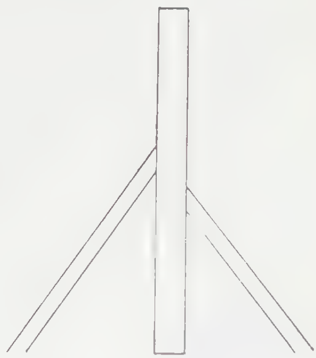

Fig. 41.

The conditions were modified by Witasek[2] in such fashion that, while the parallel vertical bands were seen by one eye, the system of oblique lines was seen by the other eye, and thus Zöllner's figure was the result of binocular fusion. Under these circumstances, he found that the illusion was much reduced in amount. He connected this fact with some extremely significant considerations which will be mentioned hereafter in the Appendix.

Some observations have been published by Benussi[3] concerning the effect of colour on Zöllner's illusion.[4]

[1] Hering, *Beiträge zur Physiologie.* Heft 1. 1861.

[2] Witasek, *Zeitschr. f. Psychologie* etc. XIX. 1899. S. 81.

[3] Benussi, Über den Einfluss der Farbe auf die Grösse der Zöllnerschen Täuschung. *Zft. für Psychologie* etc. XXIX. 1902, pp. 264 and 385.

[4] ¶See also F. Giese, Untersuchungen über die Zöllnersche Täuschung. *Psychol. Stud.*, 9 (1914), 405-435. (J. P. C. S.)

The so-called POGGENDORFF illusion has been studied in a whole series of modifications.[1] The upper left-hand portion of the interrupted straight line in Fig. 39 is apparently not the continuation of the lower portion on the right, but is situated too high. Moreover, the impression we have is that it is not the lower one of the two lines on the left-hand side, but the upper one, that is aimed toward the point where the straight line on the other side meets the vertical line. The middle portion of the inclined (broken) line in Fig. 40 apparently is not in the same direction as the two outer portions, but is turned clockwise with reference to them. More striking still, is the illusion in Fig. 41, where the oblique lines on the right-hand side are really directed to the points where those on the left-hand side meet the upright column; although this is contrary to the impression produced by the figure.

It may be regarded as a sort of contrast, when a given magnitude is apparently diminished by being in the vicinity of larger magnitudes, or is apparently magnified in the presence of smaller magnitudes.

Fig. 42.

A case of this kind is represented by one of the illusions described by MÜLLER-LYER,[2] where a given line-segment is apparently shorter when it is part of a long line, and apparently longer when it is part of a short line (Fig. 42). Here belongs also the illusion BALDWIN describes (Fig. 43), where the central dot is apparently a little nearer the large black circle on the left than the smaller one on the right. Contrast phenomena are likewise responsible for an illusion described by LOEB.[3] In Fig. 44 the upper one of the pair of lines on the left seems to be distinctly higher than the lower one of the pair on the right. Moreover, the rule given by LOEB (which will be referred to hereafter) comes under the principle of contrast.[4]

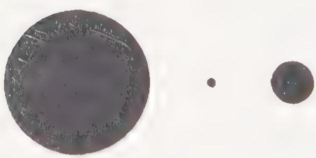

Fig. 43.

[1] DELBOEUF, Notes sur certaines illusions d'optique. *Bulletins de l'acad. roy. de Belg.* 2me S. XIX. S. 195. 1865. Seconde note sur de nouvelles illusions d'optique, *Ibid.*, XX. S. 70. Une nouvelle illusion d'optique, *Ibid.*, XXIV. S. 545. 1893.

[2] MÜLLER-LYER, Über Kontrast and Konfluxion. *Zeitschr. f. Psych.* IX. S. 1 and X. S. 421. 1894. *Idem*, Optische Urteilstauschungen. *Archiv. f. Physiologie.* 1889. Suppl. S. 263.

[3] LOEB, Über den Nachweis von Kontrasterscheinungen im Gebiete der Raumempfindungen des Auges. PFLÜGERS *Archiv* LX. S. 509. 1895.—Idem, Über Kontrasterscheinungen im Gebiete der Raumempfindungen. *Zeitschr. f. Psych.* 16. S. 298. 1898.

[4] ¶See R. PINTNER and M. M. ANDERSON, The MÜLLER-LYER illusion with children and adults. *J. of Exp. Psychol.*, 1 (1916), 200-210. (J. P. C. S.)

On the other hand, in a certain way opposed to the condition of contrast, there are other cases in which figures in space appear to unite to produce a total impression, the result being that connections involved in the process are translated to parts of the resultant figure where they are not suitable. This effect is known as *confluence* (*Konfluxion*), to use the term proposed by MÜLLER-LYER (*loc. cit.*). Perhaps this is the place to mention the illusion called by the name of this author (Fig. 45). The two vertical lines are really of the same length; but the one on the right appears considerably longer than the one on the left owing to the forked terminals at their extremities.

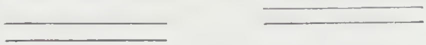

Fig. 44.

Here likewise seems to be the proper place to refer to the illusion given by BOURDON,[1] in which the contour of the figure (Fig. 46) appears to be convex upward, although the upper edge is straight.

A very striking illusion is given by the two congruent figures shown in Fig. 47, the upper one of the two being apparently distinctly

Fig. 46.

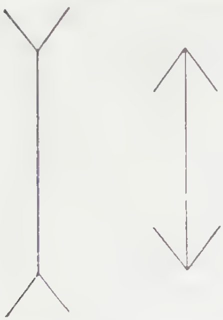

Fig. 45.

Fig. 47.

the larger one. The same sort of thing has been described in a great variety of modifications (trapezoids, etc.). There are certain special kinds of illusions connected, not with rectilinear and angular magnitudes, but with *curvatures*; for which HÖFLER[2] has suggested the descriptive term of *curvature-contrast.*

Metrical determinations of geometric-optical illusions interested BURMESTER[3] and HEYMANS,[4] the extent of the illusion being considered as depending on a number of variables in the eye.

[1] BOURDON, *La perception visuelle de l'espace*, 1902. (¶See also: B. R. RUBIN and H. P. WELD, A preliminary study of the BOURDON illusion. *Amer. J. of Psychol.*, 35, 1924, 272-279.—J. P. C. S.)

[2] HÖFLER, Krümmungskontrast. *Zeitschr. f. Psych.* X. S. 99.

[3] BURMESTER, Beitrag zur experimentellen Bestimmung geometrisch-optischer Täuschungen. *Zeitschr. f. Psych.* XII. 1896. S. 355.

[4] HEYMANS, Quantitative Untersuchungen über die ZOELLNERsche und die LOEBsche Täuschung. *Zeitschr. f. Psych.* XIV. 1897. S. 161.

The most important conclusions to be drawn from these effects are those that are connected with definitely known physiological relations.

Here is to be included HERING's theory (mentioned above on page 202, and, in my opinion, rightly regarded as not being relevant), according to which the impression of size is not concerned with the angular distance between the retinal images (or with the arc), but with the length of the chord connecting them.[1]

The importance of *irradiation*, to which HELMHOLTZ himself has alluded [page 195], has since been emphasized, especially by LEHMANN[2] and MÜNSTERBERG.[3] In accordance with his whole theory of the space-sense, WUNDT[4] endeavoured above all to establish a connection between the geometric-optical illusions and the movements of the eye. Moreover, the explanation that DRESSLAR[5] tried to give of the POGGENDORFF illusion starts out with certain connections of the ocular movements. Similarly, KIESOW's explanations[6] are based on the relations of impulses of motion.

Another consideration, which certainly deserves to be taken into account, and to which attention was called first by EINTHOVEN,[7] is the question of the vague perception of objects by indirect vision. Since "in determining the location of a figure that is vaguely perceived it is not possible to be guided by the centre of its retinal image," figures or parts of figures may be more or less shifted in indirect vision. Obviously, we can try to establish a connection between this fact and the phenomena known as "confluences."

Of the other modes of explanation, the one that may be mentioned here first of all is that of *contrast*, which has been employed by a great

[1] To avoid all misunderstanding, the point made here is that HERING's explanation of the so-called KUNDT illusion depends on the assumption of a certain lack of symmetry of the space-values, and has nothing to do with the idea referred to above. In my opinion this illusion is absolutely and essentially of a different character from the geometric-optical illusions, and, consequently, it has not been mentioned at all in the above discussion. It will be discussed later, along with HERING's explanation of it.

[2] LEHMANN, Irradiation als Ursache geometrisch-optischer Täuschungen. PFLÜGERS *Archiv.* CIII. 1904. p. 84.

[3] MÜNSTERBERG, Die verschobene Schachbrettfigur. *Zeitschr. f. Psych.* XV. 1897. p. 184.

[4] WUNDT, Die geometrisch-optischen Täuschungen. *Abh. d. Kgl. Sächs. Ges. d. Wissensch. Math.-physikal.* Kl. XXIV. 2. 1898. Idem, Zur Theorie der räumlichen Wahrnehmung. Philos. Studien. 14. pp. 1–119. 1898.

[5] DRESSLAR, A new illusion for touch and an explanation for the illusion of displacement of certain cross lines in vision. *Amer. Jour. of psychol.* VI. 1894. p. 275.

[6] KIESOW, Über einige geometrisch-optische Täuschungen. *Arch. f. d. ges. Psych.* VI. p. 289.

[7] EINTHOVEN, Eine einfache physiologische Erklarung für verschiedene geometrisch-optische Täuschungen. PFLÜGERS *Archiv.* LXXI. 1898. p. 1.

many authors in a variety of forms and special applications. Here let me say that, as a matter of fact, the formulation of this principle as given by HELMHOLTZ is somewhat unfortunate and not altogether satisfactory. What he says is, that "in all perceptions of the senses distinctly perceptible differences appear to be larger than differences of the same objective size which are only vaguely perceived" (page 192).

In my judgment this is not in accordance with what is meant by contrast in ordinary language. It is a conception involving something besides that is of more significance for our purpose. Contrast invariably implies the modification of an impression that is the result of an antagonism or difference. Starting from this, I should say that the law of contrast may be expressed somewhat as follows: *Wherever a series of ideas varying continuously in content can be arranged in a series* (such that the transition from each element to the next in order seems to us to be a variation of the same sort or in the same sense), *we find that the impression of the single element is* (actually or apparently) *modified, when it is produced in temporal or spatial proximity with another element, being apparently displaced in the direction in which it is diverted by this other element.* It is in accordance with this rule that medium grey looks brighter when it is seen after or along with black, or darker when it is seen after or along with white. But it is the same reason that makes a set of parallel lines look divergent by the side of numerous impressions of a convergence taking place in the given sense, or that makes parallel lines appear convergent by the side of the impression of divergent pairs of lines. If we start from this conception, evidently the principle involved is one of extremely wide application. Indeed, its significance extends to all cases in which we can arrange the content of ideas in a continuous series proceeding in the same sense or in which the various stages are apparently in the same sense; and the fact that it occurs to such an exceedingly great, nay, absolutely unlimited extent, is connected with fundamental psychological facts, which, however, cannot be discussed here. Thus there is not only contrast between one place and another, but also between one direction and another, between convergence and divergence, between big curvature and little curvature, etc.

When the real nature of contrast is regarded in this way, we see, for example, that LOEB's rule above mentioned, whereby "two points or lines of different space-values, which are simultaneous objects of attention, affect each other as if they were mutually repellent," is a direct corollary of the above definition.

Many explanations appear to me to be nothing more than other modes of expressing the principle of contrast, or as being attempts

to grasp this very idea more fully so as to understand it better, or else to find a deeper basis for it. Such are the explanations of ZÖLLNER's illusion as given by CLASSEN[1] and BRENTANO,[2] as well as by ZÖLLNER[3] himself. Perhaps HEUSE's explanation[4] of HERING's illusion [Fig. 30] may be considered also as a modification of the principle of contrast. According to it, the angles between the pairs of sloping lines apparently become smaller in the direction of their divergence, because each successive angle is included between the sides of the preceding one.

As above stated, *confluence* as a principle of explanation was proposed first by MÜLLER-LYER, who appreciated its importance. The explanation of the MÜLLER-LYER illusion that was given by BRUNOT[5] was also in accordance with this principle.

In a manner even more pronounced than in the case of the two principles last mentioned, specific *psychological* considerations become factors, in case we resort to modes of explanation involving ideas that are not actual perceptions, but the result of imagination. The first questions of this kind are such as are concerned with continuations, completions, etc., of the figures presented to the eye. From this standpoint the significance of the principle of contrast may be regarded as being essentially extended. A case in point would be to explain Fig. 47 by assuming that a divergent continuation of the upper figure is supplied by the imagination, in comparison with which the lower figure appears smaller by contrast.

Eventually, as was true especially in the case of LIPPS,[6] we arrive at notions of an altogether different kind, by admitting in the region of consideration acts of the imagination that are more complex still, and by including in the perception of figure ideas of forces and motions, involving things like growth and exertion, pushing and pulling, tensions, resistances, etc. Evidently, in this way a wide field is thrown open to the possibility of all sorts of explanations, but at the same time it will be exceedingly difficult to reach a decision as to the real interpretation.

In conclusion, brief reference will be made here to a mode of explanation which is obtained by considering that in some circumstances plane figures are apparently not flat, but show differences of

[1] CLASSEN, *Physiologie des Gesichtssinnes.* Jena 1876.

[2] BRENTANO, *Zeitschrift für Psychologie.* III. p. 349.

[3] ZÖLLNER, *Über die Natur der Kometen.* Leipzig 1872. p. 378.

[4] HEUSE, Noch einmal das ZÖLLNERsche Muster. *Archiv f. Ophth.* XXV. (1). p. 121. 1879.

[5] BRUNOT, Les illusions d'optique. *Revue scientifique.* LII. (7). 1893. p. 210.

[6] *Raumästhetik und geometrisch-optische Täuschungen.* Leipzig 1897.—LIPPS, Zur Verständigung über die geometrisch-optischen Täuschungen. *Ztschr. für Psychologie.* XXXVIII. 1905.

depth, and that the amounts of illusion may be connected with these impressions of depth. Cases of this sort are easily produced with suitable perspective drawings. A very common and familiar picture, for instance, consists in the delineation of three human figures of equal height, one above the other, which are all connected by the perspective representation of a set of steps ascending in the direction away from the spectator. The uppermost figure appears to be standing on top of the steps, and so he seems to be farthest away and also largest, while the lowest seems to be nearest and smallest.

An illusion described by BEZOLD[1] is also of this kind. Of course, certain relations between impressions of distance and of absolute size, which will be described further on, are involved in these cases. In the large majority of geometric-optical illusions the intermingling of such associations does not have to be considered.

WITASEK's noteworthy contribution referred to above is especially valuable, because it gives us a survey of this whole subject. It should be added here that in this work he endeavoured to make a distinction between two fundamentally different modes of apperception, which are the starting points of most attempts at explanation. On the basis of this consideration, he differentiated between *sensation-hypotheses* and *judgment-hypotheses*, thereby classifying the various explanations in two categories. It is true that this classification was found not to be thoroughly exhaustive, because many writers were not sufficiently clear in their expositions with respect to the points that were important for this purpose.

On the basis of the above mentioned experiments for distributing the systems of lines in ZÖLLNER's illusion between the two eyes, WITASEK himself decides in favour of the sensation-hypothesis.

Extensive critical discussions have frequently been undertaken as to the value and justification of the various principles of explanation; as will be found in most of the works that have been cited and in articles by BLIX,[2] BRENTANO,[3] V. ZEHENDER,[4] etc. As I have already stated, it does not seem to me to be advisable to go very deeply into these questions here. Perhaps the references that have been made to this subject really belong to certain perfectly general discussions as to the nature of space-perception, the connection between judgment

[1] BEZOLD, Eine perspektivische Täuschung. POGGENDORFFS *Annalen*. XXIII. 1884

[2] BLIX, Die sogen. POGGENDORFFS optische Täuschung. *Skandin. Archiv.* XIII. 1902. S. 192.

[3] BRENTANO, Über ein optisches Paradoxon. *Zeitschr. f. Psychol.* III. 1892 S. 349; V. 1893. S. 72.

[4] v. ZEHENDER, Über geometrisch-optische Täuschungen. *Zeitschr. f. Psychol.* XX. 1899. S. 65.

and sensation, and matters of that sort. Hence, it will be well to reserve these considerations for the Appendix where they can all be discussed together. We shall have occasion then to refer again to WITASEK's classification.—K.

Note (by J. P. C. S.)—The following list of more recent literature on various optical illusions may be inserted here:

V. BENUSSI, Stroboskopische Scheinbewegungen und geometrischoptische Gestalttäuschungen. *Arch. f. d. ges. Psychol.*, 24 (1912), 31-62.—O. POLIMANTI, Étude de quelques nouvelles illusions optiques géométriques. *J. de psychol. norm. et path.*, 10 (1913), 43–47.—J. W. GILTRAY and F. W. EDRIDGE-GREEN, An optical illusion. *Nature*, 93 (1914), 189–214.—R. HENNIG, Eine unerklärte optische Tauschung. *Zft. f. Psychol.*, 72 (1915), 383–386.—E. BONAVENTURA, Le illusioni ottico-geometriche. *Riv. di psicol.*, 16 (1920), 220–236.—P. WINGENDER, Beiträge zur Lehre von den geometrisch-optischen Tauschungen. *Zft. f. Psychol.*, 82 (1919), 21–66.—M. LUCKIESH, *Visual illusions, their causes, characteristics and applications.* New York, 1922.

Concerning perception of form and geometrical figures:

F. SELETY, Die Wahrnehmung der geometrischen Figuren. *Arch. f. syst. Phil.*, 21 (1915), 49–58.—M. J. ZIGLER, An experimental study of visual form. *Amer. J. of Psychol.*, 31 (1920), 273–300.—E. BECHER, W. KOHLERS physikalischer Theorie der physiologischen Vorgänge die der Gestaltwahrnehmung zugrunde liegen. *Zft. f. Psychol.*, 87 (1921), 1–44.—A. R. GRANIT, Perception of form. *Brit. J. of Psychol.*, 12 (1921), 223–247.—E. RUBIN, *Visuellwahrgenommene Figuren.* Copenhagen, 1921.—W. BLUMENFELD, Visual form. *Zft. f. Psychol. u. Physiol. d. Sinnesorg.*, 91 (1923), 1–82, 236–292.

§29. The Direction of Vision

The facts which have been discussed thus far were concerned simply with the relative positions of the various luminous points as they appeared side by side in the field of view. We have yet to consider how our judgments are formed as to their absolute directions. And here in the first place there are two things to be distinguished. In general, the direction of a line is given by two angles which it makes with the directions of certain fixed axes or planes that have been suitably chosen, without having to stipulate then that the line shall pass through a given point. All lines parallel to the first one are said to have the *same direction*. For example, all the magnetic needles suspended in a given town have the *same direction* from south to north. It is not quite the same thing to give the direction merely in general with reference to a definite system of coördinates—for example, with reference to a system defined by the plumb line in a certain town, the horizontal plane, and the terrestrial meridian there—as it is to refer the directions all to a specified central point. In the latter case the directions are indicated by perfectly definite straight lines all passing through the chosen centre, a given direction being defined by two

angles which it makes with suitably chosen fixed axes. In this case the direction cannot be indicated by another parallel line *co-directional* with it, but it must have the *same* or *identical direction*; that is, when prolonged sufficiently it must completely coincide with the first line.

Thus, as long as it is merely a question of *equality of directions*, all that is necessary is to give the angles that define the direction; but when it is a matter of *identity of directions*, the point has to be assigned also which is to act as centre. In the former case we can say that all we do is to define the *direction*, whereas in the latter case a definite *direction-line* is specified.

The so-called directions of vision are certainly referred to a centre, that is, to the observer himself and the place where he is in space. However, there are a series of phenomena that do not depend on the specification of the centre of the direction-lines, particularly, all those phenomena that are liable to occur in looking at distant objects like the stars, for instance, or even at distant mountains and buildings. For such objects are also necessarily large, and every direction-line drawn through a point in our head or body even parallel to a certain direction will proceed to the object.

Except in case of the illusions mentioned in the previous chapter, the direction of objects on the visual globe will generally be determined as soon as we know, first, the direction of the line of fixation, and, second, the direction of any meridian passing through the point of fixation.

The direction of the point of fixation varies with the position of the eye with reference to the head or with reference to the body. However, we are generally in a position to judge correctly the direction of the line of fixation each time. The sensations, which enable us to perceive changes of position of the parts of the body through muscular action are known as the *muscular feeling*. This term includes, however, several essentially different sensations that have to be distinguished. Thus, we may perceive:

1. The *intensity of the effort of will*, whereby we endeavour to bring the muscles in action[1];

2. The *tension of the muscles*, that is, the force by which they try to act; and

3. The *result of the effort*, which, regardless of its being perceived by other organs of sense, such as sight and touch, makes itself felt in the muscle by a contraction which actually takes place, and in which it may be possible to perceive after a fashion the change of tension of the skin over the parts affected.

[1] As to this matter and the allied question of the feeling of innervation, about which there has been so much discussion, see the remarks in the Appendix.—K.

Thus in case of muscles that are much fatigued, I may be able to perceive that I have to make the utmost exertion of the will to produce tension in the muscles, but that their tension is no longer sufficient to obtain the result. On the other hand, in the case of powerful muscles, with a moderate effort of will, I can produce a distinctly perceptible tension in them, yet, owing to some external opposition, without obtaining the desired result. All these cases are differentiated in my perception from the case where I actually obtain the result, and these various conditions have to be differentiated likewise in the theory of muscular feeling.

The present investigation will naturally be confined to the conditions occurring in case of the eye.

To begin with, we know by common experiences that our judgments of the direction of vision are not made by the actually existing position of our eye, when this position is varied by forces other than those of the muscles. If pressure is exerted on one part of the eyeball where it is covered by the lids, or if the skin around the eyeball is pulled, small changes will be produced thus in the position of the eyeball itself. The best way to show it is by pinching the skin at the outer corner of the eye, and then turning the eye inwards until the conjunctiva over the eyeball is stretched on the outer side. If both eyes are opened while the skin is being pulled, double images will be seen, due to the fact that the image in the eye that is tampered with is shifted in a different direction from that of the other eye. If only the former eye is opened, then with each pull on the skin, an apparent motion of the objects in the field of view will be seen to occur. Every pull on the right eye that is directed straight outwards causes the objects to move apparently to the left. The direction of the visual axis in this case is shifted to the right; but our judgments as to the positions of objects is as if the pulling had no effect on changing the direction of the visual axis.

Thus it is found that the positions of after-images in the closed eye, or as they are projected on a uniform screen of unlimited extent, appear to stay where they are while the eye is being pulled, although, as a matter of fact, they do move with the eye.

On the other hand, while the eye is being pulled in this way, every movement of the eyes produced by the muscles leaves the apparent positions of external objects unchanged, whereas the after-images seem to move.

When the eyeball is rolled outwards thus as the result of an external pull, of course, the internal rectus muscle will be elongated, and the external rectus contracted just as much as if the rolling of the eye had been produced by muscular action. For even in equilibrium the

muscles are elastic bands that always contract as far as their points of attachment will allow.

Thus, our judgment as to the direction of the visual axis is not formed either by the actual position of the eyeball or by the actual elongation or contraction of the ocular muscles that is the result of this position.

That our judgment of the direction of the visual axis is not formed by the tension of the ocular muscles, is shown by the fact that in those cases where certain muscles have suddenly been paralyzed, when the patient tries to turn his eye in a direction in which it is powerless to move any longer, apparent motions are seen, producing double images if the other eye also happens to be open at the time. For instance, if the external rectus of the right eye is paralyzed, or the nerve leading to it, this eye can no longer be pulled around to the right. As long as the patient continues to turn it inwards only, it still makes regular movements, and he perceives correctly the directions of objects in the field of view. But the moment he tries to turn his eye outwards, that is, to the right, it ceases to do his bidding, and remains standing in the middle, while the objects appear to move to the right, although the adjustment of the eye and the positions of the retinal images in it have not varied.

In a case of this kind when a muscle is paralyzed, there is no movement of the eye, no contraction of the muscles that should be contracted nor even any increase of tension in these muscles, as the result of exertion of will-power. The latter has no effect whatever beyond the nervous system; and yet our judgment as to the direction of the visual axis is formed as if the will had produced its normal effects. In the case instanced above, we fancy that the visual axis has been shifted to the right; and since no change has taken place in the positions of the images on the retina of the paralyzed eye, we get the impression as if the objects shared the supposed movements of the eye.

If the paralysis is not complete, and if, while the eye can still focus on an external object, the impaired muscle requires a greater degree of innervation than would be needed under normal conditions, a wrong idea will be obtained of the direction of the visual axis and of the position of the object; as can be seen by requiring the patient to reach for the object quickly. He will miss it at first.[1]

These phenomena prove conclusively that our judgments as to the direction of the visual axis are simply the result of the effort of will involved in trying to alter the adjustment of the eyes. It is true, there

[1] A. v. Graefe in *Archiv für Ophthalmologie.* Bd. I. Abt. 1. S. 67. Anmerkung.—A. Nagel, *Das Sehen mit zwei Augen.* 1861. pp. 124–129. Alfred Graefe in *Archiv für Opthalmologie.* XI, 2. pp. 6–16.

are also certain faint sensations in the eyelids when the cornea turns underneath them, which might give some information concerning the actual position of the eye. Moreover, in case of strenuous lateral movements of the eyes, we are aware of a feeling of fatigue in the muscles. But all these sensations are apparently too faint and too vague to be of any use for the perception of direction.

We know therefore what impulses of the will have to be employed, and how strong they must be, to bring the eye into some definitely intended position. Under ordinary normal conditions there is nothing outside to hinder the movement of the eye, and so the effect can generally be judged well enough from the force of the impulse of the will, with much more certainty at least than would be possible in case of the limbs and most of the other movable parts of the body. The sole action of the impulse of the will which is perceived in the eye directly and with sufficient clearness, is the change of position of objects on the visual globe for the new position of the eye. Now it can be shown that, as a matter of fact, these variations of the image are being continually utilized to regulate the proper relation between the impulse of the will and its effect.

Take two glass prisms with refracting angles of about 16° or 18°, and place them in a spectacle frame, with their edges both turned toward the left. As seen through these glasses, the objects in the field of view will all apparently be shifted to the left of their real positions. At first, without bringing the hand into the field, look closely at some definite object within reach; and then close the eyes, and try to touch the object with the forefinger. The usual result will be to miss it by thrusting the hand too far to the left. But after trying for some little while, or, more quickly still, by inserting the hand in the field and, under the guidance of the eye, touching the objects with it for an instant, then on trying the above experiment again, we shall discover that now we do not miss the objects, but feel for them correctly. It is the same way when new objects are substituted for those with which we have become familiar. Having learned how to do this, suppose now we take off the prisms and remove the hand from the field of view, and then, after gazing steadily at some object, close our eyes and try to take hold of it. We find then that the hand will miss the object by being thrust too far to the right; until after several failures, our judgment of the direction of the eyes is rectified again.[1]

Here it is not the muscular feeling of the hand that is at fault or the judgment of its position, but the judgment of the direction of the gaze,

[1] The experiment, practically as described here, was given by CZERMAK in *Wiener Berichte*. XVII, pp. 575–577.

as is shown by the fact that, if after having become used to looking through the prisms and finding the visible objects with the right hand, then we close our eyes and try to touch the same objects with the left hand, which has not been previously used, and which was not in the field of view, we find that there will not be any difficulty about touching them with perfect certainty and precision. Accordingly, in a case of this kind the place is determined perfectly correctly, and thereafter it can be found with certainty by another organ of touch.

We know by experience that children three months old are very slow in learning to point their hands toward objects they see, although they may know very well from the sensations of touch how to direct them to the mouth or to an itching place on the skin. They have to make many trials before they learn to understand the correspondence here between movements of eyes and hands; and so also even in the case of grown people the accuracy of this correspondence has to be continually regulated by constantly repeated experiments and observations.

It has been previously stated that the correspondence between the movements of the two eyes can be disturbed in the same way by gradually raising the image of the visual globe in one eye by the aid of a prism. Then the eye involved follows the movement, and both eyes continue to see singly, one of them being directed slightly more upward than the other. Here too it soon becomes a habit to use this adjustment as the normal adjustment for fixation; and if the prisms are removed, fixation continues to be carried on in the same way by getting double images of the objects one above the other which can only be fused quickly by varying the adjustment of the eyes. Thus it appears that the harmonious adjustment of both eyes is regulated by the result, and we accustom ourselves to giving such impulses of the will as are requisite, under the existing conditions, for directing both points of fixation on the same object.

This enables us to explain how it happens, that after having gazed steadily for a long time at objects in motion, the objects that are at rest presently seem to be going in the opposite direction. The vision of these apparent movements is what is known as *giddiness*. For example, when a person travelling on a train has been looking for some time at objects close to the track outside, and then turns to look at the floor of the carriage, although the latter is at rest relative to his body, it seems to be moving from under him in the same direction as the train. The reason of this is because there is an apparent motion of the objects on the track in the direction opposite to that of the motion of the train. Whenever the traveller tries to focus one of them,

he has to jerk his eyes quickly in the direction opposite to that of the motion of the train. Having got accustomed to regard the impulses of the will exerted under these circumstances as the correct ones for the fixation of an object, he attempts to focus stationary objects in the same manner also. But these same impulses of the will produce movements of the eyes, and as the observer considers his eyes as being fixed, not only the objects but the previously observed objective motion seem to him to be going in the opposite direction.

But if the passenger gazing out of the coach should happen to fix his attention constantly on a speck on the window, the aforesaid giddiness will not be developed, although, as before, he is aware of objects flying past him, without, however, making the movements necessary to focus them. Incidentally, moreover, when the eye is steadily focused on a point that is stationary with respect to it, the images of moving objects will be completely obliterated when the speed is such as is needed for this illusion. The only way to recognize them is by pursuing them with the eyes for short distances. The requisite movements of the eyes are not much understood, and so they have not been noticed by PLATEAU[1] and OPPEL,[2] who carried out observations on these phenomena. But that ocular movements do occur, is shown by the fact that when the focus is absolutely fixed, the moving images are obliterated.[3]

The same thing is noticed in the case of giddiness caused by whirling the body for a while around its vertical axis, keeping the eyes open all the while. The instant we pause, the objects seem to continue moving for a time in the same direction as that in which we were turning. I find that when I close my eyes and whirl round, this kind of apparent motion does not occur, provided I wait until I have really come to a dead stop before opening my eyes. But if I open my eyes too soon, the objects will appear to be turning opposite to the way I myself had just been whirling round. However, the body does not come to rest at the exact instant it is supposed to do so, but goes on turning the head through about a quadrant. And so in this case the apparent motion of the objects is due to an illusion about the time when the body itself comes to rest. Incidentally, this giddy motion in the opposite sense to the actual rotation of the body sometimes results from whirling round with the eyes open, because the test in

[1] PLATEAU in POGG. *Annalen.* LXXX. 287.—*Bull. de Bruxelles.* XVI.

[2] OPPEL, POGG. *Annalen.* XCIX. 543.

[3] The opinions expressed here as to movements of after-images have been very much modified in conjunction with decidedly different notions of the perception of movements. See Note 1 at the end of this chapter.—K.

this case is not so pure and simple as the other ones are where the observer's body does not take part in the motion.[1]

These forms of giddy vision are liable to occur also when various portions of the object have been moving in different directions. For example, if the disc represented in Fig. 53 of Vol. II (p. 217) is made to turn in the same sense as the spiral, the latter will appear to be continuously expanding or contracting, depending on the direction of its rotation. When the disc is suddenly stopped, the spiral will appear to contract or expand for a moment afterwards, just opposite to the way it had appeared to be doing before. And other objects too, such as a sheet of printed paper, for instance, or anything that happens to be observed immediately after looking at the spiral will exhibit this same sort of movement of contraction or dilatation.

Much less distinct is a similar giddy movement that is manifested after looking at a rotating star-shaped figure. In this case a really stationary object which is observed subsequently will appear to turn a little in the opposite direction to the motion of the star.

These latter kinds of giddy illusion will be produced most distinctly when the gaze is directed to the stationary central point on the axis, at the same time taking heed of the moving figure as seen by indirect vision. The pattern should not turn so fast as to make it impossible to perceive its individual motions, nor so slowly that there is no trouble at all about perceiving them. If the eye is steadily focused on the central point of the axis, without paying heed to anything else, of course, just as before, the image of the moving figure will be on the lateral portions of the retina, but the giddy movement will not occur. Hence, I think it may be inferred that in noticing the moving figure delicate movements of the eye are involved, probably circular movements directed always toward that special part of the visual globe where the attention happens to be attracted by indirect vision. Indeed, unless the moving pattern is followed by the eye in some such way, these movements could not appear so very distinctly as they do in case of the kind of observation that develops giddiness. When the

[1] More recent experiments on the static organ and the sensations released by it have led also to new views as to the phenomena of giddiness here mentioned. See Note 2 at the end of this chapter.—K.

¶ Incidentally, in connection with these particular movements, it is interesting to watch the performance of a ballet dancer. Poised on the toes of one foot, and whirling round rapidly in the centre of the stage, she will sometimes execute such an astonishing number of revolutions before pausing that it is a wonder she is able to preserve her balance and make her curtsey at the end. It is curious to notice how the head is always jerked around after each partial revolution of the body. In performing such movements the dancer is specially trained to keep the eyes steadily focused as much as possible in a definite manner. (J.P.C.S.)

same mode of observation is afterwards employed with a stationary body, naturally the latter must appear to have a motion in the opposite sense.

As long as the field of view contains a large number of stationary objects, it is easy to be constantly aware of the degree of innervation required to hold the eye in definite positions. But when most of the objects in front of us are in motion, it is difficult to judge correctly as to rest and motion. In crossing a plank over a rapid little stream, one must be careful to avoid looking at the water so as not to lose his balance. When a person steps out on one of the lower ledges of the castle Laufen at the falls of the Rhine, and sees nothing before him but the mass of tumbling water, he has a tendency to fall over backward. That is why we are so confused about our orientation on board ship. The pull of gravity seems to be first toward the right and then toward the left, sometimes forward and sometimes backward, because we can no longer tell the direction of the vertical. It is only after getting accustomed to it, as I know by experience, that one learns to use the force of gravity as a means of orientation; and then the giddiness also disappears. To the novice a barometer fastened in the cabin of a ship by "CARDAN'S suspension," seems to sway back and forth, though in reality it always hangs vertically. The cabin, on the other hand, seems to stay steady, in spite of the fact that gravity itself pulls it first one way and then the other. As soon as the giddiness is gone, the barometer is seen to be steady and the cabin to be swaying. But how much the certainty of the innervation of the ocular muscles is impaired for the time being, is shown by the fact that passengers who had been seasick on the voyage, even after they have come ashore, every time their eyes make a rapid movement, will continue to see the walls of the room in which they happen to be apparently performing the same motions as the cabin of the ship used to do.

All these phenomena distinctly show that there must be a continuous control of the amount of innervation needed to adjust the eyes and move them about, which is obtained by observing its effect on the visual images, if our judgments as to the direction of the visual axis and the objects of fixation are to be correct.[1]

Another kind of illusion that belongs here has been described by F. ZOLLNER.[2] Draw a circle on a sheet of paper, and cut a slit in a dark card, making it longer than the diameter of the circle and between one and three tenths as wide as the diameter is long. Holding the card fixed, move the piece of paper under it back and forth, until the

[1] Concerning this subject, see Note 3 at the end of this chapter.—K.

[2] Über eine neue Art anorthoskopischer Zerrbilder. POGG. *Annalen*. 1862.

circle is shoved completely past the slit, first in one direction and then in the other. Under these circumstances the circle looks like an ellipse with its axis major at right angles to the direction of motion. The reason of this is because, in trying to see the moving figure, the observer, unconsciously and without being distinctly aware of it, follows it with his eyes, but not so fast. Thus on the different strips of the retina, where the image of the slit is produced during this motion, successive impressions are formed of the arc of the circle seen through the slit at each instant, exactly as in case of the *anorthoscope*, except that there the slit itself moves while the eye is at rest, whereas here the eye moves and the slit stays still. The optical impression in this case is the same as if the motion of the slit were in the opposite direction to that of the eye, and therefore opposite also to the motion of the image. In the *anorthoscope*, as explained in Vol. II, pp. 187-189, the result is an apparent contraction of the figure in the direction of motion.

That this illusion is caused by movements of the eyes, is proved by the fact that at the speed that is most favourable for producing it none of the figure can be seen any more, the moment the eye is focused steadily on the edge of the slit. In order to be able to recognize the figure, the eye must keep following it along. Besides, a second observer can also notice such movements of the eyes as were found by ZÖLLNER.

On the contrary, when the circle goes very slowly past the slit, it appears to be elongated in the direction of the motion. That may be because, owing to the apparent magnification of the acute angles, the parts of the curve seen through the slit seem to be steeper with respect to the sides of the slit than they really are. This same thing would be the case in reality if a transversely elongated ellipse were drawn past the slit, for the observer then would interpret the figure as such an ellipse.

Having first satisfied ourselves, by the facts described above, that the harmony existing between the perceptions of sight and those of touch, even in the developed eye of an adult, is not permanently gained except by continually testing them with experience, we should have no difficulty about the explanation of the vexed question as to how objects appear to be erect, although their images on the retina are inverted. The sense of touch by itself is capable of forming perfect apperceptions of space, without any help whatever from the sense of sight. We know this from observations of persons who were born blind. Indeed, the direction of gravity, by which we define up and down, is perceived immediately by the sense of touch alone, and not by the sense of sight. That the visual sensations by themselves, without

any previous experience, should evoke ideas as to a definite direction of the object perceived, seems to me an absolutely unnecessary hypothesis. And from the point of view of the *empirical theory,* there is even less ground for the supposition that the idea of direction in this case is influenced by the spot where the image is formed on the retina and that because the image of a point was situated down below, it had to appear therefore as being down below, when, as a matter of fact, there is nothing in our natural consciousness to tell us even of the existence of a retina or of optical images on it, much less of their position.

In the *intuition theory* of sense-perceptions, where it is assumed that the stimulation of the nerve, directly and independently of all experience, will evoke the idea of a definite place where the observed object lies, of course, it has to be assumed that the intuitive localizations through the sense of sight are in some sort of intuitive harmony with those of the sense of touch. That is, we must imagine, either that the fibres of the optic nerve coming from the lower edges of the two retinas were turned upwards in the brain, resulting in the formation there of an erect image of objects, and that this is the image which the mind sees; or else, that the apperception is made to take place in the retinas, and that the tactile perceptions corresponding to the observer's own hands and legs, which are also seen inverted, are likewise made to be inverted in the perceptual image; in which case, therefore, all our notions of space would be, and would remain, upside down. Of course, in this way there is latitude for the wildest speculations.

My opinion is, that an intuitive harmony between the localizations of the two senses of sight and touch, which is contrary to the experiences that prove the reality of the permanent control by experience of the correct relations of the two senses to each other, is a position that cannot be maintained; because it involves the difficulty, that the professedly innate harmony, supposed to be the result of direct sensation, may at any moment be changed by experience, that is, by acts of judgment, and so entirely upset that nothing appreciable is left of this hypothetical sensation.

From my point of view the only interest in this question of the cause of upright vision is the psychology of why it should be so difficult even for men of considerable scientific attainments to consent to recognize really and truly the subjective factor in our sense-perceptions and to see in them effects of the objects, instead of regarding them as being unmodified reproductions or *Abbilder* (*sit venia verbo*) of the objects; which latter idea is evidently self-contradictory.

Thus far we have simply investigated the directions in which we think we can see very distant objects. The next question is to define the *centre* from which these lines of direction radiate; which is not a matter of no importance, particularly in judging of the direction of near objects. Heretofore we have usually supposed that the visual objects were projected outward by each eye along the so-called lines of direction as defined in Vol. I, p. 96; and in this case the directions in which near objects were seen, would generally be different for the two eyes. However, in this connection, E. HERING has recently called attention to a remarkable illusion, according to which the direction of the observed object is perceived as if both eyes were in the median plane of the head, and were focused on their common point of fixation.

Suppose at first that both eyes A and B (Fig. 48) are looking out in parallel directions Aa and Bb, and that then the eye B is closed, while A continues to be focused steadily on the infinitely distant object a, the directions of the two eyes remaining therefore unchanged. Under these circumstances, a will be seen in the right direction. Suppose now that A is accommodated for a much nearer point f on the line Aa, in which case there will be no change whatever in the position of the eye A or of its visual axis Aa, nor in the position of the image of a on the retina of the eye A, the latter being simply a little less sharply defined. The consequence is that an apparent movement of the object a takes place, by which it moves over, say, in the direction Ac. When the eye is again accommodated for infinity, a apparently moves back to its original place.

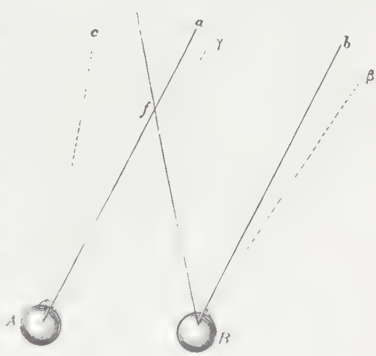

Fig. 48.

Now in this experiment the direction of the visual axis Aa is not altered at all, that is, not to any appreciable extent worth considering. All that is changed is the position of the closed eye B, because in trying to accommodate for the point f, the visual axis of the other eye will be directed to this point at the same time. Thus, while f is being focused, the visual axis of the eye B takes the direction Bf.

Conversely, it is possible for me to make the visual axes of my eyes diverge even when they are shut, so that the eye B will look in the direction $B\beta$. It takes me a long time to do it, and so I do not notice any distinct apparent movement. But there is a movement of this

sort, if I suddenly desist from trying to diverge, in which case the visual axes instantly become parallel again. Then the object a will be seen to move back from the position γ, say, to a.

Thus not only the position of the seeing eye A, but also that of the closed eye B, influences our judgment as to the direction of the object of fixation. If the open eye remains immobile, while the closed eye moves to the right or left, the object on which the open eye is focused will move apparently in the same way, that is, to the right or left.

The amount of this apparent motion is quite different for my two eyes. It is slight when the right eye is open and focused, but much greater, when the left eye is open and the right eye shut. Thus the direction of the visual axis is determined by the innervations exerted on both eyes simultaneously, and not only by the innervations on the open eye. It may be conjectured here that the apparent direction of the visual axis corresponds, in general, to the mean direction of the visual axes of the two eyes; in which case, however, with persons who are in the habit of using one eye by preference in looking through a microscope or telescope, the apparent direction more nearly approaches that of the true direction of the visual axis of the favoured eye than that of the other eye. More exact information on the apparently simultaneous directions of the two visual axes will be obtained later when we come to study the phenomena of double images.

Now I have found that, just as in the case of the apparent direction of the visual axis, there is a similar connection between the position of the retinal horizon and the torsional-rotations of the two eyes.

The simplest way by which I succeeded in making the necessary experiments was as follows. A black thread was stretched along the diameter of one end of a cylindrical tube, which was about a foot long. One eye being closed, the other end of the tube was placed in front of the open eye. A piece of white paper was held in front of the farther end of the tube, so that none of the objects in the room were visible at all. Then by turning the tube around its axis, I tried to adjust the black thread exactly horizontal or vertical, all the time keeping the lines of fixation parallel, which is a condition I learned to fulfil even with one eye shut. On removing the sheet of white paper from the front of the tube, I could compare the direction which I had given the thread with the directions of various objective horizontal and vertical lines in the room. In these experiments I always took a firm position in an easy chair, bending my head forward or backward, or holding it vertical, the tube meantime being always held horizontally, sometimes straight ahead, and sometimes to the right or to the left; so that the line of fixation was adjusted successively in every possible position with respect to the head.

In all these positions, so far as the eye could move without sensible constraint, it was found that, when the axes of the two eyes were parallel, I was able to set the apparently horizontal line so that it was really so, and that I set the line which was apparently vertical at an angle which did not differ from the true vertical by more than an angle of the same order of size as that between the apparently vertical meridian of the given eye and the really vertical meridian.

Thus the special result of these experiments is that the original horizontal meridian, or what we have termed the retinal horizon, can by no means always be considered as horizontal for every position of the eye, nor the one perpendicular to it as vertical.[1] On the contrary, in case of a glance directed sideways or toward the forehead or cheek, the angle between the retinal horizon and the horizontal plane may be as much as 10°, and yet even then a really horizontal line lying in the horizontal plane of sight will be considered as horizontal.

But it is different when the eyes are convergent. Suppose we throw back the head, and look through the horizontal tube when it is pointed straight forwards, and keeping the axes of the two eyes parallel, set the thread horizontal. Then when the direction is tested, it will be found to be really horizontal, as was stated above. Now suppose that a point is focused on the thread itself, or that the eyes are accommodated for the closest possible vision, without changing the directions in which they are looking. Immediately, there will be a very marked apparent rotation of the thread, taking place in the same sense as the rotation of the retinal horizon of the observer's other eye as it is being changed from the parallel position to the convergence position. Thus, for example, suppose the head is bent back, and the right eye is directed horizontally straight ahead; then as convergence sets in, the right end of the thread apparently will be lowered and the left end raised. When the head is bent forward, the result will be just the reverse. Moreover, the effect in the left eye is the reverse of what it is in the right eye. In order for the thread to appear horizontal when the eyes are convergent, the tube must be turned several degrees opposite to the sense of its apparent deflection, and then when the visual axes are again made parallel, the thread will no longer look horizontal. The necessary rotations of the tube in this case were much more considerable than the exceedingly small real rotations which were made by my observing eye as convergence of the other eye was being produced (see p. 51), and could not be explained by means of the latter.[2]

[1] The rule was stated in this form by Mr. E. Hering (*Beiträge zur Physiologie*, S. 254), but he did not experiment with parallel positions of the eyes, nor by looking in those directions where the deviation might have been manifested, for his point of fixation was always in the median plane.

[2] I could not succeed in measuring these angles, because strenuous accommodation, often repeated, soon gives me violent headache.

This is rather the same sort of phenomenon as we find in trying to judge the directions of visual objects. In spite of the fact that the eye which does the seeing is kept steady, the altered direction and the movement of the other (non-seeing) eye influence us to change our judgment as to the directions of horizontal and vertical lines.

All observers are not able to make their eyes parallel or convergent at pleasure, without having a corresponding point of fixation; and so I have modified the method for parallel visual axes in the following manner. A long black cord with a little weight attached was suspended vertically in front of an extended wall painted a uniform grey all over. Horizontal cords were fastened to opposite sides of the weight. Both of them were passed through rings, and a little weight attached to one of them, while the other proceeded to the observer, seated about six feet in front of the vertical one. By pulling or releasing the cord in his hand, the observer could deflect the vertical cord to one side or the other of the vertical line. This cord was observed through a cylindrical tube supported horizontally. No other vertical or horizontal lines were in the field of view; and the experiment consisted in trying to set that cord exactly vertical. The lower end of the vertical cord moved in front of a small scale on which its deviation could be read.

Experiments by this method were carried out by Dr. Dastich in the local physiological laboratory. Being near-sighted in his right eye, he used his other eye chiefly because that was emmetropic. When the cord seemed to him to be vertical, he found that invariably the lower end of it had been adjusted a little too much to the right, corresponding to the direction of the deviation of the apparently vertical meridian from the true vertical meridian. The values of the deviations from the vertical were as follows:

Left Eye

Head erect, looking straight ahead	1° 52′
" " looking to the right	2° 4′
" " looking to the left	1° 49′
Head bent forward, looking straight ahead	1° 37′
" " " looking to one side	2° 22′
Head bent backward, looking straight ahead	1° 37′
" " " looking to one side	2° 7′

Right Eye

Head erect, looking straight ahead	0° 42′

The oblique settings were all as far removed from the primary position as possible without producing any appreciable strain on the muscles of the eye. Between the downward directions of vision on the

right and left the difference should have amounted to about 16°, on the supposition that the same meridian of the eye always corresponded to the vertical direction. Instead of this, the difference was inappreciably small. It was the same way in case of the upward directions of vision on the right and left. The slight differences exhibited here mainly between the angles of the left eye may be due to little irregularities in the movement of the eyes, and possibly also to the fact that, while the directions of vision were almost parallel, they were not absolutely so. From correspondence with Mr. A VOLKMANN I have ascertained that the lines which he adjusted as vertical with the axes of his eyes parallel were not always absolutely vertical and did not coincide with the vertical meridian, but appeared to lie about halfway between the direction of an absolutely vertical plane and that of the vertical meridian of the eye. Mr. VOLKMANN is more near-sighted than Mr. DASTICH or myself, and this deviation might perhaps be due to the fact that the vision of near-sighted eyes with parallel axes is not accurate enough anyhow to obtain reliable results.

The difference produced by the convergence positions can be shown in these experiments by first setting the distant long cord vertically, and then, with the head in the same position, setting the thread which is stretched across the tube, all the time looking steadily at it; and then, finally, comparing the two settings.

Lastly, if the eyes are converged and focused on a point in the median plane of the head, then, as was found by HERING,[1] lines will be considered as horizontal if they correspond to the position of the retinal horizon of the given eye. For this purpose he used two tubes one inside the other, about five or six inches long and as wide as the face. A thread was stretched across the front end of one of them; and the eyes focused on the middle of it. Then by turning the tube, the apparently horizontal direction could be adjusted. The average was taken of from ten to twenty settings.

All these facts indicate that both eyes have an influence on the torsional-rotations similar to that which they have on the judgment of directions; and apparently the facts known at present (which ought certainly to be verified by still more accurate measurements) may be conveniently summarized by the following rule, which would be an extension of the principle proposed by HERING for the directions of vision.

[1] *Beiträge zur Physiologie*, pp. 254–256. Mr. HERING's criticism of my principle of easiest orientation is based on this experiment. He proposes, instead, the principle of avoidance of apparent motion. But both his criticism of the former and his argument in favour of the latter lose their force, because the result of this experiment does not agree with his data, *except when the point of fixation is in the median plane.*

Midway between the two eyes suppose there were an imaginary cyclopean eye which was directed to the common point of fixation of the two eyes, and that it rolled according to the law governing the rollings of the two real eyes. Imagine the retinal images transferred from one of the real eyes to this imaginary eye, so that the point of fixation of the imaginary eye is the same as that of the real eye, and the retinal horizon of the imaginary eye is the same as that of the real eye. *Then the points of the retinal image will be projected out along the line of direction of the imaginary cyclopean eye.*[1]

For example, when the right eye is kept fixed, while the other eye changes from parallelism to convergence, and therefore turns in to the right, usually executing a rolling movement in so doing, the cyclopean eye would also have to turn to the right through half the angle and roll about half as much. The result of this is that the visual images of the immobile right eye are apparently shifted and turned through the same angle as the cyclopean eye.

As long as the point of fixation lies in the median plane, the cyclopean eye does not undergo any rolling movement, and accordingly for all such positions the retinal horizons appear to be horizontal.

To give the explanation of this special behaviour, we must remember that our natural vision is binocular, and that all we learn directly from experience is to judge of the relative positions of the objects, which we focus with our eyes, with respect to the position of our own body, which we feel. We decide that a body is on our right when it lies on the right of the median plane of our body; which, however, in case it is nearer this plane than the right eye, can be seen by turning the right eye slightly to the left, when the left eye is turned far over to the right. We do not attempt to judge the direction of objects with respect to each eye separately, nor even with reference to the head, but rather with reference to the trunk of the body as being the seat of our organs of motion. The important thing in the end is the relation with reference to the latter.

Accordingly, the token of the senses by which an object is indicated as lying to the right is not that in fixating it one eye is turned to the right, or both eyes, but that their mean direction is turned to the right. Besides, it is only in some few cases that we are skilled in distinguishing the impressions of each eye separately, those being the cases where it is of practical importance, as in binocular vision of bodies. Thus while we have much skill in perceiving the mean direction and rotation common to both eyes, and of judging thereby about the position of

[1] The real difference between this rule and that given by HERING is that the cyclopean eye here is supposed to roll, whereas the retinal horizon of HERING's cyclopean eye lies always in the visual plane (*Visierebene*).

the focused object, we have little ability for judging of the direction of each eye separately, and generally cannot separate in our consciousness what belongs to one eye or the other.[1]

Accordingly, when we use the term direction of vision, we are not in the habit of distinguishing between the different directions of the two eyes and we are not trained to do it; and so we prefer this direction generally to the median plane of the head, relative to the body. In this sense HERING is right in referring the projections of the two eyes in the field of view to a common centre lying between them both in the median plane of the body and somewhere in the region of the bridge of the nose. This is a correct expression of the facts, although I should not like to make it the original basis of the explanation of visual phenomena, as that author has done. One reason for not doing this is because the direction of the attention has a decided influence on some of the phenomena which belong here.

Hold a sheet of paper before the lower part of the face so as to hide the hands and arms, and look at a distant object with one eye. Then insert the forefinger of the right hand up under this screen until it comes high enough to point at the observed object. The finger will come into sight behind the paper on the left of the focused object, supposing it is the right eye that is used, or on the right, in case of the left eye.

It is just the reverse in looking at a near object instead of a distant one, for instance, at a tiny dot on the edge of the paper screen, and then trying to bring the finger up farther away so that it seems to be just out beyond this dot.

This is in accordance with the rule given by HERING. In ordinary, unembarrassed vision the directions of sight are referred to the root of the nose, and the finger is inserted between the latter and the object of fixation, in which case, as a matter of fact, it does not happen to be in the real visual axis.

However, the experiment here described is also very apt to fail. For instance, if I concentrate my attention on the circumstance that I am only using my right eye, and let myself think deliberately about the place of that eye in my head, and then interpose my finger so as to hide the focused object, it comes up actually in the right direction.

We shall return to the discussion of these phenomena in the theory of double vision.[2]

[1] As to distinguishing between the impressions made by the right and left eyes, see also §31 and Note 4 on the subject at the end of that chapter.—K.

[2] ¶The following references may be inserted here to some of the more recent literature on the direction of vision:

J. S. SYSMANSKI, Versuche über den Richtungssinn beim Menschen. PFLÜGERS *Arch.*, 151 (1913), 158–170.—H. KÖLLNER, Die klinische Prüfung der Richtungslokalisation im

I ought to mention here an experience which I have often had, as follows. If I shut my eyes and hold up my forefinger, and try to focus it without opening the eyes, the moment I do open them I see double images of it; indicating that the lines of fixation are parallel or nearly so, and therefore pass by the finger about equally far from it on both sides. But in some strange fashion, with my eyes closed, I do contrive to get a clearer idea of the place where my finger is by touching the tip of it and rubbing it with the thumb of the same hand. Then indeed, even with eyelids closed, it is possible for me so to focus my eyes that the moment they are opened the finger is seen singly. It is the same way too when I touch and feel an external stationary object.

When we have compared the perceptions of touch and vision and thus at length found out the direction in which the observed object has to be searched for, the final result of it will be the localization also of the optical images that have originated elsewhere and of subjective stimulations of the retina and the nervous mechanism of vision.

Thus all stimulations of the fibres of the optic nerve are referred out in space, according to the law by which luminous phenomena are supposed to occur in those parts of the field (or of both fields), where material objects appear to be that are in such a position to send light to the corresponding places on the retina. The truth of this statement is manifested simply by evoking subjective phenomena, while real objects are visible in the field of view at the same time. For instance, having developed an after-image of the sun in the eye, look at a landscape; the after-image will coincide with certain external objects, which cannot be seen so well as they might otherwise be on account of the existence of the after-image. Certain parts of the retina will be fatigued; and hence the images of those external objects depicted there will be fainter than usual. The aggregate of these fainter objects in the field is the after-image. Obviously, therefore, the after-image in the field coincides with those objects whose images are formed on the fatigued part of the retina. In the same way shadows of entoptical objects, vascular figures, pressure images, and electrical images in the field of view may coincide with external objects. The invariable effect of such coincidence will be either to extinguish or enfeeble the sen-

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peripheren Sehen, ihre Ergebnisse bei Einaugigen, sowie uber die phylogenetische Bedeutung des Lokalisationsgesetzes. *Arch. f. Augenhk.*, 88 (1921), 117–138.—Idem, Die Sehrichtungen. Ibid., 89 (1921), 67–79.—Idem, Die haptische Lokalisation der Sehrichtungen sowie über die Sehrichtung von Doppelbildern. Ibid., 121–136.—F. B. Hofmann, Über die Grundlagen der egozentrischen (absoluten) optischen Lokalisation. *Skand. Arch. f. Physiol.* 43 (1923), 17–34. (See also *Zent. f. d. ges. Ophth. u. ihre Grenz.*, 10, p. 329.)—F. B. Hofmann and A. Fruböse, Line of direction in visual space. *Zft. f. Biol.*, 80 (1924) 91–130.—Idem, Eye movements and relative optical localization. *Zft. f. Biol.*, 80 (1924), 81–90. (J.P.C.S.)

sation of the external light coming from certain points of the field, or else to mingle it with other subjective light sensations. In noticing the corresponding variation of appearance of certain external points, naturally, we are bound to localize the changes taking place in the field of view as being in the same locality where the points whose appearance is altered were presumed to be at first; and the same rules will apply to the projection of the subjective phenomenon into the external world as were found to be valid as the result of experience in the case of points that are perceived by means of real external light.

It is true that individual subjective luminous phenomena may occur also in the absolutely dark field of view, where, of course, they will be localized according to the same rule. And, although they may not coincide with perceptible images of really visible external objects, still we know by experience for every place on the retina the direction in which visible objects would have to be for their images to be formed there; and the subjective phenomenon would coincide with these objects. That even in the dark field subjective phenomena, like after-images, for example, are localized by the same law as the impressions of really visible objects, is shown empirically by suddenly making the dark field bright, without moving the eye; then the after-image, without having changed its place, will be seen to coincide with definite objects in front of us and to overlap them. As there was no change in its place in the transition from darkness to light, it must previously have been localized in the same way as the external objects with which it eventually appeared to coincide.

These considerations scarcely admit of any doubt as to the truth of the rule by which every impression on the retina is projected precisely to that part of the field of view where an external object would apparently be which was properly located so as to make the same impression on the retina as the result of light proceeding to the eye in straight lines.

The law may be proved also by more direct methods, although not with very much precision. We know that the image of a luminous object over to the right will be formed on the left side of the retina, and *vice versa*; and, similarly, that the image of a luminous object situated above will be formed on the lower part of the retina, and also *vice versa*. In persons with thin, translucent eyelids the optical image of a very brilliant light may be seen shining through the sclera, at those very places in fact (Vol. I, p. 212). When the right side of the eye is pressed with the finger nail, the pressure image will be seen over to the left (Vol. II, p. 7). When light is concentrated by a lens externally on the right side of the sclera, a corresponding luminous phenomenon will be visible in the left-hand side of the field of view.

If a descending electric current is made to pass out of the eye at the place above mentioned, the corresponding bright spot will likewise appear in the field over on the left.

On the other hand, when the eye is stimulated on the left, the subjective phenomenon will be on the right in the field of view; when it is stimulated below, the phenomenon will appear to be above, and *vice versa*.

The optical illusions depending on this principle are very numerous. They may be classified in the following main divisions:

1. *The rays of light coming from the object have been deflected from their path by reflection, refraction or diffraction before entering the eye.* If the light continues homocentric after changing its path, then (aside from the illusions of judgment described previously) we imagine we see the object at that place in space, where the rays (produced backwards of course) intersect before entering the eye. This point of intersection is called the *optical image* of the object (Vol. I, p. 58). The optical effects produced by refracting and reflecting telescopes and microscopes, plane and spherical mirrors, magnifying glasses and other lenses and prisms also (supposing they are used so as to give a practically homocentric beam of light), are all of this nature. There is no need to explain the action of these instruments in detail, because the theory of them constitutes an extensive and systematic branch of physical optics. All of them cast optical images of objects, and we are supposed to see the former and not the latter; and so they produce optical illusions, but of such a character that we can easily avoid being deceived by them, and yet at the same time the magnified or otherwise modified optical images will enable us to perceive many details that were not perceptible in observing the object directly. Thus, a plane mirror enables us to see objects from a point of view which we cannot often have in reality, that is, from the point of view of an observer behind the plane of the mirror who sees our own face, for instance, from the front; which is something we cannot do directly. A prism enables us to separate the images of a luminous object and to display them in all the homogeneous colours of which the light from the object is composed; and so on.[1]

On the other hand, when the light in changing its path ceases to be homocentric, more or less blurred spots of light will be seen in those

[1] Here should be included also the case where light reaches the retina in a way that is different from the ordinary way; that is, by penetrating the sclerotic coat. Occasionally the opinion has been defended that in this case there occurs paradoxically an irregular localization. (Veraguth, *Zft. f. Psychol.* 1. Abt. XLII. p. 162); an assumption which, however, has been shown by control experiments to be due to an illusion. Grützner, Pflügers *Archiv.* 121. p. 798. 1908.—K.

parts of the field of view corresponding to the illuminated areas of the retina. Rainbow phenomena, diffraction fringes, the sparkling of water in motion, etc., are effects of this nature.

2. *The light enters the eye along straight lines, but the eye is not accommodated for the luminous point.* In a case of this kind, when the pupil is free, instead of luminous points being seen in the field of view, there will be apparently luminous areas more or less irregular in form and similar to the familiar star figures of blur circles (Vol. I, p. 189). Smaller objects, like the crescent of the moon, are very commonly seen as double or multiple images (Vol. I, p. 191). These phenomena are due to the fact that the light coming from a point of the object is no longer concentrated on a single point on the retina, but is spread over a small area of it. The result of the illuminated retinal area is that we have a luminous phenomenon extending over a certain area of the field of view.

When the pupil is not entirely free, as in looking through a stenopaic opening, the objects will also be seen in wrong directions and sizes. When the slit is moved, the object appears to move also, as explained in Vol. I, pp. 125-128. Here undoubtedly every luminous point of the object is reproduced by a practically punctual image on the retina, but, on account of the faulty accommodation of the eye, it does not occupy its normal position.

On observing objects through two or three narrow apertures, with imperfect accommodation, we get two or three images of them.

These experiments are important, because they show that exact accommodation is one of the requisites of the normal vision that is acquired by practice in localizing the impressions of the senses. The blur circles or parts thereof that remain on looking through stenopaic apertures are projected in the field of view as if they were images formed by exact accommodation. Here also for each illuminated point on the retina a luminous point is inferred in the field of view. These experiments also have had some importance in the development of physiological optics, by enabling us to see that it is not the direction in which a ray of light enters the eye, nor the direction in which it meets the retina, but simply the place where the light falls on the retina, that determines the state of projection. Referring to Fig. 56, Vol. I, p. 127, we see that the projection lines $f\varphi$ and $g\gamma$ are essentially different from the actual directions of the refracted and unrefracted rays.

3. *There are appearances of material objects which are in the eye itself,* so-called entoptical objects, *mouches volantes,* shadows of blood-vessels, the *fovea centralis,* etc., as described in §15 and partly in §25. Casting shadows on the posterior layer of the retina, these objects

appear, therefore, as shadows in the field of view itself. Hence, in this case, the optical illusion consists in projecting objects outside which are in the eye. They will generally be upside down, because ordinarily the shadow of the object on the retina is right-side up. Since the positions of these figures can be determined only by their subjective appearance, they do not teach us anything new so far as the theory is concerned.

4. *The nerves are stimulated, or the degree of stimulation is altered.* In these cases it is not the light itself that is changed, but the light sensation. In this category belong pressure images, the accommodation-phosphene, the luminous sheaves which are seen at the entrance of the optic nerve during the movement of the eye, the intrinsic light of the retina, and the electrical phenomena described in §17. In these phenomena the illusion no longer consists simply in a false localization of a luminous or dark object. The case is rather one in which there is nothing of that sort present at all but only the sensation which is wont to be produced regularly by such objects.

All the illusory phenomena above described may, perhaps, be in the field of view of a healthy person who is wide awake, and yet they will persist in spite of his knowing better and being aware that they are illusions. However, as a rule, when we do have this clearer insight, the illusion is seen to be such. When we look through an optical instrument or in a mirror, we are aware that the conditions of vision are altered, and we soon learn to arrive at a correct judgment of the real nature of the object in spite of the deceptiveness of the images. For example, by looking at ourselves in a mirror, we know how to shave and to comb the hair, etc., although the image in the mirror is perverted everywhere so far as right and left are concerned. After a little practice we learn to make preparations with needles under a lens or even under a microscope, although in either case every movement of our fingers is greatly magnified, besides being also inverted in the microscope. Thus, indeed, these illusive optical images may enable us to acquire a new training for our movements.

As to the rest of the phenomena which have their basis in the eye itself, it seems to be mainly the circumstance of the subjective phenomena sharing the movements of the eye that enables us to tell that they are subjective. With phenomena which flash out quickly and then die down again just as suddenly, this criterion fails, and then, indeed, there may often be a question whether it was something real that was seen. Thus, suppose a person is trying to find his way in the dark, and, as he makes some movement of the body and eye, he suddenly sees indistinctly a flash of light off to one side; under such circumstances occasionally the most practised observer will be at a

loss to say definitely whether the phenomenon was objective or subjective. It is quite likely that the origin of many ghost stories is to be found in subjective phenomena of this character. The intrinsic light of the retina abounds in figures that may easily be given all sorts of weird interpretations by a frightened person, especially when the eye is focused steadily on the dreaded apparition, and so cannot detect that it moves with the eye. In fevers and disorders of the brain, where the regular connection of ideas is deranged, so that they cannot be retained separately and compared and united, there is too an absence of the deliberation needed to perceive the subjective nature of these particular optical phenomena, and fantastic ideas are frequently intermingled with them. In *delirium tremens* there are black spots in the field of view, which flit about rapidly with the eye, exciting the idea of mice, black beetles or flies running hither and thither. On the other hand, in the description of the hallucinations of patients who are delirious from fever, we are apt to hear again of the luminous and coloured points and circles, which may be produced by a gentle pressure on the eyeball even when a person is well, and which are taken sometimes for sparks of fire, sometimes for flaming eyes, etc.

In the phenomena described thus far, the head has always been supposed to be held erect or, if not, its inclination was supposed to be known. In conclusion, let us mention another illusion depending on a false conception of the direction of the head. AUBERT[1] made a slit 5 cm long and 2 cm wide in the shutter of a room that was otherwise dark. It was the only bright and visible object anywhere in the room. When this bright line was vertical and his head was bent over to the right, so that his right ear was held downward, the line seemed to slant diagonally with its lower end to the right and its upper end to the left. When the head was tilted to the left, the line appeared to slant the other way. When the line made an angle with the horizontal of less than 45°, its lower end being on the left, it would appear to be vertical, or even turned past the vertical on the opposite side, when the head was tilted to the right; or it would appear horizontal, or even turned past the horizontal, when the head was tilted to the left. The greatest rotation of the bright line occurred when the inclination of the head was around 135°.[2]

The rotation of the bright line accompanies the tilting of the head, when the head turns gradually; but, if the head is suddenly tilted considerably, several seconds will elapse before the line completes its rotation.

[1] VIRCHOWS *Archiv für pathologische Anatomie und Phys.* Bd. XX.

[2] With respect to this phenomenon, see Note 4 at the end of the chapter.—K.

If, without changing the inclination of the head, the dark room is illuminated, the vertical line will appear vertical again. When the light is extinguished, it will be inclined, as before.

Now this is not a case where the eye really turns in the head, as can be proved with after-images. When an after-image is developed in the vertical meridian of the eye, and the head is tilted to the right through an angle of 90°, the after-image in the dark room will not appear to be horizontal, as it really is, but it will extend diagonally with its left end downwards; and a luminous line which happens to be really inclined in this way will appear to be vertical.

The illusion seems to be due rather to the fact that in the dark we are apt to consider the inclination of the head as less than it really is.

Instead of making the observations in a dark room, the line can be marked on a uniform wall, a cylindrical screen being adjusted in front of the face to hide all lateral objects.

This is the place to mention also those familiar appearances of objects being in motion, because we ourselves are gliding along in a boat or on a train which is proceeding slowly and gently; and also the contrary illusion of being in motion ourselves, when we are really sitting still, while the objects in front of us are in motion with uniform velocity. The former kind of illusion is illustrated on a big scale by the apparent immobility of the earth and the apparent motion of the stars. When two trains stop in a station alongside each other, a person in one train looking at the other is often in doubt. Then if one of them starts to move, it may be hard for the observer to tell which train is moving, unless it is possible to see stationary parts of the ground or buildings. So also in observatories with revolving dome-shaped roofs, such as are used for mounting the heliometer, when the dome turns, we get the illusion of the floor turning and the roof standing still.

In such cases generally we are apt to regard the larger portion of the field of view as being stationary and the smaller portion as moving. Besides, when a motion starts, we naturally expect our bodies to be jolted or shaken, or at least to feel the effects of inertia. If the motion begins very gently, as when a boat starts to move, we do not think we are in motion; or if we have been jolted by a passing train, whose vibrations have been transmitted to the stationary train, we may fancy that we are in motion. If either interpretation is equally possible, the observer can induce the one or the other apperception in his mind as he pleases.

For the study of giddiness induced by watching a motion, Mr. J. J. Oppel, who experienced this sensation in looking at the whirling water of the Rhine at Schaffhausen just above the falls, has designed an apparatus called an *antirrheoscope*, which enables the phenomenon to be observed at any time.

It consists of five parallel rollers placed side by side, each 2.5 inches in diameter and 2.5 feet long. They can all be set to rotating in the same direction by means of a larger disc (*Rolle*). Each of them is covered with a sheet of white paper, with two black spirals, of 2.5 turns, drawn on it. Each of the spirals is composed of a broad central black stripe, 1.5 inches wide, in between two narrower stripes, a half inch wide, and a half inch away from it. The white band between the black stripes of one of the spirals and the next one to it will, therefore, be 1.5 inches wide also, white and black being thus symmetrically distributed. Now on turning the larger disc, whose edge rubs against the ends of the rollers, all five of the latter will turn the same way, the middle one revolving a little faster than the outer ones, in imitation of the unequal motion of the water in the river. The spiral bands will appear to be shifted along the rollers lengthwise, with uniform speed; and if the observer, after gazing for some time on the apparent motions of the spiral bands, turns to look at stationary objects, the latter will seem to go backwards.

Mr. OPPEL also fastened a mark on the rollers, so as to keep the gaze steady. However, it seems, in trying to look steadily at this mark, he often failed to do so. And so, believing that steady fixation was required to produce giddiness, and that steady fixation was prevented simply by looking at the moving mass, he used for the mark of fixation a little wooden diamond, a half-inch wide and three quarters of an inch high, which was itself turned slowly by the mechanism of the apparatus, presenting first one side to the spectator, and then the other. His efforts then were successful, owing to the fact, in my opinion, that this contrivance made it impossible to gaze steadily and continuously at one and the same fixed point; for every point in the wooden diamond, at which one might have tried to look, alternately disappeared and reappeared. The results of my own experiments lead me to the opposite conclusion from that of OPPEL in this matter; that is, when the gaze is absolutely steady, giddiness does not occur, but is produced entirely by the involuntary and usually unconscious little movements that are made in following the moving objects. However, OPPEL is right in maintaining that larger voluntary movements of the eye, by which the moving body is consciously pursued a long way, do hinder the illusion.

The reason why objects are seen erect, although their retinal images are inverted, was explained by KEPLER[1] as being produced by rays coming from an upper part of the object. SCHEINER[2] was of the same opinion. PRIESTLEY[3] deduced this peculiar characteristic of visual ideas from analogy with the sense of touch. DESCARTES[4] expounded the natural method of judging of the sizes, positions and distances of objects by the direction of the axes of the eyes; comparing it with the way in which a blind man judges of the size and distance of a thing by means of two rods of unknown length, when the hands in which they are held are opposite each other at a known distance and in a known position. Incidentally, the question about upright vision of objects led to the publication of numerous articles on the subject.[5]

KEPLER[6] discovered too the correct rule for the apparent positions of objects as seen in lenses, mirrors, etc., by assigning them to the places where

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[1] *Paralipomena*, p. 169.—SMITH, *Opticks*, p. 4 of Remarks.

[2] *Oculus*, p. 192.

[3] *Geschichte der Optik*, German translation (KLÜGEL's). Leipzig. 1776. p. 69.

[4] *Dioptrice* p. 68, and *De homine*, p. 66.

[5] KAESTNER, *Hamburger Magazin*, VIII, St. 4, Art. 8; IX, St. 1, Art. 4.—LICHTENBERG in ERXLEBENS *Naturlehre*. 6. Aufl. p. 328.—RUDOLPHI, *Physiologie*, II, 227.—L. FICK in MÜLLERS *Archiv fur Anatomie*. 1854. p. 220.—See other references in the bibliography below.

[6] *Paralipomena*, p. 285 and pp. 69–70.

the rays were focused before entering the eye. The difficulties that afterwards led to numerous discussions on this point did not so much involve the question as to the direction in which the object was seen as its distance; which is to be considered in the next chapter.

PORTERFIELD's notion was[1] that, by virtue of an original natural disposition, objects are seen somewhere along the straight lines that are drawn normally to the retina at the place where the image is formed. The same theory was proposed also by D'ALEMBERT,[2] BARTELS,[3] and many others. VOLKMANN[4] took the normals to the retina as the lines of direction, which, by the definition given in Vol. I, p. 96, are the lines drawn from the retinal image to the (posterior) nodal point of the eye. These lines are, indeed, the right ones for finding the luminous point objectively in physical investigations, when the place of the retinal image and the adjustment of the eye are completely assigned, the latter being supposed to be properly focused. Thus the lines of direction have an important rôle in physiological optics, especially when it is a question of ascertaining what external objects are responsible for images that coincide with certain other stimulations of the retina, whether the latter are produced by light or by internal stimulus. Thus, as to telling correctly and objectively the place where the thing is that is seen, VOLKMANN's mode of representation is right. But a correct judgment of this sort applies almost entirely to points seen directly with both eyes, and not always for them. The directions of all points seen indirectly are misjudged, by making the angles too small between the lines of direction drawn to them and the line of fixation, as was shown in the preceding chapter. Whenever the eyes are made to converge and are directed to near objects, the directions of the things in view are incorrectly estimated, as we saw from the above experiments. One of the main difficulties about VOLKMANN's theory is in explaining binocular double images, as HERING has rightly observed.[5] VOLKMANN's theory, therefore, cannot be considered as being an intuitive and elementary law whereby the direction of what is seen is determined *per se*. HERING did a real service in bringing to light the influence of convergence positions in connection with this matter.

The effect of giddy movements and apparent movements was investigated by PLATEAU,[6] OPPEL,[7] and ZÖLLNER[8]; and the effect of mistaken judgments of the position of the head was investigated by AUBERT.[9] As to the effect of paralysis of individual muscles, see A. v. GRAEFE[10] and NAGEL.[11]

1604. KEPLER *ad* VITELLIONEM, *Paralipomena*. pp. 169; 285; 69–70.
1619. SCHEINER, *Oculus*. Oenipontii. 1619. p. 192.
1637. DESCARTES, *Dioptrice*. Leyden. p. 68.

[1] *On the eye*, II, 285.

[2] *Opuscula mathem.* I, p. 26.

[3] *Beiträge zur Physiologie des Gesichtssinnes.* Berlin 1834.

[4] *Beiträge zur Physiologie des Gesichtssinnes.* Leipzig 1836, and Article on Vision in R. WAGNERS *Handworterbuch der Physiologie.* See also MILE concerning Richtungslinien des Sehens. POGGENDORFFS *Annalen*. XLII, 245; and MÜLLERS *Archiv fur Anatomie*. 1838. p. 387

[5] *Beiträge zur Physiologie.* Leipzig 1861. pp. 35–64.

[6] *Bulletin de Bruxelles* T. XVI.—POGG. *Annalen* LXXX. p. 287.

[7] POGG. *Annalen*. XCIX. 543.

[8] *Ibid.*, CX. 500.

[9] VIRCHOWS *Archiv für pathologische Anatomie*. XX. 381–393.

[10] *Archiv. fur Ophthalmologie*. I, 1. p. 67.

[11] *Das Sehen mit zwei Augen.* Breslau 1861. pp. 124–129.

1667. HONORATUS FABRI, *Synopsis optica.* Lugd.
1709. BERKELEY, *Essay towards a new theory of vision.*
1740. LE CAT, *Traité des sens.* Rouen.
— WEDEL, Über den Radius visorius des HONORATUS FABER in HALLERI *Disputat. anat.* IV, 216.
1754. CONDILLAC, *Traité des sensations.*
1759. PORTERFIELD, *A treatise on the eye.* Edinburgh. Vol. II. p. 285.
1761. D'ALEMBERT, *Opuscula mathem.* I. p. 26; 265.
1771. BOEHM, De visione erecta. *Acta Hassiaca.* 64.
1772. PRIESTLEY, *History and present state of discoveries relating to vision, light and colours.* (KLÜGEL's German Translation). Leipzig 1775. p. 69.
1783. ROCHON in *Recueil de Mémoires sur la Mécanique et Physique.* VI. p. 241.
1784. du TOUR, Mémoire pour établir que le point visible est vu dans le rayon qui va de ce point à l'oeil. *Mémoires de savans étrang.* Paris. VI. p. 241.
— FEARN, *A rationale of the laws of cerebral vision, composing the laws of single and erect vision, deduced upon the Principle of Dioptrics.* London.
1788. WALTER, *Berliner deutsche Abhdl.* 3.
1793. ARALDI, Esame di uno fra i diversi dubbi messi dal celebre D'ALEMBERT ai principi dell' Ottica; con alcune considerazioni sopra la teoria psicologica della visione. *Memor. dell' Istit. nazion. Ital.* I. p. 451.
1794. LICHTENBERG in ERXLEBENS *Naturlehre.* 6. Aufl. p. 328.
— KAESTNER in *Hamburger Magazin.* VIII, St. 4, Art. 8; IX, St. 1, Art. 4.
1820. RUDOLPHI, *Physiologie.* II. 227.
1826. J. MÜLLER, *Zür vergleichenden Physiologie des Gesichtsinns.* Leipzig.
1834. BARTELS, *Beiträge zur Physiologie des Gesichtsinns.* Berlin.
1836. VOLKMANN, *Beitrage zur Physiologie des Gesichtsinns.* Leipzig.—Also, in R. WAGNERS *Handwörterbuch der Physiologie.* Article: Sehen.
1837. MILE, Über Richtungslinien des Sehens. POGG. *Ann.* XLII. 245; and in J. MÜLLERS *Anat. u. Physiol.* 1838. p. 387.
1844. D. BREWSTER, Law of visible position in single and binocular vision. *Edinb. Trans.* XV. 1844.
1849. PLATEAU, Sur de nouvelles applications curieuses de la persistance des impressions de la rétine. *Bull. de Bruxelles* XVI. II. 30, 254.—*Institut* XVIII. No. 835. p. 5.—*Phil. Mag.* XXXVI. 434, 436.—POGG. Ann. LXXX. 150, 287.
1852. H. BOENS, Étude sur la vision de l'homme et des animaux. *Bull. de Bruxelles* XIX. 2. pp. 155–161. (*Cl. des sciences.* 1852. pp. 443–449.)
— LOTZE, *Medizinische Psychologie.* pp. 362–369.
1854. L. FICK, Bemerkungen zur Physiologie des Sehens. MÜLLER, *Archiv für Anat. und Physiol.* 1854. pp. 220–225.
— A. v. GRAEFE, Beiträge zur Physiologie und Pathologie der schiefen Augenmuskeln. *Archiv. für Ophthalmol.* I, 1. p. 67.
1855. H. HELMHOLTZ, *Über das Sehen des Menschen. Ein populär wissenschaftlicher Vortrag.* Leipzig. pp. 20–42.
— E. B. HUNT, On our sense of the vertical and horizontal. SILLIMAN's *J.* (2) XX, 368–375.
1856. J. J. OPPEL, Neue Beobachtungen und Versuche über eine eigentümliche, noch wenig bekannte Reaktionstätigkeit des menschlichen Auges. POGG. *Ann.* XCIX 540–615.
1858. UEBERWEG, Zur Theorie der Richtung des Sehens. *Zeitschr. für rat. Medizin.* (3) Bd. V. 268–282.
1860. J. J. OPPEL, Zur Theorie einer eigentümlichen Reaktionstätigkeit des menschlichen Auges in bezug auf bewegte Netzhautbilder. *Jahresber. d. Frankfurter Vereins,* 1859–1860, 54–64; *Zeitschr. für Naturw.* XVII. 258–260.
— H. AUBERT, Eine scheinbare bedeutende Drehung von Objekten bei Neigung des Kopfes nach rechts und links. VIRCHOWS *Archiv* XX. 381–393.

1861. NAGEL, *Das Sehen mit zwei Augen.* Breslau. pp. 124–129.
— E. HERING, *Beiträge zur Physiologie.* Leipzig. Heft 1. pp. 35–64.
1862. F. ZÖLLNER, Über eine neue Art anorthoskopischer Zerrbilder. POGG. *Ann.* CXVII. 477–484; *Zeitschr. für Naturw.* XXI. 163.
1863. J. CZERMAK, Über das sogenannte Problem des Aufrechtsehens. *Wiener Ber.* XVII. 566–574.
1865. ALFRED GRAEFE, Über einige Verhältnisse des Binokularsehens bei Schielenden. *Archiv für Ophthalmologie.* XI, 2. pp. 6–16.

Notes on §29 by v. Kries

1. The phenomena which HELMHOLTZ describes here (p. 248) are known at present as motion after-images, and perhaps it is more usual nowadays to take a different view of them. They cannot be explained without discussing at the same time the perfectly general ideas in regard to the *vision of motions*, which have been developed in the last decades. This is a subject which has been found to have a significance on its own account; and hence it will be discussed here thoroughly and systematically.[1]

It should be stated in the first place that the impression of a motion as was first shown by EXNER in a very important research, can be produced in one of two ways, either in a *direct* way, as we may call it, or in an *indirect* way.[2]

The indirect mode, which was the only one that had been considered prior to this work, simply implies that, at the expiration of some interval of time, the given object will be seen at a place which is different from where it was initially. Psychologically speaking, therefore, an indirect perception of motion of this kind involves both the idea of an interval of time and that of two places. At first thought it would seem that this would always have to be the case whenever a motion was noticed; but it appears that this is by no means the fact. While it is true that the fulfilment of these conditions can be proved absolutely, particularly in the case of motions that are slow and that do not last too short a time, we find that in case of more rapid motions

[1] At the same time, we are not unmindful of the fact that, according to our present way of regarding these relations, strictly speaking, they should not be included in the contents of this chapter at all. Hence, in recent treatises on the visual perceptions, an independent chapter is devoted to the vision of motions. However, with the idea of preserving the scheme of HELMHOLTZ's method as far as possible, it is appropriate to consider this subject here.

[2] S. EXNER. Das Sehen von Bewegungen und die Theorie des zusammengesetzten Auges. *Wiener Sitzungsber.* LXXII, 3. Abt. 1875. For the reasons given in the Appendix, I cannot consent to use EXNER's expression and to speak here of a "sensation" of motion.

the impression is a more immediate one, and so also a more powerful one in another characteristic way. Even in such motions, to be sure, the conditions in most cases are such that the difference is noticed and comprehended between the places where the object was seen at the beginning and end of some interval of time. Only this does not necessarily have to be the case: for it may also happen that we get this powerful impression of the motion of an object, although at the end of the time the place where it appears is not different from where it appeared at first.[1] The distinction between the direct perception of the motion and the indirect comes out very clearly here, and shows particularly the independence of the modes by which they are produced. But even when this is not the case, we can tell by carefully observing ourselves that the impression of the motion in these cases stands in an entirely different relation with respect to the impression of difference of place from what it does in those cases where we speak of an indirect perception of motion. It may be accompanied by it, but is not a result of it. If the way we perceive the motion of an insect or of a bird flying past us a little way off is compared with that of seeing the motion of a crawling beetle or, better still, that of a moving train some kilometres away, there will be no difficulty about recognizing the truth of EXNER's distinction.

While there is necessarily a closer connection between the indirect perception of motion and the perception of bodies at rest, an independent physiological significance attaches to the direct perceptions of motion from the way in which they originate. Considering their physiological basis, we see at once that this may be twofold. On the one hand, the images of a moving body may glide over the retina when the eye is fixed; and on the other hand, the eye may also follow the motion of an object. Thus the gliding of the images on the retina and the motion of the eye are two sets of factors that have always to be considered as basis of an immediate impression of motion. It is important to note that, while we may be able to isolate one of these factors in the experiment, the other cannot be isolated. Indeed, by riveting the gaze the eye can be kept steady, at any rate approximately, while other objects in the field of view are allowed to glide over the retina. But in trying to follow the motion of an object with the eye, it is impossible to prevent a shifting of the images on the retina. The truth probably is that, under all circumstances, the only way it is possible for the eye to follow the motion is by the object's being shifted a little so as to be perceived excentrically and thus to enable

[1] See the remarks in the Appendix in regard to this point also.

the eye to catch up with it. An entire series of remarkable facts has been discovered bearing on the more specific conditions of the perceptions of motion. In discussing them we must try to keep separate as far as we can the above mentioned modalities of the perception of motion. We shall see, indeed, that it is not always possible to do this with complete success, partly on account of the difficulties in the nature of the case, and partly too because the observations have not always been made in a way suitable for this purpose.

One of the first questions to be raised here is with reference to the amounts of the motions that are either just perceptible or just not perceptible, that is, as to the so-called *threshold values*. This question splits up into several others, depending on the various ways in which a motion can be varied, and also on the various ways by which it can be brought to the limit of perceptibility. Perhaps, the first case that comes into the mind, but not the simplest by any means, is that in which the *speed* is diminished. It may be supposed that by reducing the speed a limit can always be found at which the *direct* impression of motion ceases. This limit seems to have been determined in AUBERT's experiments.[1] According to him, the angular velocity of an object must be between one and two minutes of arc per second, "in order for it to appear immediately to be moving," whereas with lower speeds it takes several seconds to detect the motion. However, as might be expected, the result of these experiments depends largely on whether there are other stationary objects in the field of view, and on how close they are to the observed object. Both in the experiments above mentioned and in PORTERFIELD's earlier experiments (which, by the way, gave similar results), the field of view was perfectly free, and so a quantity of stationary objects could be seen. We shall return in a moment to the differences that this involves. Moreover, it is to be noted that in these experiments the object that had to be judged was *fixated*; and according to what was said above we are not able to decide how much a shift of the images on the retina has to do with the impression of motion, and how much the motion of the eye in following the motion of the object is involved. In this respect the conditions for objects that are seen excentrically are easier to control than for objects that are focused in the fovea; because in the former case the fixation of a stationary mark (that is known to be stationary) operates as a condition that is self-evident. AUBERT[2] found the values that are summarized in the subjoined table.

[1] PFLÜGERS *Archiv*, XXXIX. p. 347; *ibid.*, XL. p. 459. 1887.
[2] PFLÜGERS *Archiv* XL. p. 477. 1887.

Angular distance from the fovea	Least perceptible angular velocity	Angular distance from the fovea	Least perceptible angular velocity
15′	54″	5° 10′	5′ 30″
1° 15′	1′45″	7°	6′ 44″ to 8′ 45″
2° 15′	2′ 8″	8°	9′
3° 15′	3′ to 4′	9°	15′

The importance of having a stationary object to compare the perception with, was pointed out by AUBERT in connection with the above results. He got about ten times higher threshold values when the object to be tested was observed through a slit in a small box, so that objects elsewhere were hidden, although the edges of the slit, it is true, were visible still. The simplest case fundamentally would be to observe an object that was visible all by itself in an otherwise dark room. I am not aware of any experiments that enable us to determine the threshold of a *direct* perception of motion in this case. In what is known as the *indirect* method, obviously, a motion would always be perceptible if we allowed it to continue for an unlimited time in the same direction. There is no sense in speaking of definite limiting values unless the duration of the motion is limited, or unless the motion is made to take place, say, in a closed path, and not in the same direction. Incidentally, the observations made on the perception of the motion of an absolutely isolated point have encountered difficulties of another sort, thereby preventing some of the investigators at any rate from obtaining definite values. AUBERT especially stressed above everything the frequent occurrence of illusions, such that "sometimes a person is absolutely sure of seeing motions when no objective motions are present, whereas, on the other hand, he will not be aware of very active objective motions and will not notice them anyhow." With reference to the former case of the appearance of motion of a point that was really stationary, AUBERT employed the term *autokinetic sensation*. All the phenomena of this nature were subsequently described and investigated in great detail (CHARPENTIER,[1] EXNER[2]). In spite of the difficulties caused by those apparent motions, which were encountered also by BOURDON,[3] the latter author believes he can succeed in getting approximately definite threshold values. He

[1] *Comptes rendus.* CII. pp. 1155 and 1462. 1886.

[2] *Zeitschrift f. Psychologie.* XII. p. 313. 1896.

[3] *La perception visuelle de l'espace.* p. 178. 1902.

states that with velocities of 14 and 21 minutes of arc per second in the majority of cases it was still possible to detect the motion correctly.[1]

Threshold values in a different sense will be obtained by varying the *excursion* of a motion, not its speed; in which case it is advisable that the speed should be chosen so as to have as favourable a value as possible. Incidentally, to be perfectly precise, it ought to be stated whether the speed is kept constant or the total duration of the motion. Experiments of this sort have recently been reported by BASLER.[2] In a theoretical way he obtained the very important result,[3] that in the fovea where vision is most distinct movements whose range was not more than 20 seconds of arc could be perceived with certainty. This value is still considerably below that of the smallest perceptible interval between two points.[4] The motion lasted about the fifteenth of a second. The corresponding fact for the excentric parts of the retina had been found previously by AUBERT and EXNER (see references cited above). The latter, in particular, emphasized the ability of the peripheral parts of the retina for perceiving displacements that are less than the least perceptible interval between stationary objects, and gave an interesting explanation of how the duty of the peripheral parts of the retina for general physiological purposes consists in just this noticing of *variations*. BASLER's results are perhaps not out of harmony with this view. No doubt, there is the same difference for the centre of the retina also. Here too a shift can be detected that is less in amount than the interval required to distinguish objects that are seen simultaneously, and yet this difference is not so large for the centre as it is for the periphery. The resolving power (*Distinktionvermögen*, as this relation may be termed) decreases more rapidly as we proceed from the centre than the sensitivity to motion; as was shown by RUPPERT[5] by a research with this end expressly in view.[6]

Finally, in case of motions of fixed range, the question may also be with reference to an *upper limiting value* of the speed or duration, since

[1] ¶See H. F. ADAMS, Autokinetic Sensations. *Psychol. Monog.*, 14 (1912), No. 59.—P. SCHILDER, Über autokinetischer Empfindungen. *Arch. f. d. ges. Psychol.*, 25 (1912), 36–77. (J.P.C.S.)

[2] PFLÜGERS *Archiv.* CXV. 1906. S. 582.—*Ibid.*, CXXIV. 1908. p. 313.

[3] In this method likewise the importance of having a stationary object for comparison is very evident. Thus when no objects of this sort whatever were present (the observation being conducted in an absolutely dark room), BASLER found that his threshold values were about four times as high.

[4] By the way, the phenomena observed by FREY and METZNER in the domain of the sense of touch are analogous. *Zft. für Psychologie.* XXVI. p. 33. 1901 and XXIX. p. 164. 1902.

[5] *Zeitschrift für Psychologie.* XLII. 1908. p. 409.

[6] ¶See also A. BASLER, Über die Helligkeitsschwelle bewegten Felder. PFLÜGERS *Arch.*, 167 (1917), 198–227.—Idem, Influence of brightness on recognition of small movements. *Arch. f. d. ges. Physiol.*, 109 (1923), p. 457. (J.P.C.S.)

it might be expected that when the duration is too brief the motion will no longer be perceived as such, but rather that the object will be seen at all the various parts of its path at the same time.[1] We are indebted to BOURDON for researches of this character. For direct vision the limiting times as found by him were between 0.027 and 0.079 second, corresponding to angular velocities of between 3.5 and 1.4 degrees per hundredth of a second;[2] whereas for indirect vision the values obtained were between 0.023 and 0.061 second, corresponding to angular velocities between 4 and 1.8 degrees per hundredth of a second. The whole range of the motion was about ten degrees. Incidentally, observations of similar kind had been previously made by EXNER.[3] The most remarkable thing that he found was that a motion could still be detected when its entire duration was not more than 0.014 second; whereas when he illuminated two points at the ends of the path at just this interval of time between them, it was not possible to tell that they did not appear simultaneously. (An interval of 0.045 sec was needed to tell that they were not simultaneous.).

As to the *difference-sensitivity* for impressions of motion at medium speed, data have been published by AUBERT[4] and BOURDON.[5] The former found that a difference of about 1′ per second could be perceived (about the value, therefore, of the zero-threshold); a result which is certainly a little curious because it makes us miss the connection that is familiar to us everywhere else between the just perceptible difference and the absolute value of the magnitudes to be compared. BOURDON found that two velocities could be distinguished when the difference between them amounted to between one-twelfth and one-eighth of their value.

Lastly, something should be added here concerning the *absolute magnitude* of the impressions of motion. In EXNER's article referred to several times already, that writer especially, speaking of this point, emphasizes the fact that motions that are perceived excentrically are considerably over-estimated. Hang a small balance pan by a cord about 2 metres long so as to swing slowly like a pendulum; and place

[1] The basis of this is the fact (which belongs here in a certain sense) that various visual impressions rapidly succeeding one another may blend in the perception of a unitary object in motion (as in case of stroboscopes, kinematographs, etc.). But as this subject has been discussed by HELMHOLTZ in another connection, we merely refer to the notes that have been added there in the third edition concerning the more recent experiments on these relations.

[2] *Loc. cit.*, p. 188. The smaller values were obtained by using a larger object and the higher values by using a smaller object.

[3] EXNER, PFLÜGERS *Archiv.* XI. 1875. p. 409.

[4] *Loc cit.*

[5] *Loc. cit.* p. 192.

a lighted candle in it. Now observe these vibrations very indirectly at first, and then turn the eyes to look straight at them; it will be astonishing to see how much smaller the motion is when it is perceived more exactly by foveal vision than it appeared to be in indirect vision. —But the kind of perception of motion also influences its apparent magnitude. According to FLEISCHL's observations[1] a motion that is observed by the fovea where vision is most distinct is apparently about twice as large when the eye is held steady, so that the image glides over the retina, as if the eye were following the object.

Here, by the way, should be included also the so-called *anorthoscopic phenomena* as being characteristic illusions connected with ocular judgment of motions. The more modern way of regarding the perception of motion is doubtless responsible also for somewhat changed points of view with respect to these phenomena.

The contrast phenomena to be observed in the perceptions of motions are of great interest above everything else. They are principally phenomena of the *successive contrast* type which, as above stated, are generally spoken of at present as *motion after-images*.

Here we touch on the point, where we have to differ from the view that HELMHOLTZ maintained, since these phenomena cannot be referred to ocular motions, at least not to them entirely, as he tried to do. The fundamental experiment of this kind consists in looking in the same direction for some time at moving objects, such as waves and small objects floating on the surface of a stream, and then turning the eye to a stationary point; under these conditions, the latter will appear to glide in the opposite direction. When this experiment is made without special precautions, no doubt, the eye will consistently follow the moving objects a little distance in the regular way, and then be jerked back again; and hence it seems possible to connect the motion after-image which arises in this case with a continuance of this kind of ocular movements. But there can be no doubt, that the other factor also, which, as we saw, can be at the bottom of the immediate impressions of motion, that is, the *gliding of the images on the retina*, may be responsible for a motion after-image. The simplest way of testing this is to make an experiment of a similar kind, only modifying it so that the eye does not follow the moving objects, but gazes steadily at a fixed mark. The concordant results of numerous observers prove beyond doubt that the phenomenon of the motion after-image can likewise be perceived perfectly distinctly under these conditions.

Although HELMHOLTZ's results are here in positive conflict with what has since been quite established, there is some satisfaction in being able to

[1] *Sitzungsberichte der Wiener Akademie.* Math.-naturwiss. Kl. (3) LXXXVI. 1882.

assign the reason for this conflict with some degree of probability. Presumably, it was due to the fact that HELMHOLTZ confined his experiments to cases in which the velocity of the moving bodies was too small. As a matter of fact, it is precisely in case of comparatively slow motions that the phenomena can be seen best; whereas they are not noticeable when the motion is so rapid that the objects become confused and vague. For instance, the motions of outside objects as seen from a moving train, when we are gazing steadily at a point on the window glass, are of this latter sort. What HELMHOLTZ says on page 248 shows that his observations were all made under just such similar conditions.

In these observations, no doubt, it might be conjectured that unintentional and unnoticed movements of the eye were also involved; and in the most extreme case, when after looking steadily at the centre of a revolving disc a rotation in the opposite direction is observed as after-image, rolling movements of the eye might be suspected. But an explanation on this basis is ruled out entirely, if, after considering motions extending radially all over the field either toward the point of fixation or away from it, we find we have a corresponding motion after-image that also extends radially in all directions.

Consequently, the facts undoubtedly show, as was brought out especially by EXNER,[1] that, exactly as is the case with the impressions of motion that are due to the eye's following the moving object, *the gliding of the image on the retina leaves behind an after-image such that the image, really lingering at the same places on the retina, gives us the impression of a motion in the opposite direction.* And on the basis of the phenomena last mentioned, this rule may be supplemented by saying that, *in this respect the various parts of the retina are to a certain extent independent of each other, so that the motion after-image is determined for each individual place by the image-displacements that have previously taken place there.*

This statement of the fundamental fact may be supplemented by certain more specific details that have been learned in regard to motion after-images. BORSCHKE and HESCHELES[2] have shown that when images are made to glide over the same place on the retina in different directions at the same time, the motion after-image is found to be the resultant of the component motions, as obtained by a process similar to that of the parallelogram of forces. Measurements were made by CORDS and v. BRÜCKE.[3] Their method was first to produce a motion after-image, and then to expose the eye, not to stationary objects, but to objects that were moving again in the same direction, but

[1] EXNER, Einige Beobachtungen über Bewegungsnachbilder. *Zentralblatt für Physiologie.* I. 1887. S. 135.—*Zeitschrift für Psychologie.* XII. 1892. p. 388.

[2] *Zeitschrift für Psychologie.* XXVII. 1902. p. 387.

[3] PFLÜGERS *Archiv.* CXIX. 1907. p. 54.

slower than before, and with a speed that could be varied properly. By a series of experiments with the same pattern of motion the speed of the motion after-image was found that would be just compensated by the objective motion of the observed object, the latter appearing thus to be stationary. The after-image speeds were found by these observers to vary between 3 and 60 minutes of arc per second. They increased with the speed of the object up to a certain maximum value, beyond which they began to decrease.

As to the action of simultaneous contrast on these phenomena, it might be supposed that the familiar sight of clouds sailing past the moon and causing the latter to seem to be moving, was an instance of this effect. But in my opinion this explanation is not satisfactory. Gazing at the moon, we get the impression of its moving, it seems to me, not because there is a contrast effect produced by clouds moving in the field of view, but rather because, as a result of the given conditions, there is a modification of that factor that is involved in determining the perception of the *entire field of view*. The psychological way of expressing it would be to say, that the illusion is induced of supposing that the eye is in motion, although it is really at rest; or *vice versa*, if we are gazing at a cloud instead of at the moon. Accordingly, instead of the apparent motion of the clouds being enhanced, as ought to be the case in a contrast relation, the latter are apparently standing still. The effect that is observed here is, therefore, in my opinion, no contrast phenomenon, but another sort of influence of *a factor contributing to a harmonious determination of the total perceptions.*

However, certain phenomena described by v. SZILY[1] may be regarded as instances of real simultaneous contrast in the realm of the impressions of motion. The connection between the two eyes with reference to motion after-images will be discussed hereafter.[2]

2. Experiments that have been made in the last twenty years or so on the *static organ* have taught us to regard the phenomena of

[1] v. SZILY, Bewegungsnachbild und Bewegungskontrast. *Ztschr. für Psychologie.* XXXVIII. p. 81.

[2] ¶Some recent literature on the subject of perception of motion is as follows:

M. WERTHEIMER, Experimentelle Studien über das Sehen von Bewegungen. *Zft. f. Psychol.*, 61 (1912), 162–266.—H. J. WATT, The psychology of visual motion. *Brit. J. of Psychol.*, 6 (1913), 26–43.—P. STUMPF, Über einige Methoden zur Untersuchung der Augen mit Bewegungsreizen. *Arch. f. Augenhk.*, 77 (1914), 381–394.—T. KEHR, Allgemeines zur Theorie der Perzeption der Bewegung. *Arch. f. d. ges. Psychol.*, 34 (1915), 106–120.—A. KORTE, Kinematoskopische Untersuchungen. *Zft. f. Psychol.*, 72 (1915), 193–296.—W. FILHENE, Über das optische Wahrnehmung von Bewegungen. *Zft. f. Sinnesphysiol.*, 53 (1921), 134–144.—Idem, Über foveale Wahrnehmung scheinbarer Ruhe an bewegten Korpern und deren Lokalisation, sowie über die Aberration der Sterne. *Zft. f. Sinnesphysiol.* 53 (1921), 234–254 and 54 (1922), 159–160. (J.P.C.S.)

giddiness in a different light from that given in the description in the text (p. 249). According to the new way of thinking, the static organ is supposed to adjust our impressions independently of states of motion of our body. Especially under the conditions that are being considered at present, the sensation of giddiness depending on the function of the static organ is aroused in the most pronounced manner; and undoubtedly it is this that causes the apparent rotation of the observed objects. It would not be quite safe, it seems to me, to try to explain more specifically how this occurs. In the first place I suppose we must assume that when we have the impression of turning clockwise, say, the immediate consequence is that the objects we see seem to be turning with us.

However, perhaps there is more to it than this. The results of experiments on giddiness do not indicate at all that the ocular movements assumed by HELMHOLTZ do not exist. On the contrary, their existence has been proved with complete regularity. The only thing is that now we have come to regard their origin in a different light. They can hardly be attributed to our becoming accustomed to a certain mode of action, as HELMHOLTZ thought, but, according to our views at present, they are reflex actions that are released by the sensation of giddiness. Undoubtedly, however, the effect of even reflex ocular movements released in this way will be such that the displacements of the retinal images produced thereby will be manifested in the form of apparent motions of the visible objects. Thus it is quite possible that an essential part of the apparent motions of the visual objects in giddiness is due to ocular movements, but it is not likely that the latter are an indispensable condition for them.

3. HELMHOLTZ is very emphatic (p. 250) as to how extremely uncertain we are about localizing the entire field of view with reference to our body (that is, psychologically speaking, about judging of the position of the eyes in the head and of the head with respect to the body). There are various other phenomena in which this is noticeable. An idea of the degree of precision can be formed by comparing the optical localization with the haptical (*haptische*) localization, for instance, by trying to touch a visible object with the hand. Above all, of course, we must be careful not to guide the movement of the hand by the eyes. After having first caught sight of the object it disappears, and *then* the finger may be brought to the place where the object was previously visible before; or another way is to hide the finger by a screen while it is being brought to the required place. However, it should be noted that in trials of this kind the precision of that factor, which is supplied by the positions of the eyes or by the effort of will

acting on the muscles of the eyes, is by no means manifested to its full extent; but here also the optical perception of our own body contributes a great deal to the result. Attention has been called to this circumstance especially by E. Fick.[1] Leaving out this kind of help entirely, and testing the ability to put the finger on a visible point in an otherwise absolutely dark room, we find that our success is of the most imperfect kind. Evidently, the possibility of the phenomenon of autokinetic sensation, mentioned previously, and equally also the common illusions occurring in gazing at the moon and clouds, are connected with this uncertainty. The perception of the vertical and horizontal directions, to which we shall return in the discussion below of the so-called Aubert phenomenon, is much more certain.

4. Just as was the case with the theory of giddiness, modern experiments on the static organ have had a profound influence on our present way of regarding the Aubert *phenomenon* described on p. 265. Here we have to begin with the fundamental fact that the physiological significance of the direction of the vertical at any time (or, to be more accurate, of the acceleration due to gravity) is the result of a specially aggressive action on this very organ. The possibility of indicating more or less correctly at each instant the vertical direction, and how the body also is oriented in space, depends on it. Accordingly, when we undertake, for different positions of the body, to adjust a line vertically either by sight or by touch, it means always an involved performance in which not only the function of the static organ but presumably also tactile sensations of various kinds are concerned. These phenomena would have a special optical significance, provided it could be shown to be likely that certain deviations might be due originally to a special mode of functioning of the optical mechanism itself. Thus an extremely interesting question would be whether, for example, (speaking psychologically) the compensatory rollings of the eye remain out of consideration, that is, whether the original vertical meridian is seen to slant in the direction in which the head or the static organ actually does slant. If this were the case, and no other question were involved in the illusions, the line that is really vertical would always have to be turned apparently by the amount of the compensatory rolling in the opposite direction to it, that is, therefore, in the same way as the head is really tilted. However, this is not the case by any means. Hence it is very doubtful whether any such relation as has just been supposed really exists; in any case it is not the only decisive factor in the phenomena. Under these circumstances, it seems to me that significance and

[1] E. Fick. Die Verlegung der Netzhautbilder nach aussen. *Zeitschrift fur Psychologie.* XXXIX. 1905. p. 102.

interest so far as this whole group of phenomena is concerned lies mainly in another direction, as in fact the discussion of them has been especially directed to the static organ. Therefore, the opposite point of view[1] need not be pursued more fully here.[2]

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§30. Perception of Depth

In the two foregoing chapters the apparent configuration of objects was described as they are seen side by side on the surface of the visual globe; and the factors were discussed there which affect the mode of their distribution in the field and the apparent intervals between them. It is true that, in order to simplify the geometrical treatment, we ventured to assume that the form of the field was spherical, although it was expressly stated at the time that all that was intended thereby was merely that it was a surface distribution of two dimensions, without implying in the least any particular form of surface of definite size and position. The form of this surface was left rather completely indefinite. However, just because it was left completely indefinite, we are at liberty to assume any arbitrary form for this surface, as soon as there are any new factors of perception tending to throw light on it.

In the first place, monocular vision suffices simply to enable us to perceive the direction of the observed point. It may move to and fro in the line of sight on which it is situated without producing any change in the impression it makes on the eye except as to the size of the blur circle formed on the retina; and provided this shifting does not exceed the length of CZERMAK's *line of accommodation* (see Vol. I, p. 123), the amount of variation of the blur circle will be absolutely imperceptible. The errors made in the perception of the direction of a line of sight like that just mentioned have been described in the preceding chapter. Thus, in the first place, all that is supplied by monocular vision is simply the *apparent direction of the line of sight*, where the observed point is to be found.

In order to obtain a thorough knowledge of the actual distribution of the observed objects in space, the *distance* from the eye of every point seen in the said line of sight must also be known. Besides knowing

[1] The following investigations of AUBERT's phenomenon may be mentioned: W. A NAGEL, *Zeitschr. f. Psychol.* XVI. 1898. p. 373.—SACHS and MELLER, *Archiv f. Ophth.* LII. (3) 1901. p. 387; also, *ibid.*, p. 7.; and *Zeitschr. für Psychologie.* XXXI. 1903. p. 59.—FEILCHENFELD, *ibid.*, XXXI. 1903. p. 127.

[2] ¶See also: G. E. MÜLLER, Über das AUBERTsche Phänomen. *Zft. f. Sinnesphysiol.*, 49 (1915), 109–246. (J.P.C.S.)

the *surface dimensions* of the field, we must know also its *depth dimensions*. Everyday experience shows that these latter dimensions are estimated more accurately in some cases than in others. The question now, therefore, is to see how we find out the distance of the perceived object from the eye.

Here we have two sources of information which have to be distinguished from each other. One of them depends on experience and some previous acquaintance with the special nature of the perceived object, and enables us merely to form some *idea as to the distance*; whereas in the other method sensation is involved, and we have an actual *perception* of the distance. This latter comprises: 1. *The sense of the necessary effort of accommodation*; 2. *The observation made by moving the head and body*; and 3. *The simultaneous use of both eyes*.[1]

Before considering the latter question as to when and how far the depth of the field can be determined by the perception of distance, we must first inquire what can be learned in this way by experience, so as to distinguish these factors from the others. This first division of the subject will include everything with respect to the depth of the field of view that can be made out with one eye alone, when the head is held in a fixed position, and the objects are all so far away or so blurred that we are not conscious of exerting any effort of accommodation in viewing them. Here we must take into consideration, first, our previous knowledge of the size of the body before our eye, and not only its form but also the shadow it casts, and, lastly, any disturbance it may produce in the air in front of it.

The same object seen at different distances will be depicted on the retina by images of different sizes and will subtend different visual angles. The farther it is away, the less its apparent size will be. Thus, just as astronomers can compute the variations of the distances of the sun and moon from the changes in the apparent sizes of these bodies, so, knowing the size of an object, a human being, for instance, we can estimate the distance from us by means of the visual angle subtended or, what amounts to the same thing, by means of the size of the image on the retina. Persons or domestic animals in a landscape are particularly good objects for this purpose, because they are easy to recognize by their movements, they do not vary much in size, and we are familiar with them. Soldiers, especially are usually trained in this way to gauge correctly the distances of remote bodies of troops in an

[1] ¶Perhaps, it might be well to distinguish between a binocular *perception* of depth and a monocular *conception* of depth. The former is the so-called *stereoscopic* impression, which is something very much more definite and distinct than the mere "*plastic*" impression, which may be obtained more or less vaguely in monocular vision, and which usually involves many concomitant factors. (J. P. C. S.)

unfamiliar country. For military purposes also various little optical devices have been designed for measuring the apparent height of a distant man and then reading the corresponding distance on a scale attached to the instrument. Houses, trees, plants, etc., may be used for the same purpose, but they are less satisfactory; because, not being so regular in size, such objects are sometimes responsible for bad mistakes. A person accustomed to a flat country may easily take a vineyard for a potato-patch or pine trees on distant high mountains for heather, and thus underestimate both the distance and the height of the mountains. Similarly, artists arrange figures of persons and animals in landscapes to enable them to form some idea of the dimensions of the other objects in the scene.

There is another circumstance to be considered in connection with what we have just been saying. If distant objects such as the moon or a range of mountains, owing to their being seen through a haze or for some other reason, are regarded as being farther away, they will invariably appear also to be magnified in size to the same degree. Moreover, anyone looking at a distant landscape through a telescope will be apt to get the impression, not that the objects are larger, but that they are closer; and he has to open the other eye to be convinced that the images have been magnified by the instrument.

Incidentally, this relation between distance and size is something that can only be acquired by long experience, and so it is not surprising that children are not very proficient at it and are apt to make big mistakes. I can recall when I was a boy going past the garrison chapel in Potsdam, where some people were standing in the belfry. I mistook them for dolls and asked my mother to reach up and get them for me, which I thought she could do. The circumstances were impressed on my memory, because it was by this mistake that I learned to understand the law of foreshortening in perspective

Not only may we recognize the size of the object, but frequently we can make out its form also, especially when one of the observed objects happens to be partly hidden by another. For instance, when two hills are visible far away, the base of one extending in front of the other and partly concealing it, we conclude immediately that the hill that is hidden is the more remote of the two; for if this were not the case, the form of the object would be different from that of any other hill that ever was seen; not to mention the strange coincidence that the outline of this peculiar hill should happen to be exactly continued by the countour of the other one. It might be a possible explanation of the picture presented to the eye, but it certainly would be contrary to all experience. Of course, the same sort of thing can occur with all

kinds of objects, when some of them are partially concealed by the others. Even if we are thoroughly unacquainted with their forms, the mere fact, that the contour line of the covering object does not change its direction where it joins the contour of the one behind it, will generally enable us to decide which is which. It is easy to produce optical illusions by intentionally holding a sheet of paper in front of part of an object so that one edge of the paper seems to be the continuation of the part of the contour of the object that is visible.

The most remarkable illusions of this sort are those produced by mirrors or other contrivances which form a real image in the space between the spectator and the instrument. It is hard for most persons to believe that this image is in the air in front of the mirror; for they see gaps in the image where there are little specks on the glass, and notice that the image is limited by the edge of the mirror. In fact, they see all the little defects in the surface of the mirror right through the image itself. The image looks just as if it were the concealed or rear object, whereas it really is in front. Indeed, even when we use both eyes and accommodate them and move the head besides, which are all calculated to aid us and would undoubtedly make us see the image where it really is, still it is not always easy by any means to get rid of the illusion. The best way of doing so is to take a screen with an aperture in it and mount it in the plane where the image is, so as to hide the edge of the optical contrivance without hiding the image. Then the spectator can readily see that the image is in the plane of the screen.[1]

There is another matter of experience which has to do with this subject, and which pertains to subjective visual phenomena occurring when both eyes are open. These appearances always seem to be projected on the surfaces of material objects that happen to be visible in the field of view. They are clearly distinguished from objective appearances, as not having any reality. If they arrest the attention at all, they appear simply like spots on real objects. Generally, indeed, this is what happens, when binocular after-images are developed in both eyes, which might make it possible to perceive a definite locality in space. Ordinarily, instead of forming a stereoscopic apperception of them in space (which never succeeds unless the attention is specially called to it), there is a tendency to project them on real objects in the field.

In many instances it is sufficient to know or assume that the object perceived has a certain regular form, in order to get a correct idea of

[1] See Dove, Pogg. *Ann.*, LXXXV. 1852.

its material shape from its perspective image as presented to us either by the eye or in an artificial drawing.[1] If the objects portrayed are man's handiwork such as a house or a table, we may presume that the angles are right angles, and that the surfaces are flat or cylindrical or spherical. This is enough to obtain correct apperceptions of the object from an accurate perspective drawing. There is no difficulty about comprehending a perspective representation of a building or a piece of machinery, even when the details are fairly complicated. If the shading is good, it is easier still. But the most perfect drawing or even a photograph of a thing like a meteoric stone, a lump of ice, an anatomical preparation or some other irregular object of this sort hardly affords any picture at all of the material form of the body. Photographs especially of landscapes, rocks, glaciers, etc., are usually just an unintelligible medley of grey spots to the eye; and yet the same pictures combined properly in a stereoscope will be the most astonishingly faithful renditions of nature.

When these same regularly-shaped products of human industry, consisting of combinations of rectangular blocks, cylinders and spheres, are inspected a short distance away, so that the images of their anterior parts on the retina are on a distinctly larger scale than those of the posterior parts, we usually can get a correct perspective idea of them, and are not perplexed about what parts are in front, and what parts behind. If, however, they are seen farther off or in very low relief, there may be some doubt as to what they represent. A case of this sort was an observation made by SINSTEDEN[2] on a wind-mill projected against the bright evening sky; so that only half of the side was silhouetted as a uniformly dark object on a bright background, merely its outline being visible. He observed that the wings of the mill seemed to go round first in one direction and then in the other. Looking at it in this fashion, he was unable to tell whether the front side, where the wings were, or the back side of the mill was towards him; or whether he was looking obliquely at the wings from in front or from behind. If he were seeing it from in front, the side of the wings next the mill ought to have been the one nearer to him in perspective; but if he were seeing it from behind, this side ought to have been the one farther from him. According to the interpretation he put on it, the side of the wings next him appeared to go up or down as they revolved, and so merely by changing his idea of the image, he could reverse the apparent motion of the wings. Apparently, it was simply by accident which way the phenomenon was interpreted at first. Nor could it always be

[1] RECKLINGHAUSEN, *Arch. f. Ophthalmologie*. V. 2. (1859). p. 163.

[2] POGG, *Ann.*, CXI. 336–339.—MOHR, *ibid.*, 638–642.

explained why the appearance frequently changed suddenly. On the other hand, the change could always be induced by imagining one's self to be on the other side of the mill. The moment the visual impression accords perfectly with this idea, the idea assumes the *rôle* of a visual perceptual image.

In this category also belongs a drawing given by SCHROEDER,[1] which is reproduced in Fig. 49 without being shaded. It is most readily interpreted at first as the geometrical projection of a stairway alongside a wall, the surface denoted by *a* being nearer the spectator than that denoted by *b*. But it may also be regarded as a piece of overhanging masonry as seen by an observer situated on the left and looking up at it from underneath, the surface *b* now being nearer to him than the surface *a*. It no longer suggests steps, but is more like a mass of work that has been begun on the rear vertical wall *a*, and left only half finished. The first interpretation is the more natural one, and it is therefore the one that is apt to occur first; but yet for no particular reason this idea passes easily into the other. Whichever notion I have at first as to what the figure represents, immediately that apperception of it is formed. If the first view does not pass of itself into the other view, all we have to do, as SCHROEDER says, is to turn the figure slowly around through 180°, watching it all the time. Then the surface *a* supposed to be the one nearer the spectator remains continually nearer to him, until finally when the figure has been completely inverted, it has the same appearance as in the beginning, except that the positions of the letters *a* and *b* have been interchanged, and that now apparently the vertical surface on the right above has become the nearer one. The figure, as given by SCHROEDER, is shaded in two ways, but the result is the same.

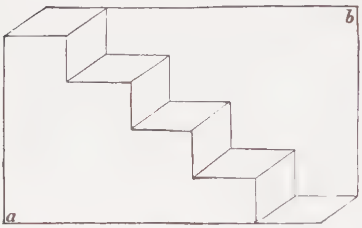

Fig. 49.

The same kind of effect may be observed in numerous perspective line-drawings which are intended to represent geometrical projections of regular objects, models of crystals, etc., as viewed from a great distance. Corners or edges which appear at one time to stand out from the plane of the paper may appear at another time to be behind it. The idea we get frequently changes involuntarily. Still my experience is that we can produce the changes at pleasure, provided we are bent on getting a different picture.[2]

[1] POGG. *Ann.*, CV, 298.

[2] ¶One of the simplest illustrations of this kind is a figure consisting of two equal

The apparent inversion of the relief of matrices for medals is the same kind of phenomenon, although in this case the shadows have some effect also. If an impression of a medal in low relief is made in plaster or wax, representing therefore a matrix in which all the convex curvatures in the original appear concave, and *vice versa*, and then if it is viewed obliquely by daylight, so as to make the shadows show up well, it is easy to believe, when we look at it in this way with one eye, that it is a casting having the original form of the medal. But when we look at it with both eyes, or if we move it or the head to and fro, the illusion disappears. The steadier we hold the eyes and the object too, the easier it is to produce the illusion. Particularly, when the relief represents a human head or body or forms of animals, leaves, etc., the illusion is well-nigh unavoidable under these circumstances, as SCHROEDER has noticed especially. It is much easier not to have this effect when the relief consists of mere letters or filagree.

There is likewise a characteristic illusion in this case due to the illumination. A hollow relief shows the shadows on the parts next the window and the lights on the parts away from it; whereas with a raised relief it is just the opposite.

Consequently, when the matrices look to us like their opposites (patrices), they seem at the same time to be illuminated from the side toward the window. Besides, a raised relief illuminated obliquely in this way would necessarily cast an appreciable shadow on a flat ground, and this shadow would, of course, not be in the matrix seen under the same conditions. Thus we have, as SCHROEDER describes it, a sort of magical illumination of the relief, coming, as it were, from the interior. The reason for it, in my opinion, is that the shadow is absent from the flat ground, making the latter appear to be transparently illuminated.

Incidentally, as was noticed by RITTENHOUSE and subsequently by many others, the illusion may be heightened and facilitated also by reversing the illumination of the matrix. One way of doing this is as OPPEL did in his *anaglyptoscope*,[1] in which the light from the window is intercepted by a screen, but is reflected from a mirror on the opposite side, which the observer does not see. Then the apparent patrix seems to be illuminated by the window. Another way is to view the matrix through a reflecting right-angle prism or through a lens, which produces an inverted image of it. In all these cases the illumination appears to be correct, although there is always something strange about it owing

parallelograms with a common vertical side, intended to represent the perspective appearance of a rectangular card bent in the middle along the common side of the two parallelograms and standing vertically on a horizontal surface. The card may appear to be bent towards or away from the spectator, just as he chooses to regard it. (J.P.C.S.)

[1] POGG. *Ann.* XCIX (1855), 466–469.

to the absence of shadows, especially if the relief is very high. Moreover, the inversion by the lens has the effect on the observer of separating the form from the rest of its surroundings, and necessitates keeping the eye perfectly fixed, because, otherwise, the image of the medal will be hidden by the border of the lens. All these circumstances conduce to produce the illusion. Perhaps this is the reason why it was first perceived in the case of inverted images made by lenses and mirrors.

On the whole, it is much more difficult to make the medal look like the matrix; the reason apparently being because the former usually shows some shadows that make it impossible to interpret convexities as concavities.

A characteristic illusion of this sort has been described by D. BREWSTER.[1] Footprints in the sand looked to him as if they were raised. It seemed that the wind had blown in the brighter sand and heaped it up on one edge, so that this edge was apparently more highly illuminated. SCHWEIZER noticed too that occasionally the relief of the moon was apparently inverted when it was viewed in the daytime through an astronomical telescope.

SCHROEDER also called attention to some other phenomena of a similar kind. When a rectangular strip of paper is laid on a horizontal board and viewed obliquely through a convex lens that inverts its image, then, when the inversion is right, the upper edge of the image of the piece of paper and the board ought to be apparently nearer the observer, and the lower edge farther away. But as a rule it is the other way; the board and the paper seem to be where they really are, and if a little pin is stuck in the paper at an angle, and the flame of a candle adjusted so that a well defined shadow of the pin is cast on the paper, it often happens, as a result of the inversion, that we take the image of the shadow for that of the needle, and *vice versa*. In this kind of illusion, BREWSTER noticed that, owing to the inversion, an intaglio cut in a plane is apt to look like a relief, because the nearer side of it is supposed to be the one farther away.

The shadows cast by the body are of still more importance than the differences of illumination of its surface due to the inclination of the incident light. When we see an illuminated surface, the source of illumination must necessarily be *in front* of it. If a shadow falls on it, the body that casts the shadow must likewise be *in front* of the surface. (The terms "in front of" and "behind" are used here with reference to the surface, and not with respect to the position of the observer.) Accordingly, there is a certain necessary geometrical relation between the body that casts the shadow and the surface on which it is received.

[1] *Athenaeum* 1860. II. p. 24.—*Rep. of Brit. Assoc.* 1860. II. pp. 7–8.

How extremely decisive is the part that shadows play in the interpretation of visual phenomena, will be shown presently in the case of so-called pseudoscopic effects. Everybody knows that we can get a much better idea of an object from a well-shaded drawing than from a mere outline of it; and how much more distinct the view of a landscape is, especially from an eminence, when the sun is near the horizon than when it is high up in the heavens. Here it is not simply a question of the greater richness of the colours when the sun is low down, but it is largely a matter of the better modelling of the forms of the ground due to the clearer shadows. As a rule, few declivities are so steep as not to be illuminated by the sun high above them. And hence at noonday, with rare exceptions, all is bright, and little shadow is present. Consequently, the forms of mountains and valleys, unless they are very rugged, are less distinct. On the other hand, when the sun's rays are oblique, and there is much alternation of light and shade, everything is much clearer and plainer.

The so-called *aerial perspective* constitutes another factor besides illumination that enables us to form some estimate of the distance of objects, especially when they are far away. This term has reference to the clouding and change of colouring in the appearance of distant objects, due to the imperfect transparency of the layers of air in front of them. When the air contains moisture and is a little misty, as it generally is near the earth's surface, particularly in the vicinity of a large surface of water, it acts like a hazy medium, and appears itself bluish when it is lighted up in front of a dark background, at the same time transmitting light from brighter objects and giving them a reddish tinge. The farther the light from the distant object has to travel through the air before it reaches the observer's eye, the more its colour will be altered; more in the blue if the object is darker than the air in front of it, and more in the red in the opposite case. Thus distant mountains appear blue, whereas the setting sun looks red.[1]

The influence of aerial perspective on our judgment may easily be noticed when the air is unusually clear or unusually hazy. In the former case distant mountains look very much nearer and smaller than usual, whereas in the latter case it is just the reverse. A person accustomed to a flat country is liable to a common kind of illusion on

[1] ¶Many allusions to the effect of aerial perspective and distance are to be found in literature, especially among the poets.

'Tis distance lends enchantment to the view
And robes the mountain in its azure hue!

In a fog a person may suppose that there is a big tree some fifty yards or more ahead of him, without suspecting the truth until almost at his next step he stumbles on a bush (J.P.C.S.)

visiting a mountainous region. In the lowlands, particularly in the neighbourhood of large bodies of water, the air is apt to be hazy, but in the mountains the air is usually exceedingly clear and transparent. Consequently, in the mind of a traveller the sharpness of outline of a distant peak, especially if it happens to be covered with snow and sparkles in the sun, is associated only with objects that are near by, and the result is he is apt to underestimate greatly all distances and elevations, until he measures the dimensions themselves and learns better by trial and experience.

This brings us to the famous question as to why the moon looks bigger near the horizon than when it is high up in the sky, although, owing to atmospheric refraction, it ought to look distinctly smaller there along its vertical diameter. PTOLEMY and the Arabian astronomers[1] were perfectly aware that one reason for the moon's seeming to be bigger on the horizon was because it appeared to be farther away there. The real question, therefore, is why does the firmament look nearer to us at the zenith than it does around the horizon. A great many explanations have been proposed, and my opinion is that various causes contribute to produce this effect, so that it is hard to say what is the principal one in each given instance.

In the first place, we must remember that there is no positive reason why the celestial vault should look to us like the regular surface of a globe. It displays objects that are infinitely remote, and all that can be concluded from this fact is that it may appear to us as being a surface of any indefinite shape whatever, if there were some good reason for thinking of it in this way. If we were floating in free space and were able to see the whole wide expanse of the starry sky all at one time, or if the vault were in such rapid motion that we could get a real apperception of it through our senses, there might be more reason for regarding it as being just a spherical surface. But, as a matter of fact, its apparent direction and form is very variable, depending on the piece of it we happen to see, and how it is encompassed by various terrestrial objects, and on whether we are looking specially at some point on it higher up or lower down. We shall see hereafter that, in looking steadily at a point in the sky with both eyes, there is a tendency to regard the vault as a plane at right angles to the lines of fixation at the time.

It is entirely different when the sky is cloudy. It is true that the clouds themselves are generally so far off that we are practically unable to make out anything about their distance by means of our two eyes

[1] MONTUCLA, *Histoire des Mathém.* Vol. I, pp. 309, 352.—ROGERI BACONIS, *Perspekt.* p. 118.—PORTA, *De refractione.* pp. 24, 128.—PRIESTLEY, *Geschichte der Optik.* Periode 6. Kap. 8.

alone and by movements of the body. But they are frequently striated in parallel lines, generally moving over the sky with uniform velocity in the same direction. Near the horizon their upper edges are more definitely outlined, and the illumination is usually such as to enable us to see that they are bodies extending horizontally and foreshortened in perspective. All this helps us to realize that the real form of the cloudy sky, at least near the zenith, is a very flat arch. Of course, on the horizon these considerations are of no aid, and there both the clouds and the mountains look as if they were painted on a surface ascending from below and blending with earth and sky. As we have no way of distinguishing by the senses the interval between the cloudy sky and the celestial dome, it seems only natural to ascribe to the latter the real shape of the former, as far as we can distinguish it at all; and it is probably in this way that we get the very vague, indefinite, and variable notion of the flat, dome-shaped curvature of the sky.[1]

The magnification of the sun or moon, by the way, is not very positive and striking unless the air near the horizon is quite moist, and the intensity of the light from these luminaries is not very great. Then the effect is the same as in the case of distant mountains, and they look much farther, and consequently larger, than they do when the air is clear. Moreover, the effect is much enhanced by the presence of suitable terrestrial objects on the horizon. For instance, when the moon goes down alongside or behind a tree some 2000 feet away, having itself a crest 20 feet in diameter, the visual angle subtended is practically the same in both cases, but the moon seems to be much farther off, and therefore much bigger, than the tree; whereas when it sets below the bare horizon, there is no object close by for comparison, to enable us to tell that, although its apparent size is slight, its actual size is considerable.

When a plane mirror is adjusted so as to bring the image of the moon down near the horizon, I find that this image does not appear to be decidedly larger than the moon itself looks to my naked eye when it is high up in the sky; although the apparent size of the moon's image in the mirror can easily be compared with the terrestrial bodies seen at the same time. But the image in the mirror lacks the appearance of being seen through the hazy portion of the earth's atmosphere.

Moreover, it seems to me that the apparent magnification near the horizon is much more noticeable in the case of the moon than in

[1] ¶See M. Luckiesh, The apparent form of the sky vault. *J. of Franklin Inst.*, 191 (1921), 259-263.—W. Filehne, Über die scheinbare Gestalt des Himmelgewölbes. *Ztschr. f. Sinnesphysiol.*, 54 (1922), 1-8.—See also F. Best, Celestial vault and kindred questions. Depth-perception. *Zent. f. d. ges. Ophth. u. ihre Grenz.*, 7 (1922), p. 449. (J.P.C.S.)

that of the sun. For the latter, when its shape can be distinguished at all, is usually bright enough still to prevent us from looking at it conveniently and from comparing it directly with terrestrial objects near the horizon. But when the sky is quite clear, the illusion is not very good even with the moon. It is always dependent to a very great extent on the state of the atmosphere.[1]

The considerations enumerated above are the only ones that can be utilized by artists to convey an idea on canvas of the material objects portrayed in their sketches and paintings. Their task is easier when the objects are familiar ones (such as forms of men and animals) or of regular geometrical shape (such as buildings, furniture and other manufactured products). For then it usually suffices to make a correct perspective drawing, and if it is shaded properly, the representation may be very life-like and real. The old masters of portraiture, as we know, were especially adept in the art of using deep shadows to bring out the form of the body very clearly. A face a little shaded, but illuminated on all sides, and yet correctly portrayed, certainly makes a vivid impression, provided the person represented is not seen frequently; but otherwise it is apt to lose some of its resemblance to life. The artist's task is harder when he has to portray natural objects of irregular shape, such as landscapes, mountains, and rocks. Accessory figures like men, animals, trees, houses, etc., will afford then an important external means of indicating the distance of the objects in the picture to some extent. But the chief aids in securing this result are aerial perspective and shadows. Accordingly, not every illumination is suitable for representing a landscape. A certain amount of haziness in the air, with the sun low in the sky, productive of much variation of light and shade, is a prerequisite for bringing out clearly the forms in the landscape, aside from the richer and variegated colourings which also enhance its beauty.

The factors involved in the apperception of depth, which have been described thus far, are likewise interesting and important from a psychological point of view, because they show the influence experience has on the seemingly direct perceptions of the senses, with which the mental activities have had nothing to do. It is only by experience that we ever could have learned about the laws of illumination, shading, atmospheric haze, geometrical perspective, concealment of one body by another, the sizes of men and animals, etc. At any rate, no advocate of the intuition theory has yet ventured to maintain that the origin of these apperceptions was intuitive. Some of them

[1] Concerning illusions as to the size of celestial objects, see Note 1 at the end of this chapter.—K.

take long practice, and in such instances it can be demonstrated directly with children, as has been already stated, that they are not instinctive. And yet in numerous circumstances these factors are sufficient to evoke an apperception of spatial forms and relations as vivid as though they were the results of sensation, without our being in the least aware of the interplay between the impression at the time and previous impressions of a similar sort. The image at the time awakes the memory of everything like it experienced in previous visualizations, and likewise the recollection of everything regularly associated by special experiences with these former visual images, such as the number of steps we had to take to reach a man who appeared in the field of view to be of a certain size, etc. This kind of association of ideas is unconscious and involuntary, and is produced by a sort of blind force of nature, no matter if it occurs also according to the laws of our mental being; and hence it enters into our perceptions with all the external and compelling power of impressions that come to us from outside. And so whatever is superposed on the sensations at the moment, as the result of these associations of ideas based on all our experiences collectively, seems to us to be communicated directly without any effort of will or conscious activity on our part, just like the sensations themselves; that is, seems to be immediate perception, needing simply to be taken into account in forming our ideas.

In this connection, those cases involving illusions about the relief of medals or about perspective drawings, etc., in which there may be a fluctuation between two opinions are of special interest. Here we discover that at first we espouse one of these opinions involuntarily, naturally the one that recalls the greatest number of similar memory-images. This is illustrated in the case of the relief of human faces where we are apt to believe that we see the convex form corresponding to the reality. In other instances the interpretation wavers involuntarily, as in the case of SINSTEDEN's wind-mill, where, owing to external accidents or movements of the eye, first one opinion prevails, and then the other. But an intentional change of opinion may also be produced by imagining the appearance of the alternative figure as vividly as possible, until its similarity with the visual image that has just been seen begins to appear, and then this picture will persist by itself without any further effort to produce it. But during the time it lasts it exists with all the energy of sensational certainty; and if, as the result of some change of conditions, the alternative interpretation recurs once more, it possesses again the same clearness and sureness, although now conscious attention has been called to the fact that it is a question of ambiguity of apperception.

We proceed now to discuss the second class of factors that are involved in perception of depth, namely, those that depend on definite sensations. The first thing to be considered here is what effect the *accommodation of the eye* may have in this respect. Undoubtedly, any one who has had occasion to watch the changes of accommodation of his eye and knows the muscular feeling connected with it, is able to tell, in looking at an object or an optical image, whether he is accommodating for great distances or small distances. But judgment of distance in this way is exceedingly unreliable. WUNDT[1] made experiments on this subject, in which the observer was required to look with one eye through an aperture in a screen at a black wire stretched vertically. A white wall formed the background. The wire could be shifted along a horizontal scale and adjusted at measured distances from the observer. While practically nothing could be determined by this method as to the actual distance of the object, still it was very evident from the necessary change of accommodation for two successive positions of the wire whether it had been moved nearer or farther. However, when the wire was brought closer to the eye, thereby involving an increase of active muscular effort on the part of the mechanism of accommodation, the effect was easier to detect than when the wire was moved farther away from the eye. The fatigue of the eye incident to the experiments rendered it more and more difficult to be certain about the perception, even when the object was brought closer to the observer. The results of WUNDT's experiments were as follows.

Distance of wire from eye	Minimum perceptible difference when the wire was moved	
	nearer the eye	farther from the eye
250 cm	12	12
220	10	12
200	8	12
180	8	12
100	8	11
80	5	7
50	4.5	6.5
40	4.5	4.5

When two wires were exposed simultaneously at different distances, the results were the same as when the single wire was moved toward the observer.[2]

At one end of a tube, painted black on the outside, I mounted a black screen with two vertical slits in it, one of which was covered by

[1] *Beiträge zur Theorie der Sinneswahrnehmung.* Leipzig and Heidelberg, 1862. pp. 105–118.

[2] See Note 2 at end of chapter.—K.

a piece of red glass and the other by a piece of blue glass. It required a decidedly greater effort of accommodation to see the red mark distinctly than it did to see the blue one. Finally, after the two marks had been compared for a considerable length of time, the impression prevailed that the red mark was nearer and the blue one farther; but the illusion was hard to obtain and disappeared quickly, the only way of maintaining it being to vary the accommodation continually, first for one mark and then for the other. It was enhanced by making the red mark a little wider than the other one, and so giving it the appearance of being nearer.[1]

More important, however, for the estimation of distances, and more accurate than all the incidental aids above mentioned, is the comparison of the perspective views of the same object as seen from different points. Practically, there are two ways in which this can be done, either in monocular vision by moving the head and body, or in binocular vision by means of the two different images of the same object at the same time in the two eyes. Since the two eyes occupy positions in space that are not quite the same, the objects in front of us are seen from two slightly different points of view, and, consequently there is the same kind of difference in the images as would be produced by moving in space from one place to the other.

In walking along, the objects that are at rest by the wayside stay behind us; that is, they appear to glide past us in our field of view in the opposite direction to that in which we are advancing. More distant objects do the same way, only more slowly, while very remote bodies like the stars maintain their permanent positions in the field of view, provided the direction of the head and body keep in the same directions.[2] Evidently, under these circumstances, the apparent angular velocities of objects in the field of view will be inversely proportional to their real distances away; and, consequently, safe conclusions can be drawn as to the real distance of the body from its apparent angular velocity.

Moreover, in this case there is a relative displacement of objects at different distances with respect to each other. Those that are farther off as compared with those that are nearer seem to be advancing with the observer, whereas those that are nearer seem to be coming toward him; and the result is we have a very distinct apperception of the fact that they are unequally far from us. Suppose, for instance, that a person is standing still in a thick woods, where it is impossible for him

[1] Concerning the origin of depth-impressions due to differences of colouring, see Note 3 at the end of the chapter.—K.

[2] See Note 4 at the end of the chapter.—K.

to distinguish, except vaguely and roughly, in the mass of foliage and branches all around him what belongs to one tree and what to another, or how far apart the separate trees are, etc. But the moment he begins to move forward, everything disentangles itself, and immediately he gets an apperception of the material contents of the woods and their relations to each other in space, just as if he were looking at a good stereoscopic view of it.

Evidently, too, the direct impression on the sense of vision produced by these apparent relative motions of the various trunks, branches, and leaves of the trees of the actual woods will necessarily be entirely different from that which could be obtained from a painting of this forest, no matter how perfect it was. In going past a flat canvas with a picture on it, the apparent positions of all parts of it with respect to each other remain the same all over in the field of view. A part of the painting which represents more remote objects moves with respect to the observer exactly in the same way as an adjacent part on which a nearer object is portrayed. All that a painting can ever do is to represent the view of a scene as it looks from some single fixed point of view. If it is intended to produce the most perfect illusion that can possibly be obtained, the spectator must not move from the spot where he is expected to stand. The effect of every movement is to bring out instantly the difference in visual appearance between the original and the copy.[1]

Objects that are nearer appear to move faster, those that are farther appear to move more slowly. When the spectator himself is going unusually fast, on a train, say, the objects rapidly whirling past him may easily seem too near, and, therefore, smaller than they really are. This is a visual illusion which has often been observed and described.[2] Personally, I have never been able to notice this particular contraction of objects very distinctly, but there are many illusions of this kind that cease to be apparent to a person who is accustomed to close observation and has learned not to let his judgment of visual phenomena be warped by these disturbing influences.

Even in scientific observations the apparent relative displacements of objects at different distances can frequently be utilized. For instance, when the cross-hairs in a telescope are to be adjusted exactly on the image of an object, the observer need only move his eye a trifle

[1] "Much of the delight which a picture is intended to afford may be lost in this way by not viewing it from the proper standpoint. And one reason why photographs, especially, so often convey an entirely false impression of perspective is because they were taken with lenses of such short focus that it is impossible to view them with the naked eye at the proper distance. (J.P.C.S.)

[2] Dove, Pogg. *Ann.*, LXXI (1847), p. 118.

to and fro as he looks in the ocular. He can tell immediately whether or not the cross-hairs stay still with respect to the image. If they do, they are in the same plane with the image; and if they do not, they are either in front of it or beyond it, and it is easy to tell at once which way it is.

It is well known that the parallaxes of the fixed stars are determined in this same way, the earth's motion around the sun supplying the observer's motion in this instance.

My belief too is that it is mainly by variations of the retinal image due to bodily movements that one-eyed persons are able to form correct apperceptions of the material shapes of their surroundings. If anybody with two good eyes will close one of them and look at unfamiliar objects of irregular form, he will be apt to get a wrong, or at any rate an unreliable, idea of their shapes. But the instant he moves about, he will begin to have the correct apperceptions.

There is another point to which sufficient importance is not always attached. In experiments in physiological optics, where the problem consists in estimating the distance of some observed object or image, care should be taken to see that the position of the head does not vary with respect to the thing observed; because it is possible to make a comparatively good and accurate determination of the real distance directly from this very displacement alone.

In the variations of the retinal image alluded to above which are the results of movements, the only way an apperception of differences of distance is obtained is by comparing the instantaneous image with the previous images in the eye that are retained in the memory. In connection with the theory of contrast, it was observed that a comparison from memory is apt to be much more unreliable than the comparison of two simultaneous impressions of the senses. Similarly, an estimate of distance by means of simultaneous images in the two eyes is much more perfect, reliable and accurate than can be obtained by any movements that are not more extensive, at any rate, than the slight interval of separation of the two eyes.

Each eye by itself gives us a perspective view of the objects in front of us. But as the two eyes do not occupy the same place in space, and therefore view objects from somewhat different standpoints, the two perspective views of them are slightly different from each other. When I hold a sheet of paper in front of me in the median plane of my head, the surface on the right-hand side is viewed with the right eye, and the surface on the left-hand side with the left eye. In the image in my right eye the farther end of the sheet of paper appears to be to the right of the nearer end; whereas it appears to be to the left of

it in the image in my left eye. More careful examination will reveal numerous similar differences, more or less obvious, whenever objects at various distances are viewed by both eyes. They are such differences in kind and extent as occur in case of monocular vision when the eye is moved about over a distance that does not exceed the interpupillary distance.

On the other hand, in looking at a flat drawing or painting, the retinal images in the two eyes are practically the same, except for the perspective distortions that may possibly be produced by the plane of the painting itself in the images on the two retinas. But the object that is portrayed in the picture, unless it too happened to be flat, would necessarily produce different retinal images in the two eyes. Here again, therefore, in the direct apperception of the sense of sight there is something that indicates a difference between the view of a solid object of three dimensions and the view of a flat picture of it.

It is obvious too that, when the places are known where the images of a luminous point are on the retinas of the two eyes, the position of the luminous point can be definitely located thereby, theoretically anyhow, although the observer himself may not be aware of it. All that we have to do is to draw a straight line through the nodal point of each eye and the retinal image in that eye; and since the luminous point itself must be on each of these two direction lines, it must be therefore at their point of intersection.

Thus, whereas all that we know in case of monocular vision, with the head stationary, is the direction of the point seen, binocular vision affords us sufficient facts of observation for determining the distance of this point also, provided, of course, the requisite data are accurate enough for that purpose and can be conveniently employed. In general, the accuracy of the determination of distance is less in proportion as the distance itself is greater, because there is practically no difference between the images of distant objects in the two eyes.

The fact that exceedingly accurate and distinct visual apperceptions of distance can indeed be obtained in this way, may be shown by means of *stereoscopic views*; that is, pictures representing in each case two corresponding views of an object, one for the observer's right eye, and the other for his left eye.

It has been stated that a single flat picture as seen by both eyes necessarily gives a different impression from that which would be obtained by looking at the object itself. But suppose different pictures are exposed to the two eyes, so that each eye sees the same image as it would have seen by looking at the object itself; then we are enabled to produce the same impression on each retina as the actual material object in space would have produced. Under such circumstances,

the two pictures together will produce indeed the same apperception of bodily form as would be produced by looking directly at the real thing.

Two pictures intended to give a stereoscopic effect must represent, therefore, two different perspective views of the same object as seen from different standpoints. And so the two pictures must not be the same, but, as compared with the images of infinitely distant parts of the scene, the images of nearer objects must be more toward the left in the picture intended for the right eye, and more toward the right in the picture intended for the left eye, in proportion as the objects are nearer the observer. Suppose, therefore, the two pictures are superimposed on each other, so that the images of infinitely distant points coincide in the two representations; then the corresponding images of each nearer object will be farther and farther apart, the nearer the object is. This distance between corresponding images of the same object may be called the *stereoscopic parallax*. It is reckoned as being positive when the image for the right eye is over to the left and for the left eye over to the right. The stereoscopic parallax has the same magnitude for all objects at the same distance from the plane of the picture.

If there are no infinitely distant objects represented in the picture, all we can do is to find the difference of stereoscopic parallax with respect to some arbitrary point of the object. The parallax with respect to an origin of this sort will be reckoned positive for points that are nearer than it, and negative for points that are farther away.

Let $2a$ denote the distance between the centres of rotation of the two eyes, b denote the distance of the pictures from the eyes, ρ denote the distance of the object from a plane through the two eyes parallel to that of the picture, and e denote the stereoscopic parallax; then

$$e = \frac{2ab}{\rho}$$

and hence the stereoscopic parallax is inversely proportional to the distance of the object, vanishing entirely when the latter is at infinity.[1]

The pair of *stereoscopic* views in these experiments must be so adjusted in front of the observer that the infinitely distant objects represented in the pictures will appear in the same direction for both eyes. This can be done without any apparatus by placing the two pictures side by side, one on the right and the other on the left, with the corresponding points in the two views at about the same distance apart as the observer's interpupillary distance. Then with the visual

[1] Concerning more modern ways of expressing these magnitudes, see Note 5 at the end of this chapter.—K.

¶See derivation of formula on p. 332. (J. P. C. S.)

axes of the two eyes parallel, the observer will see both pictures with both eyes and will obtain the stereoscopic effect. It is true that in this case he will see with each eye not only the picture intended for that eye, but also alongside of it the picture intended for the other eye as well. Accordingly, when the proper position of the eyes has been obtained, the observer will see apparently three images side by side, the two outer ones being viewed with one eye alone (the right one by the left eye, and the left one by the right eye) and not making any "plastic" impression, whereas the one in the middle, which is viewed by both eyes together, will produce such an impression.

In this mode of experiment the presence of the three images creates a disturbance; and, besides, the accommodation has to be changed for near objects without converging the eyes at all, which is contrary to the way we are accustomed to using our eyes. And so it takes some practice to learn how to get stereoscopic vision without any other external aid. Incidentally, the visual illusion obtained in this fashion is just as perfect as it is when the instrument is used which is about to be described. Unskilled observers can succeed with the experiment by using two tubes painted black on the inside and looking through them at the two stereoscopic views, because in this case the outer images will disappear. Under these circumstances, the interval between the two drawings should be slightly less than the inter-pupillary distance. With a little practice one can succeed even without this help, and, in fact, this is the most convenient way of viewing a large number of stereoscopic views in succession. Instead of directing the visual axes of the two eyes to a very remote point so that they are practically parallel, they may also be converged on a nearer point, and the two images made to fuse, by turning the right eye toward the one on the left and the left eye toward the one on the right, so that the lines of fixation of the two eyes intersect at a point between the observer and the plane of the pictures. In this case the adjustment of the eyes is the same as if they were both gazing at this intermediate point, the stereoscopic object appearing to be situated there, that is, nearer the eyes than the pictures actually are. But, of course, in this experiment the picture intended for the right eye must be placed on the left-hand side, and that for the left eye on the right-hand side; otherwise, the stereoscopic parallax will be negative, and the relief reversed. This may readily be verified by fusing two unshaded line-drawings (representing models of crystals, for instance), first, with the lines of fixation uncrossed, and then with these lines crossed.

The sole object of the so-called *stereoscope* for viewing stereoscopic pictures is to facilitate and maintain the proper adjustment of the

observer's eyes and to get rid of the disturbance due to the secondary images. It has no special advantage so far as the actual production of the visual illusion is concerned.

The first instrument of this kind was invented by WHEATSTONE. It is shown in section in Fig. 50. The apparatus consists essentially of two mirrors ab and $a\beta$, each inclined to the horizontal at an angle of 45°, the reflections taking place at their upper surfaces. The sides are made by two parallel boards cd and $\gamma\delta$, against which the pictures are placed. The observer's eyes, represented by r and ρ, look down on the mirrors. Light coming from cd is reflected by the mirror ab into the eye r, as if it had come from the image ff. Likewise, light coming from $\gamma\delta$ is reflected by the mirror $a\beta$ into the eye ρ, as if it had come from the image ff. Thus both eyes apparently see the image ff, and in case the images are different in detail, the observer gets the same impression by his sense of vision as if he were looking at the object extended in space and not at pictures themselves at ff. Since the drawings in this case are viewed by reflection in the mirrors, which perverts them right and left, they must have negative stereoscopic parallaxes.

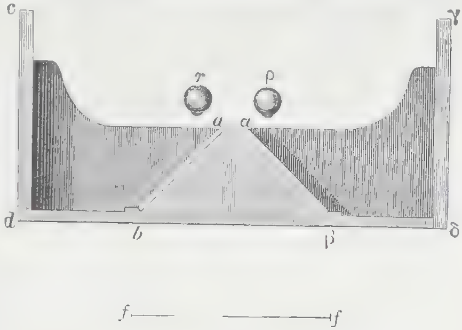

Fig. 50

BREWSTER's stereoscope, which is the one most widely used nowadays, contains two prisms p and π (Fig. 51), with convex surfaces; being in fact two pieces of a thick double convex lens of 18 cm focus, each of which acts optically like the combination of an ordinary plane prism and a convex lens. The two pictures ab and $a\beta$ are mounted on a single card. The right eye (r) views the picture ab through the prism p; and the left eye (ρ) views the other picture $a\beta$ through the prism π. The partition g prevents each eye from seeing the picture intended for the other eye. The rays cp and $\gamma\pi$ coming from the two pictures are refracted by the prisms in the directions pr and $\pi\rho$, respectively. Prolonged backwards, these refracted rays intersect at the point designated by q. The effect of the

Fig. 51.

convex surfaces of the two prisms is to make the bundles of rays less divergent, so that each eye will see an image of the picture exposed to it at the place shown in the diagram by $f\varphi$; where therefore the material object seems to be situated. The whole affair is enclosed in a suitable wooden box. In order to view transparencies, there is a ground glass plate behind the pictures $ab\alpha\beta$. The latter may be inserted in the side of the box through slits at a and β.

Brewster's stereoscope is much more compendious than Wheatstone's. It is easier to illuminate both pictures uniformly; and, besides, the pictures are magnified. Still it should be noted that there are little coloured borders on the boundaries between bright and dark unless the prisms are achromatic combinations, which, by the way, is actually the case in many of these instruments. Other forms of stereoscope will be described presently.

The most conspicuous stereoscopic effects are produced by pictures that show simply the outlines of bodies and surfaces, where there is nothing else to promote the illusion such as colour and shade; and yet the black lines are completely lifted off the surface of the paper and seem to be drawn in space. Even the most complex stereometric drawings, representing models of crystals, which are scarcely intelligible without a stereoscope, can be made perfectly clear and will look like figures in space.

Although the difference between the stereoscopic and non-stereoscopic views is most remarkable in the case of line drawings of this sort, the vividness of the illusion itself is greatest, of course, when the form of the body is brought out also by a correct shading. And yet it is well-nigh impossible to reproduce exactly with a pencil or brush all the little delicate differences of shade in the two drawings that correspond to the images as they are depicted in the two eyes. This exact agreement between the two images that is requisite for a good stereoscopic impression can be obtained only by photography. And as stereoscopic photographs of this sort are now very common everywhere I may assume that my readers are all familiar with them. They are made by photographing the same object twice, only from a slightly different point of view in each case. This may be accomplished simultaneously with two cameras or in rapid succession with one apparatus. Two cameras are needed particularly when the objects are undergoing quick changes. Even when the objects are illuminated directly by the sun, the shadows are frequently shifted appreciably during the interval between taking the two pictures, as it sometimes takes five or ten minutes to focus the camera for the second picture. The use of two cameras is practically indispensable for taking so-called instantaneous photographs of moving objects, such as waves, ships,

horses, etc., where the time of exposure in case of bright sunlight and very rapid plates may amount to no more than a fraction of a second.

These stereoscopic photographs are so true to nature and so lifelike in their portrayals of material things, that after viewing such a picture and recognizing in it some object like a house, for instance, we get the impression, when we actually do see this object, that we have already seen it before and are more or less familiar with it. In cases of this kind, the actual view of the thing itself does not add anything new or more accurate to the previous apperception we got from the picture, so far at least as mere form relations are concerned. How much we do gain by stereoscopic vision is seen best, of course, in pictures of objects which are not well adapted for pictorial representation on a flat surface or canvas, for example, pictures of irregular rocks, blocks of ice, microscopic objects, animals, woods, etc. Particularly impressive in their effects are pictures of glaciers with their deep crevasses illuminated through the mass of ice itself. The single picture, viewed by itself, is apt in these cases to give simply the impression of an unintelligible jumble of grey splotches; whereas in the stereoscopic combination not only the forms of the blocks of ice, but the light reflected and transmitted all come out most distinctly. The reason why the single picture is so hard to comprehend in this case is because irregular shapes like blocks of ice are not clearly reproduced by simply illuminating them by incident light, the ordinary laws of shading being completely changed by the transmitted light.

The stereoscopic representation of brilliant objects is also very beautiful, such as the surface of water with ripples on it. However, this is a part of the subject that must be reserved for discussion in the next chapter.[1]

Let us proceed now to see how accurate are our estimates of depth dimensions in the field of view as the result of the simultaneous activities of the two eyes. Here we must make a distinction between *the estimation of the absolute distance* of the object from the eye and *the estimation of the difference of distance* between two different points. Except for the considerations which we have just been discussing,

[1] ¶See also the following: C. PULFRICH, *Stereoskopisches Sehen und Messen*. Jena, 1911.—Idem, Art. on "Stereoscope" in *Encyc. Brit.*, 11th ed. (1911).—A. v. SZILY, Stereoskopische Versuche mit Schattenrissen. GRÄFES *Arch.* 105 (1921), 964-972.—J. W. FRENCH, Stereoscopy re-stated. *Trans. Opt. Soc.*, 24 (1923), 226-256.—E. DIAZ-CANEJA, Stereoscopic vision. *Arch. de Oft. Hisp.-Amer.*, 23 (1923), 224-230.—E. LAU, Versuche über das stereoskopische Sehen. *Psychol. Forsch.*, 2 (1923), 1-4.—L. E. W. VAN ALBADA, A wide-angle stereoscope and a wide-angle view finder. *Trans. Opt. Soc.*, 25 (1924), 249-258.—R. J. TRUMP, Binocular vision and the stereoscopic sense. *Trans. Opt. Soc.*, 25 (1924), 261-270. (J. P. C. S.)

estimates of actual distance must be based simply on the sensation of the absolute amount of convergence of the two lines of fixation at the moment when they are directed to a certain point of the object. The difference between the two retinal images cannot contribute anything here; or at any rate it would seem that such differences as might be of some value for this purpose are too inconsiderable to be of any real use.— The estimate of the difference of distance between different objects will depend on the difference between the images on the two visual globes. It may depend first on the perception of some distinction between the two retinal images when the lines of fixation were both stationary; or on the perception of the different motions that occur as the eyes change their focus from one point of the object to another. No experiments thus far have indicated any difference in the acuity of perception as the result of avoiding or executing ocular movements, and hence it would seem that the images on the two retinas must be compared with such extraordinary delicacy that there is no need of taking account of the differences of movement in this connection. However, as we shall see presently, the evidence for the illusion is supported mainly by the movements of the eyes, especially in the case of images that are hard to combine.

We shall take up first the estimation of *differences of distance* in so far as this judgment depends on a comparison of different retinal images. However, it should be stated that, while the images on the two visual globes are not sufficiently different for us to be aware of the differences themselves, still we do notice and estimate the differences in the depth-dimension that are due to the otherwise imperceptible differences in the images.

The comparison between the images on the two retinas, as indicated by the use that is made of it in perceiving the depth-dimensions of the field of view, must be extraordinarily accurate. It enables us to distinguish differences of this nature that could scarcely be perceived in any other way without the aid of special instruments of mensuration. Even in the ordinary stereoscopic photographs the differences between the two views are generally so minute that it takes exceedingly careful examination to detect them. Usually these differences are not noticed except along the contours of objects in the foreground which may hide those beyond them a little more in one view than in the other.

As an illustration of the accuracy of stereoscopic vision the following examples given in DOVE's *Optische Studien* (Berlin, 1859), pp. 25-36, may be instanced. If two medals made of different metals, but stamped with the same die, are combined stereoscopically, the resultant image appears to lie obliquely and to be curved and not flat. The explanation is that the metals, being compressed by the die, and

having different elasticities, do not recover from the pressure to exactly the same extent. The result is that medals made of different metals are not indented precisely the same way, although the differences are extremely minute. Professor DOVE showed me samples of two medals, one of which was silver and the other bronze, which were so nearly alike that it was impossible to detect any difference between them with the naked eye, even when they were placed one on top of the other; and yet the resultant image which they gave was distinctly curved.

If the same set of letter-type is set up twice in a printing press, it is impossible, at least without taking extraordinary pains, to make the distances between the type exactly the same both times. The result is that when the two prints made in this way are viewed in a stereoscope single words and letters will stand out in front of the others or behind them. A piece of printing of this sort will not look flat unless the two copies are made with exactly the same set of type; and even then the whole image may appear curved and oblique, in case the sheet of paper has become somewhat stretched by differences of moisture or tension. In this case, however, there will not be any apparently irregular elevations of single letters.

Just as we can distinguish in this way the second impression of a book from the first, so also counterfeit paper money can be distinguished from genuine bank notes, because it is not possible to make the spaces between the letters in the copy so precisely the same as in the original that certain letters will not appear elevated or depressed, when the counterfeit and genuine notes are viewed together in a stereoscope. Even two genuine notes of the same denomination will have some portions of this kind that are perhaps not exactly in the same plane owing to the fact that they have been printed on different presses; and the stereoscope enables us to tell easily how many presses were used in printing the paper. This method is very convenient also for checking linear scales and seeing whether the spaces are all of the same size. All we have to do is to compare two different parts of the same scale under the stereoscope. If the two parts are of the same size, all the marks will appear to lie in one plane. But if the divisions are irregular, some of the marks seem to be nearer or farther away than others.

Another illustration of how easy it is to make these minute variations visible by stereoscopic combinations (which I found out accidentally) is as follows. Gaze attentively at the paper on the wall of a room, looking at it with one eye perfectly free and with the other eye through the column of hot air ascending from the chimney of a lighted lamp. The paper will appear to have a large fold in it going in and out, as if it had come loose from the wall. If it is the right eye that looks through

the heated air, the fold in the paper seems to come out from the wall on the right and to go back of it on the left. With the left eye, it will be just the reverse. The phenomenon is seen most distinctly when the observer is stationed about three feet from the wall with the lamp midway between. Then the folds in the paper coming out from the wall will fall in the same place for both eyes, and thus the effect will be enhanced. The phenomenon is explained by the refraction of light by the hot current of air. The circle A in Fig. 52 is intended to represent the cross section of this ascending current; the two eyes of the observer being at the points designated by r and ρ. To the eye r the points a, b and c on the wall will appear to be in the directions of the straight lines ra, rb and rc, respectively. But owing to the refraction in the air-current A, the light comes to the other eye ρ along the paths $aa_1\rho$, $b\rho$ and $cc_1\rho$; these lines all being bent except the middle ray $b\rho$. Accordingly, the eye ρ will see the points c and a in the prolongations of the straight lines ρc_1 and ρa_1, respectively; and hence the two eyes together will see the points c and a at γ and α, where ρc_1, rc and ρa_1, ra intersect, respectively. And so the wall-paper appears to project forward on the side next the eye which is gazing through the hot current of air, and to be beyond the wall on the side next the free eye.

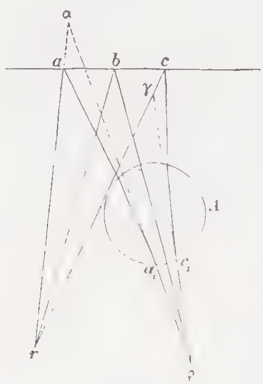

Fig. 52.

I have also made some experiments to determine the degree of accuracy that can be obtained in the stereoscopic comparison of the two images on the retina. The apparatus consisted of three small square wooden blocks placed on a table side by side, each carrying a vertical pin stuck in it near one end. The pins were 12 mm apart all in one line and approximately in the same plane. My eyes were placed on the level of the plane of the upper surfaces of the three blocks of wood, or just a little below it, so that I could see the three pins without being able to see the farther edges of the blocks where the pins were fastened. The distance between the pins and the eyes was 340 mm. Thus it was only by comparing the two retinal images that I could tell whether the pins were exactly in one vertical plane or not. In case they were not, one of the blocks could be shifted until the pins were all in the same vertical plane as well as the observer could tell; and then afterwards he could place his eye in this plane and

look along the row of pins and tell easily how well they had been adjusted. It should be noted that the pins must not be too far apart, or else a characteristic illusion of judgment will be involved which will be discussed in the next chapter in connection with the theory of the horopter. The intervals mentioned above are suitable for the purpose of the experiment, the effect of the illusion referred to being negligible then. Under these circumstances, when the plane of the pins was perpendicular to the visual axis, I have never made a mistake amounting to as much as half the thickness of a pin, that is, a quarter of a millimetre. When the plane of the pins was considerably inclined to the visual axis, the comparison was not quite so reliable. When one of the pins was out of the plane one way or the other by an amount equal to its own thickness (half a millimetre), it was possible to detect it with absolute certainty. Under these conditions, it was a simple matter to calculate the difference of position of the images of the middle pins in the two eyes as compared with the images of the other two pins, supposing the middle pin was half a millimetre in front of the plane of the other two. The distance of my two eyes apart is 68 mm. Projected on the plane of the two outer pins, the difference of position of the two retinal images of the middle pin would have been $(1/2)(68/340) = 1/10$ mm. A width of 0.1 mm as seen from a distance of 340 mm is near the limit of the least perceptible interval. It corresponds to an angle of 60.5″ or to a distance of 0.0044 mm on the retina. The result is, therefore, that *the comparison between the images on the retinas of the two eyes can be made with the same degree of accuracy as that of the perception of the smallest interval in monocular vision.*[1]

Brewster noticed that very minute differences, due to differences of refrangibility of rays of light of different colours, were also manifested by looking through a convex lens two or three inches in diameter at two objects the same distance away, one of which was red and the other blue. The red object in this case will seem to be nearer than the blue.

The stereoscopic capacity for discriminating distances diminishes rapidly for more distant objects. The mathematical law is similar in form to that for images in a convex lens. Let r and ρ denote the distances from the eyes of the farther and nearer points, respectively; then the interval between them can be distinguished, provided

$$\frac{1}{\rho} - \frac{1}{r} > \frac{1}{l},$$

[1] See Note 6 at the end of the chapter.—K.

where f denotes a certain constant on which the accuracy of the discrimination depends.

According to the results of the measurements above given, the value of f may be put equal to 240 m or more. If r and ρ denote the distances of object and image from a convex lens, whose negative focal length is equal to f, then

$$\frac{1}{\rho} - \frac{1}{r} = \frac{1}{f}\,.$$

Thus, look at any object through an extremely feeble concave lens of focal length 240 m. Its image in the lens will be at the place where another object would have to be in order for us to tell by stereoscopic vision that it was farther away than the first object. Anyone who is in the habit of viewing images in lenses will perceive at once that the intervals must be very large to be detected when the objects are far away, and that, on the contrary, they are very minute when the objects are close.

The magnitude denoted by f in the above formula denotes the greatest distance of an object that can be lifted by stereoscopic vision from the infinite background of space.[1]

An instrument called the *pseudoscope*, which is a modification of the stereoscope, is very instructive for showing how powerful this stereoscopic impression of differences of distance is as compared with the other aids to vision. This instrument is intended to change the binocular images of real objects so as to give a false stereoscopic relief.

[1] ¶Suppose the centres of the two eyes are designated by P and Q ($PQ = 2a$); and suppose also that the binocular perception of depth has a limit such that it is just possible to discern that a point (which we may call R) is farther away than a certain definite point (which we may call S). The small angle $SQR = \alpha$ (say) is called the *limiting angle of binocular perception of depth*. Evidently, if the distances of S and R are denoted by ρ and r, respectively, we may write (approximately at least):

$$\alpha = 2a\left(\frac{1}{\rho} - \frac{1}{r}\right) = \frac{2a}{f},$$

where the magnitude denoted by f is the same as that defined in the text. It is sometimes called the *radius of stereoscopic vision*.

This angle α is quite small and certainly less than one minute of arc. It varies with different individuals, but the conventional value that is ordinarily used is $\alpha = 30''$. Substituting this value, that is, $\alpha = 0.000145$ radian, in the formula given above, and assuming (for the sake of obtaining the result in round numbers) that the interpupillary distance is $2a = 65.25$ mm, we find for the radius of stereoscopic visron $f = 450$ metres; that is, f is about 7000 times as great as the interpupillary distance. (With the data above mentioned, HELMHOLTZ finds $f = 240$ metres, which is much too small.) The differences between the retinal images of any visible object which is more than 450 metres away from the naked eyes are too slight to be appreciated. All such objects appear to be flat and to lie on the infinite background of space. (J.P.C.S.)

WHEATSTONE's pseudoscope is composed of two right-angle glass prisms whose edges are placed perpendicular to the plane of sight, and through which the observer looks in a direction parallel to the hypothenuse faces. The way the rays go through a prism of this sort was explained in connection with Fig. 6. An object viewed through this prism, which lies in the direction of the undeviated ray that enters and emerges parallel to the hypothenuse face, will be seen in its correct place. But objects lying alongside of this will be shifted in position by the internal reflection, those on the right being shifted to the left, and *vice versa*. As each eye sees the objects symmetrically reversed in this way by reflection, the images in the two eyes will again be in agreement with each other. The two prisms, by the way, are inserted in short tubes with their hypothenuse faces parallel to the axes of the tubes. The latter must be capable of rotation both around their own axes and around axes perpendicular to the visual plane, so that the two images may be brought into corresponding adjustment.[1]

A simple illustration will suffice to show that under these circumstances there must also be a reversal of the stereoscopic relief. Suppose that the object is a rectangular block placed symmetrically with respect to the median plane of the head. Both eyes will see its front surface; but the right eye will see also some portion of the right side of the block, and the left eye some portion of the left side. But now look at the object through the pseudoscope. The part that was seen on the right side looks now to the right eye as if it were situated alongside the front surface on the left; whereas the left eye sees a portion of one of the sides on the right of the front surface. But this cannot be the case with a solid block, although it might be the case with a hollow trough of rectangular cross section, open on the side next the spectator. As a matter of fact, in case of such an object, the right eye would see a foreshortened image of the left side, and the left eye a similar image of the right side. Consequently, the block also as viewed through the pseudoscope does indeed look like a hollow trough. Similarly, in general, convex bodies appear concave, nearer bodies farther away, etc.

[1] ¶The sides of the two isosceles right-angle prisms in WHEATSTONE's *pseudoscope* as here described were each 3 cm. The two hypothenuse faces were next each other and separated by an interval of 5 cm. For viewing distant objects these faces ought to be parallel; but they could be adjusted at a small angle with each other for viewing near objects. When the instrument is focused on an object in the centre of the field at medium distance, nearer objects will appear to be farther and bigger and farther objects will appear to be nearer and smaller, thus giving the impression of reversed relief; as stated in the text. Of course, the illusion produced will succeed much better when the object is such that a reversal of relief corresponds to an effect that is in accordance with the nature of things as we are familiar with them. (J. P. C. S.)

But still the pseudoscopic illusion succeeds only in case of a small number of objects, because it is hampered partly by familiarity with the usual forms of bodies and partly too by the shadows that are cast. It was mentioned above that the shadows invariably give absolute information as to certain geometrical relations. The bodies that cast the shadows are necessarily always in front of the shaded surfaces. When a body protrudes from a flat surface, its shadow is formed on its support. In the pseudoscope it should properly seem to lie beyond the surface as if it were indented in it. But then the shadow has no sense and interferes with the possibility of the illusion. Similarly, the illusion is hampered by having one surface partly in front of another. Then the right eye sees rather more on the right-hand side of the anterior surface than the other eye does, and this also has no sense in the pseudoscopic reversal.

The bodies that are intended to be seen by pseudoscopic vision should, therefore, generally be set up by themselves in space in front of a more distant uniformly coloured wall as background, where they will not cast any distinct shadows. The background itself should not have any distinctive features to attract the eye. Moreover, care should be taken that part of the object is not hidden in perspective by another part. Suitable objects for pseudoscopic effects are cylinders made of wood or of paper with printing or writing on it, etc., which appear like hollow bark, cigars that look like hollow wrappings of tobacco, and medals lighted from above that look like seals in hollow relief. The pseudoscopic effect is very vivid in the case of a glass tube with a scale etched on it for measuring volume of liquids. If the scale is towards the spectator, it appears in the pseudoscope to be on the opposite side of the tube. Vertical wires or cords at different distances from the observer are likewise good objects for the purpose. Those that are really closer appear to be farther, and *vice versa*.

In cases where familiarity with the actual form of the object or shadows tend to prevent the illusion, it frequently happens that a vivid idea of how the pseudoscopic form should appear will promote the production of it; and when the idea has once assumed shape, it will persist without difficulty. On the other hand, it may also be possible to recall the apperception of the original form, but when this happens, the observer is apt to be annoyed and irritated by the incongruities of the two retinal images.

In the *pseudoscope* the relief is reversed, but in the *telestereoscope* it is enhanced and higher than it would be naturally. The latter instrument, therefore, is particularly useful for bringing out more clearly the relief of very remote objects which in natural vision do not give any stereoscopic effect at all or at least very little. The human

eyes are not far enough apart to obtain two distinctly different images of very distant objects; and hence the distance between the two points of views must be artificially increased in order to get two images that are sufficiently different. The way this is done in the telestereoscope is with the help of four plane mirrors, as represented in section in a, b, α and β in Fig. 53. The two eyes of the observer are at r and ρ. The broken lines *cbar* and $\gamma\beta\alpha\rho$ indicate the routes of the light from the object to the eyes. The four mirrors are enclosed in a box, whose sides are shown in section, and mounted so as to admit of small rotations for the purpose of making the two images coincide with each other. It is sufficient to fasten the two mirrors a and α at right angles to each other and to the bottom of the box, and to turn the mirror β

Fig. 53.

by a screw around a horizontal axis and the mirror b by another screw around a vertical axis. In order to get a large field of view, the outer mirrors should be made as large as possible.

Let r_1 designate the place where the system of mirrors a and b forms the image of the eye r; and, similarly, let ρ_1 be the place of the image of the other eye ρ in the system of mirrors α and β. Then the eyes r and ρ looking in the mirrors will see the landscape in front of them as it would look if the mirrors were removed and the eyes were actually at r_1 and ρ_1. Now as these latter points are much farther apart than the actual distance between the eyes, the differences of the two images of the scene as it would look from r_1 and ρ_1 are much greater than the natural differences in the two eyes; and, consequently, the stereoscopic relief of distant objects, especially far-off mountain ranges and contours of land, is much more distinct than it is with the

naked eye. If the mirrors are adjusted so that infinitely distant objects are seen through the instrument with parallel visual axes, it will seem as if the observer were looking not at the natural landscape itself, but at a very exquisite and exact model of it, reduced in scale in the ratio of the distance between r_1 and ρ_1 to that between r and ρ.

Most stereoscopic photographs of landscape give an effect something like that of the telestereoscope, because the distance between the two stations where the pictures are taken is usually greater than the interpupillary distance. On the other hand, stereoscopic photographs can be taken of the heavenly bodies themselves at two different times when these objects present somewhat different aspects as seen from the earth. The moon is particularly suitable for this purpose. Although on the whole she turns the same side always to the earth there are little variations in her position that make it possible to obtain stereoscopic pictures by taking the photographs in two different months, at such instants when the sun's illumination was precisely the same. These stereoscopic views reveal not only the globular form of our satellite, but even the detail of relief of the mountain ranges on her surface.[1]

Even if there were no other way of estimating the *absolute distance* of a visible object, it might be possible to form a judgment of it in binocular vision by being conscious of the amount of convergence required to direct the lines of fixation to the object. But this sensation is quite unreliable and inaccurate, and sometimes we are subject to very considerable illusions in this respect.

An experiment devised by WHEATSTONE enables us to demonstrate that the judgment of the absolute distance of a visible object, and

[1] ¶Astonishingly beautiful stereoscopic views of the starry skies have been obtained. The most celebrated photographs of this kind are the several series of "Stereoskopbilder vom Sternhimmel" made by Professor MAX WOLF of Heidelberg. For example, one of these stereograms consists of two photographs of Saturn which were made on successive evenings in June 1899. Owing to the revolution around the Sun not only of Saturn but of our own planet also, the interval in space between the two places where these photographs were taken was equivalent to a base-line of about 1,075 million miles. The nearest distance between the earth and Saturn is about 783 million miles. When the stereogram is viewed in a stereoscope, Saturn and his two moons are beheld standing out from the starry dome and floating in space. Incidentally, good measurements of the distances of the planets can be obtained by measuring the stereoscopic parallaxes on such stereograms.—The fixed stars themselves are all at such prodigious distances away that even a base-line equal to the diameter of the earth's orbit would be of little avail so far as they are concerned; because if this distance is multiplied by 7000, the radius of stereoscopic vision as thus obtained would not extend much farther than about a quarter of a light-year. However, in consequence of the fact that the entire solar system is known to be "drifting" in the Milky Way, there is the possibility of making photographs of the stars and nebulae at intervals apart of a year or more, and of thereby obtaining stereoscopic views in which even these celestial objects will be lifted off of the immeasurably remote background of space. (J. P. C. S.)

consequently of its size also, is actually made by the convergence of the lines of fixation, assuming that no other circumstances intervene and interfere with it. WHEATSTONE contrived his stereoscope so that in the first place the two pictures could be moved nearer to or farther from the mirrors in the instrument. The two parallel sides (Fig. 50) where the pictures are placed could be shifted back and forth, and the two arms of the stereoscope could be turned around a fixed axis which was in between the mirrors. The nearer the pictures were to the mirrors, the larger the images on the retina will be without changing the convergence of the eyes. Under these circumstances, the object appears larger without being apparently nearer. But if the mirrors are rotated around the axis midway between them, the pictures, however, not being shifted on the two arms, the convergence of the eyes will be changed without any change being produced in the size of the image on the retina. In this case as the convergence is increased, the apparent size and distance of the object will be diminished.

Similar reduction or enlargement of the object may be noticed in any pair of stereoscopic views which are fused either with the naked eyes or in BREWSTER's stereoscope, when the pictures are brought nearer together or separated farther apart. An apparatus for making the necessary measurements in this case has been devised by H. MEYER.[1]

Direct experiments on the estimation of distance by the degree of convergence were made by WUNDT. He gazed at a black vertical thread suspended in front of a uniform white background farther away. He looked with both eyes through a sort of tubular horizontal slit at right angles to the thread, so that all he could see of the latter was its middle portion and not its ends, also without being able to see any objects to one side which might have helped him to gauge the distance. The thread was hung from a horizontal wire stretched in the observer's median plane and capable of being shifted. He tried first to estimate the absolute distance and to compare it with the length of a measuring rod held in his hand. The results are given below in centimetres.

Actual distance	Estimated distance
180	120
160	92
140	78
120	58
100	48
90	47
80	47
70	37
50	22
40	25

[1] POGG. *Ann.* LXXXV, pp. 198–207.

In every instance the estimated distance was less than the actual distance. I have made a similar set of experiments, varying the method a little and obtaining the opposite result. I held a sheet of stiff paper in the median plane right in front of the face, and gazed at a thread hanging vertically. The paper practically concealed everything on the left of the thread from the right eye, and everything on the right of it from the left eye. If I moved a pencil from the right side in towards the thread, I could see it only with the right eye, but not with both eyes. Then I tried to touch the thread with the pencil, by shoving it in quickly. Invariably, the pencil went past the thread beyond it. If I first closed my eyes, and then opened them, after having changed my position, and, directing them to the thread, tried again to touch it quickly with the pencil, the distance between the two would not be much. If I paused a little while and looked steadily at the thread, the error was invariably larger, perhaps due to increasing fatigue of the inner ocular muscles.

The perception of change of distance in Wundt's experiments, where the thread was moved nearer or farther away, was much more accurate. The least perceptible differences of distance in this case, expressed in centimetres, were found to be as follows.

Distance of thread from eye	Limits of discrimination for adjusting the thread	
	nearer to the observer	farther from the observer
180	3.5	5
170	3	4
160	3	3
150	3	3
130	2	3
110	2	2
80	2	2
70	1.5	1.5
50	1	1

At the distance of 180 cm each eye is turned inwards 61′; and when the thread is brought 3.5 cm nearer the observer, the corresponding angular displacement of each retinal image amounts to 72″. This magnitude is on the border of what can be distinguished by the eye. When the thread is not so far away as this, the angular displacements must be greater before they can be detected. For instance, at the distance of 50 cm, the necessary angular displacement is 263″.[1]

In these experiments, by the way, there may be still some doubt as to whether the two eyes have followed the thread, and the image on

[1] As to the accuracy of depth-perception by convergence, see Note 7 at end of the chapter.—K.

the retina has been kept stationary, or whether the eyes have remained fixed while the displacement of the image was noticed. In the latter case the explanation of the decrease of accuracy with increase of convergence would be that it is more difficult to keep the eyes steady when they are under the strain of convergence than when the visual axes are parallel, and there is no such strain.

The uncertainty in estimating the distance of the point of fixation is shown too by closing the eyes and holding a pencil at some distance in front of the face, and then trying to direct the eyes toward it behind the closed lids, so that, on opening the eyes without changing their position, they will be focused on the pencil. Generally, they will not be converged enough, and the pencil will be seen double when the eyes are opened. However, this experiment is apt to be more successful, as I have already stated, if the tip of the pencil has been touched and rubbed against the finger. Then we have a more distinct sensational apperception of its location, and in my own case I can usually succeed in this way in focusing my closed eyes properly so that on opening them there will be no double images.

The uncertainty in estimating the absolute amount of convergence and, consequently, the absolute distance of the object of fixation, is manifested in many instances. For example, when a card with stereoscopic pictures on it is held in the hand and the two images are fused, they generally seem to lie at the known place where the paper is, either in the plane of the paper or just a little in front of it; and yet the parallel or nearly parallel lines of fixation ought not to intersect except at a very great distance beyond this plane, and it is there that the apparent material object ought to be. Similarly, it is not easy, as a rule, to combine negative after-images of a bright object into an apperception of a material body; and they are apt to appear as being projected on the surface of that real object to which the eyes are directed. Sometimes, however, when the after-images are quite sharp and distinct, and when the real surface in front of us has no projections coming out from it, the bodily dimensions of the after-image and its independent position in space can be perceived.

Even when stereoscopic pictures are combined in the stereoscope, where there is no other visible object with which the absolute distance of the apparent figure in space might be compared, there is still considerable uncertainty about this distance. And if the attempt is made to indicate by hand the position of the apparent object outside the apparatus, the errors are similar to those that WUNDT found in estimating the distance of a thread by binocular vision. By looking first over the instrument, and then through it, alternately, the position of the hand can easily be compared with that of the stereoscopic figure

in space, and the errors can be estimated in this way. Here too my experience is like that of WUNDT's, and I find that I am apt to regard the space-image as being nearer than it really is. It is very hard to determine by feeling the position of the invisible hand; and a much better way, as a rule, is to make comparisons, first with the right eye and then with the left, with objects that are visible from where the stereoscope is situated. In BREWSTER's instrument the box usually is not too wide to prevent the observer from seeing some of the real objects whose distances and sizes are more or less familiar to him; by using his right eye for those on his right and his left eye for those on his left. Although they can only be seen with one eye at a time, and although the distance of the stereoscopic space-image is primarily a question of binocular vision, still fairly accurate determinations can be made in this way, which are not likely to be much modified by subsequent comparisons between the space-image and real objects as seen binocularly either over the stereoscope or through it.

This latter method shows that, under favourable conditions, where there are no disturbing influences, judgment of distance by convergence of the visual axes can give fairly good results. Still it is one of those elements in the judgment that may be easily outweighed by others that are opposed to it, as in the instance mentioned above, where the images were projected on a surface whose distance was known.

The influence of convergence is also shown very definitely in the case of *wall-paper patterns*.[1] Thus, on looking at a paper on which the pattern recurs regularly, and converging the eyes to a certain extent, it is possible to fuse corresponding parts of the pattern, either the first figure with the second one next to it, or the first with the third or fourth. The resultant effect will be the appearance of an image floating in air nearer the observer than it really is and also smaller, the extent of the illusion in this respect depending on the amount of convergence. If each portion of the pattern is fused in the same way with the corresponding portion next to it, the resultant figure will not be so small or so near as it would be if the first pattern were fused with the third or fourth one.

This is the place to speak of the fusion of stereoscopic pictures which are so adjusted with respect to one another that the intervals between the pairs of corresponding points are greater than the observer's interpupillary distance. Accordingly, the lines of fixation will have to be divergent in order for the two images to be fused. When a person has not had much training in making his eyes divergent, the

[1] H. MEYER in ROSER und WUNDERLICHS *Archiv*. 1842. Bd. I.—D. BREWSTER in *Phil. Mag.* XXX, 305.

best way for him to obtain this effect will be to cut in half the card with the two stereoscopic views on it, and then insert the two halves in an ordinary stereoscope and gradually separate them farther and farther apart, all the time trying to see them continually fused in one image. Another method that can be used for the same purpose is the one which was employed by ROLLET[1] and BECKER; which consisted in drawing a set of congruent stereoscopic figures on a sheet of paper, each pair of figures being under one above it, the only difference being that the intervals in the lower pair were made a little larger than they were in the preceding pair. The set of figures which they actually used were intended to produce the stereoscopic impression of a smaller circle standing out in front of a larger one. The interval between the centres of the small circles in one pair of diagrams was made just the same as that between the centres of the large circles in the preceding pair. And so if the observer had succeeded in fusing the two large circles, no further effort would be required in order to fuse the two small ones in the next pair of diagrams; and he will proceed then and try to fuse the two large circles on this row; which will enable him at the same time to fuse the two small circles on the third row, and so on. The interval between the centres of the first pair of small circles was 44 mm, and that between the last pair was 93 mm; and yet, although my interpupillary distance is only 68 mm, I am able to fuse the latter at a distance of 30 cm.

In cases of this kind the lines of fixation of the two eyes do not intersect anywhere at all in the space in front of us. Their point of intersection is really back of the head, and yet we imagine that the stereoscopic figure is somewhere in front of us, exactly as if the distance between the two images were correct. It is possible that the feeling of unusual strain on the eyes may make us aware that the eyes are in an unnatural position. And when a stereoscopic image in space as seen with divergent visual axes is compared with very remote real objects that are visible over the top of the stereoscope, such as a distant range of mountains, for example, the image in the stereoscope simply seems to us to be very much farther away than the farthest real objects.

When two prisms, each having a refracting angle of about 4°, are inserted one in front of each eye, both base in, the visual axes of the two eyes will have to diverge in order to see singly a real object which is far away; and probably, on the whole, the object will appear to be farther off than it would appear to the naked eyes, but otherwise not

[1] *Wiener Sitzunsberichte.* May 10, 1861. Bd. XLIII. See also BURCKHARDT in *Verhandl. d. naturforsch. Ges. zu Basel*, I, 145; who made even earlier experiments with divergent visual axes.

much different. In our visual apperceptions there is still something just beyond infinity. When the divergence of the eyes diminishes, it serves to inform us that the distance of the object is increased. And we continue to be guided by this sign, even when the convergence vanishes entirely and becomes negative, although then there ceases to be any real point in the space ahead corresponding to this reversed convergence. Even if our own sense should enable us to tell more or less certainly that there was something peculiar and extraordinary about vision of this kind, with the eyes adjusted as they never are in looking normally at actual things, still all we could do would be to follow the rule usually adopted in case of abnormal impressions on the senses, and compare this impression with that which is most like it and which differs from it simply because the convergence of the visual axes is not so great, namely, with the impression made on the organ of vision by very distant objects.

Owing to the uncertainty of our judgments as to the degree of convergence of the eyes, we are liable to have illusions also about the forms of things in space as seen binocularly. The interpretation of the visual phenomena would be correct if the amount of convergence were different, but it is not correct for the convergence actually used. This sort of illusion is most noticeable with objects whose retinal images are practically not modified by altering the convergence of the eyes. The following is an illustration. Hang three fine black silk threads from three pegs which are fastened several inches apart in a horizontal beam some distance above the head; the threads being stretched by weights and all three of them at first being in the same vertical plane. Then stand directly in front of them, so that the central thread lies in the median plane of your face an arm's length away, the plane of the threads being perpendicular to this plane. At some distance behind the threads there should be a background all of uniform colour without any conspicuous points on it. Now look carefully and see whether the threads really do seem to be all in one plane. It will be found that the central one apparently lies in front of the other two. This effect will be more noticeable, the closer you come to the threads. Now move the central thread back a little so that the three threads are situated on a cylindrical surface which is concave toward you; and then stand as before. From some distance away the threads will appear to lie on a concave surface; but as you approach nearer, this surface appears to become first plane and then convex; so that, finally, although the central thread is behind the other two, it will apparently be in front of them. The distance from which all three threads appear to be in one plane is quite different for different observers. Mr. E. Hering improved this experiment by substituting threads for the needles which

I had used originally; and he finds that in order to see them all in one plane, he has to stand at a distance equal to the diameter of the right circular cylinder on which the threads are placed. He connects this fact with his horopter theory, which will be discussed later. In my own case, the surface formed by the threads still looks distinctly concave at this distance; and it is the same way with Messrs. BERTHOLD, DASTICH, and BERNSTEIN, all of whom tried the experiment in my laboratory. The first two of these observers had to come nearer by about half that distance before the threads appeared to be in a plane; but I had to come even closer still, that is, to about three-tenths of that distance; and Dr. BERNSTEIN had to come nearer than that. The relative conditions were not materially altered by varying the width of the equal intervals between the threads and adjusting the central thread at corresponding distances from the plane of the other two. Dr. BERTHOLD always saw them as lying approximately in one plane when the root of his nose was about on the axis of the cylindrical surface on which the threads were situated, while I myself always had to come nearly but not quite to the middle of the radius, that is, almost as close as a quarter of the diameter of the cylinder.

An effect due to fatigue of the eyes was manifested here. Thus, in first changing from parallelism to convergence, less error was made in estimating the position of the threads, and the observer had a tendency to come closer in order to see them all in one plane. But on maintaining the convergence, the central thread appeared to approach a little, and it was necessary for the observer to retreat again.

The following are the results of some of my own observations obtained by considering the threads for a long time; the distances being given in millimetres.

Distance between two outer threads	Distance of the central thread from the plane of the other two	Diameter of the cylinder	Distance from which I saw the threads in one plane	Distance expressed as a fraction of the diameter
256	10.5	1571	450	0.286
256	6	3737	730	0.267
117	4.2	819	237	0.289
117	8.1	429	129	0.301
120	2	1802	550	0.305

The reason for errors of judgment in these experiments, as was stated above, is that in estimating distance simply by the convergence of the visual axes, it is usually underestimated, and the estimate is uncertain anyhow.

In looking at a vertical plane divided by parallel vertical lines, the sections over on the right will subtend a larger angle in the right eye than they do in the left eye, first, because they are nearer the right eye, and, second, because the visual axis of that eye meets those sections more nearly at a right angle than the visual axis of the other eye. Similarly, the sections over on the left will appear wider to the left eye than to the right eye. The nearer the eyes are to the observed plane, the larger the difference will be between these two angles for any particular strip. Now in order to be able to tell whether the differences perceived in this sort of projection belong to a plane or to a curved surface, we should have to be able to estimate the distance of an object very accurately by means of the convergence of the visual axes. For equal differences between the images on the two sides might be shown on a convex surface which was farther from the observer or on a concave surface which was nearer to him. I am disposed to think that the reason for interpreting these particular binocular images as belonging to an object too far away is not because, or at least not entirely because, we generally overestimate distances under these conditions; as is shown by the experiments previously described, where we tried to point a pencil as seen by one eye at a cord as seen by both eyes. For, indeed, the error as to the distance would have to be larger than it really proves to be, on the supposition that it must produce an equivalent alteration in the apparent form of the perspective image. Thus in the first set of observations in the table above the distance would have to be 627 mm instead of 450 mm; and in the third line 350 mm instead of 237 mm. I have never found errors as large as this. I am inclined to think that an error is made in interpreting the images, because another circumstance that usually aids the interpretation is absent in this case. If the lines in the field of view are not simply uniform straight lines in similar situation, as the threads were in the last experiment, but lines with plainly marked points on them, or objects containing horizontal borders also, the vertical lengths lying nearer the right eye will subtend a larger angle in the right than in the left eye, and *vice versa.*

The importance of the differences in the vertical dimensions in the images in the two eyes is shown very clearly by comparing the stereoscopic patterns *A* and *B* on Plate I. The pair of figures *A* represents the two projections of a plane with a checkerboard pattern on it which is close to the observer; and, when fused, they appear to lie in one plane. But the pair of figures *B* show the two projections of a distant cylindrical surface with the same sort of pattern on it; and that is the way it looks when the two figures are fused. The point to be noted is that the intervals between the vertical lines in the two sets of

drawings are exactly equal. Hence, if the apparent curvature depended simply on the mutual positions of the vertical lines, as has been assumed by most previous writers,[1] the two pairs of drawings ought to show the same curvature of surface. But the relative position of the vertical lines corresponds *per se* to a flat checkerboard near at hand just as well as it does to a distant one bent convex to the observer; and the distinction between the two is due entirely to the path of the cross lines. On the other hand, in Fig. *C*, Plate I, the vertical lines have all been drawn equidistant from each other, but the cross lines have been curved so that they are farther apart at the outer edge than they are in the interior, as would be the case with the images of a near concave surface. The stereoscopic combination of the two actually produces this effect, in spite of the fact that the lines of fixation are parallel, as is not the case in looking at a near object. If we were to judge here simply by the difference of spacing of the vertical lines, then since there are no differences, *C* ought to look like a flat checkerboard. The inappropriate convergence does not disturb the effect here any more than it did in Fig. *A*, where we were led to suppose that we were viewing a flat surface close by, although the requisite convergence was lacking. The resemblance of the two drawings in *A* to a flat checkerboard near the observer determines the interpretation in spite of the feeling that the convergence is not appropriate.

If pictures are selected with no differences whatever for the two eyes in the vertical dimensions, that is, if, as in the experiment with the three vertical threads described above, these dimensions are absolutely uniform without any conspicuous points, then one set of data will be lacking by which we are in the habit of telling whether the images are near. The differences which indicate the horizontal distances between the threads in the two retinal images are not accompanied by the customary vertical differences, or at least the latter are not noticeable. And since convergence alone is not a very reliable method of judging how near an object is, we decide that the three threads belong to an object which is somewhat farther off than they really are, and for which the actual differences in the horizontal dimensions could not occur unless the object were convex toward the observer.

The reliability of a judgment of distance depending on convergence of the eyes being so very different for different individuals, it is easy to see how the measure of the illusion in the case of the three vertical threads might differ widely with different people. The illusion appears to have been developed most in the case of Mr. E. Hering; but in his

[1] Mr. E. Hering, especially, has announced this to be a fundamental law of binocular vision.

case the judgment of distance by convergence of the visual axes must also be specially poor, for from his own observations he is disposed to believe that convergence has nothing to do with it at all.

In order to test the explanation which I have given above, I fastened little gilt beads on each of the three black threads at intervals apart of about 4 cm, to be used as points of reference that could plainly be seen even in indirect vision. Thereupon the illusion described disappeared almost entirely. Instead of having to move the central thread back 10.5 mm so as to make all three of them appear to lie in one plane, I had to move it only 2 mm after the beads were attached. In both cases the outer threads were 256 mm apart and 450 mm away from me. When the outer threads were 120 mm apart, and the central one moved back 2 mm, I had to retire to a distance of 550 mm away before they appeared to be all in one plane; but after I attached the beads, a distance of 230 mm was sufficient.

If the three black threads are brought near any object at all containing a sufficient number of conspicuous points, the curvature of the surface on which the threads lie can be perceived, even when there are no straight lines at all on the object that can be used for comparison. For instance, a body which I happened to use was a paper-cutter, carved out and shaped something like the letter S, and the illusion as to the positions of the threads disappeared almost entirely, even when the edge that was curved most was turned toward them.

Fig. 54.

As it is very difficult to draw the vertical lines in stereoscopic diagrams with sufficient accuracy except by machinery, I have made some other experiments on the influence of convergence by producing two images from a single drawing by means of prisms. Two right-angle prisms were mounted side by side near each other with their edges parallel, as shown in section in Fig. 54; the face of one prism making a small angle α with the adjacent face of the other prism. If the ray af enters the first prism at b nearly normally, it will be reflected at c and again at d, as shown in the diagram, and will finally emerge at the last surface in the direction eg. The total deviation of the ray will be equal to twice the angle α.[1] When the prisms are mounted in this way with their

[1] No distortion of the image due to refraction at the glass surfaces need be feared here,

edges vertical and an object is observed through them, exactly the same image is formed on the retina as if the object were viewed with the naked eye; only, in order to see it, the eye has to be turned a little more to one side or the other than would be necessary without the prism.

When three vertical threads all lying on one plane are observed by the naked eyes, the central one, as we have seen, will appear to lie in front of the others. If the same observation is made by looking through the two prisms, the retinal images will be the same as before, but the eyes will be more convergent or more divergent than they were at first, according as it is the face *b* or the face *e* that is turned toward the threads. In case the divergence is increased, the central thread appears to stand out farther than before; whereas if the convergence is increased, it apparently retires to the plane of the other two or even back beyond it. Since the prism combination has a very slight telestereoscopic action, the surface *e* should be placed in front of the right eye for convergence, and the surface *b* in front of the right eye for divergence. Or the two surfaces may be placed successively in front of the left eye. The telestereoscopic action of the little instrument will be the same in the first two cases, where the effective interpupillary distance has been increased by the prisms; and it will also be the same in the last two cases, where it has been decreased.

This experiment shows that identical retinal images can produce the idea of a concave, plane, or convex surface, depending on whether the convergence of the eyes has been increased or decreased; and this would seem to imply that with objects of this kind convergence is taken into account.[1]

On the other hand, when a plane surface covered with plainly visible figures or letters is observed through the prism combination, it will look flat no matter whether the convergence is increased or diminished, because in this case the retinal images can correspond to only one actual object, and the apperception of this object will be produced even when there is an incorrect convergence. The case is similar with the threads on which the beads were strung. There also the effect of increased convergence or divergence is very unimportant, and the observer notices chiefly the telestereoscopic action of the virtual increase in interpupillary distance.

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such as occurs in oblique-angle prisms and may be very annoying in stereoscopic experiments; because the changes that are produced are merely of the same kind as occur in looking perpendicularly through a thick plate of glass with plane parallel faces, that is, they are vanishingly small in the centre of the image and symmetrical out toward the sides, and hence cannot be disturbing in the experiments which are described here.

[1] Concerning this question, see Note 8 at the end of the chapter.—K.

Ordinary weak prisms act entirely differently. If one of them is adjusted in front of the eye, base out, in the position of minimum deviation, all objects will be shifted inwards, requiring therefore increased convergence in order to observe them. But at the same time all vertical lines will appear to be concave toward the nasal side, temporal portions of the picture being too narrow and nasal portions too wide, whereas horizontal lines will appear to diverge toward the nasal side. The consequence is that in looking at objects binocularly, with a prism of this sort in front of the right eye, they seem to be nearer, but distorted, so that both horizontal and vertical straight lines are apparently concave toward the observer. The differences in the natural projection, whereby the parts of the object lying on the nasal side of the median plane appear to be reduced, are partly or entirely annulled, when the prism is used, by the apparent magnification of vertical distances on that side. The object appears to be at about the same distance as before, or perhaps, in spite of the increased convergence, rather larger and farther away. Under these circumstances the broadening of the nasal parts of the image and the narrowing of the temporal parts can be interpreted only as implying a concave curvature of the body. The curvature of the vertical lines causes the apparent concavity. When the prism is placed in front of the eye, base in, then, on the contrary, plane objects will appear convex toward the observer.[1]

Closely related to the preceding phenomena, in which binocular images of objects were observed either with increased or reduced convergence of the eyes, there is the possibility of constructing a *model* or *relief*, in which all the depths are so reduced as compared with the original that when viewed at a closer distance, it will create the same impression as the latter as to form, dimensions and shading, and not merely as it looks to one eye but as it looks to both eyes; which is achieved by constructing the model in such manner that it produces practically the same differences between the retinal images as would be obtained by looking at the original. It is just for this reason that a relief viewed from the proper standpoint is a very much more perfect means of reproducing an object, at least so far as its form is concerned, than the best plane picture ever can be. This is true not only of the low reliefs and high reliefs of sculpture representing human heads, figures

[1] ¶See also E. M. Eaton, Factors in stereoscopic vision and in the visual estimation of distance. *Brit. J. of Ophth.*, 3 (1920), 63-68.—Idem, Visual perception of solid form. Ibid., 3, 349-368 and 399-408.—L. Bard, Du grossissement réalisé par la vision binoculaire et son rôle dans la perception du relief. *Arch. d' ophtalm.*, 35 (1921), 513-523.—F. P. Fischer, Stereoskopie im starken indirekten Sehen. Pflügers *Arch.*, 204 (1924), 247-260. (J. P. C. S.)

and groups of figures, but also of theatrical properties, by which landscapes, rooms, portals of churches or any perspectively reduced corridors are represented.

The rules governing constructions of relief, as they have been found empirically by artists,[1] can be deduced from a simple stereoscopic experiment. Take a pair of stereoscopic drawings mounted on separate cards, and adjust them at first so that when they are fused, with the proper convergence of the eyes, they will combine to produce exactly the same view as the original. Then move the two pictures toward each other, keeping them in the same plane. This will increase the convergence of the visual axes of the eyes, without materially altering the retinal images except possibly for some slight variations; and so, aside from the comparatively vague realization of increase of convergence, almost the same sensory impression persists as was there at first. Now imagine that the object was constructed which for this new position of the pictures would correspond to them; then this object will be a relief copy of the original. In the relief there is a *principal plane (the plane of the background)*, in which all the infinitely distant points of the original will lie; and there is also a so-called *plane of congruence*, parallel to the principal plane, containing all the points of the original that coincide with their own counterparts in the relief. If the relief is intended to show the original in its natural size, this plane of congruence must pass through the eyes of the spectator. But if the relief is to give the impression of a diminished or enlarged model of the original, the plane of congruence will be in a different position, and will not contain the *point of view*, which is the point midway between the spectator's eyes.

All planes and all straight lines in the original will be reproduced by planes and straight lines, respectively, in the model.

All planes and all straight lines that are parallel to the plane of congruence in the original will continue parallel to this plane and to each other in the model.

All other parallel planes in the original will intersect in a straight line in the principal plane of the model.

All parallel straight lines in the original, except those parallel to the plane of congruence, will intersect in a point in the principal plane of the model.

All planes and straight lines passing through the point of view will maintain their position in the model.

And, lastly, if f denotes the distance of a point in the original from the plane of congruence, φ the distance of the corresponding point in

[1] J. A. Breysig, *Versuch einer Erläuterung der Reliefperspektive.* Magdeburg 1798.

the model from this plane, and g the distance between the principal plane and the plane of congruence, then

$$\frac{1}{\varphi} - \frac{1}{f} = \frac{1}{g} .$$

This equation, which enables us to find φ, is identical in form with the equation which would give the distance of the image in a concave lens of focal length$(-g)$.

Images of distant points lie very close together in the relief, while those of nearer points are separated by relatively much greater distances, just as is the case with the images in a divergent lens. Such a lens, therefore, shows a properly constructed *relief copy* of objects observed through it.

When the plane of congruence and the principal plane are made to coincide, the relief passes over into a plane picture in correct perspective.

Depth-differences in the original that are perceived with the same ease will be represented by equal depths in the relief. And in this sense we may say that the objective world is perceived binocularly as in a relief. Equal intervals of considerable magnitude along the line of sight may be scarcely perceptible when they are very remote from the observer, and yet even small differences of depth in the foreground can be easily distinguished.

Finally, I must discuss certain errors, which are made in estimating the directions of lines in binocular vision, and to which attention has been called by E. HERING. Suppose we are looking at a long vertical cord suspended in front of a uniform wall some distance farther away, where there are no conspicuous points or lines that might enable the observer to locate the vertical or horizontal directions. And suppose too that the cord itself is so long that its two ends are not in the field of view; or what amounts to the same thing, suppose that the cord is observed through a hollow cylinder as wide as the face and long enough to hide the ends of the cord as well as objects that happen to be on either side of it. We can make a binocular estimate as to whether the cord is truly vertical or not, and if it does not look vertical, we can try to make it so by adjusting its lower end. The results I get in this case, which are in agreement with what HERING[1] finds, indicate that a cord which is really vertical will appear to be vertical, provided that, when the head is in the chosen position, the horizontal plane of sight is in its primary position, and the cord is in the median plane. But if,

[1] *Beiträge zur Physiologie.* Heft V. p. 297.

while the cord stays in this plane, the head is tilted backward so as to make the plane of sight come below its primary position, the lower end of the cord must be pulled farther from the observer in order to make it appear vertical. Conversely, when the head is tilted forward, causing the plane of sight to be above its primary position, the lower end of the cord must be pulled toward the observer.

If the cord does not lie in the median plane, but to the right of it, it will again appear to be vertical when it is really vertical, provided the head is erect and the horizontal visual plane is in its primary position. But if the head is lowered, the lower end of the cord must again be moved nearer to the observer. In order to locate approximately the plane in which the cord has to be moved, in order for it to appear vertical, I attached another cord loosely to the lower end of the first one, by which I could pull it toward me; and when it looked vertical, I cast my eye down at the horizontal cord; which would cause me to see the vertical cord in very divergent double images. Ordinarily, the horizontal cord bisected the angle between these two images, indicating that, within the limits of error of the experiment, the cord that seemed to be vertical must be very nearly at least in the plane that bisects the angle of convergence of the visual axes of the two eyes.

When my head was tilted back, I had to move the lower end of the cord away from me, but the direction of the horizontal cord, as nearly as I could tell, was the same as before.

The explanation of these phenomena appears to me to be related to a fact mentioned in the previous chapter (p. 258), namely, that estimates of the direction and position of objects observed with the eyes convergent are made as if the (cyclopean) eye were looking in a direction parallel to the mean direction of vision, and had the proper torsional-rotation for this purpose. In this case the actual convergence of the eyes was not taken into account. Applying this explanation to the present case, we should say, that *in order for lines to appear perpendicular to the visual plane, their images must lie in those meridians of the eye that are truly perpendicular to the visual plane when the position of the eye is parallel to the line of vision of the so-called cyclopean eye.*

When the point of fixation is in the median plane, the line of vision of the cyclopean eye will lie in this plane too, and eyes which obey Listing's law will require no rotation about their longitudinal axes. Hence the meridian in each eye which is perpendicular to the visual plane in the primary position will also be perpendicular to this plane when it is inclined, provided the directions of the eyes continue to be parallel to the mean direction of vision, that is, parallel to the median plane. But if the eyes are converged, and the visual plane is below the horizontal, the eyes will turn until their two median planes which

were previously vertical will converge upward toward each other. The case is just opposite when the visual plane is elevated above the horizontal. The line of intersection of these two meridian planes is the line that is apparently perpendicular to the visual plane. Its upper end in the first case, and its lower end in the second case, will be toward the observer.

However, in looking off to one side, either up or down, the meridians of the eye that are normal to the visual plane are not the same as they were in the primary position. Even the images of the apparently vertical cord are not in the vertical meridians of the two eyes in the primary position; as may be easily seen by setting up a vertical strip against a wall directly in front of the eyes and observing the vivid after images. Occasionally, these latter will make very large angles with the apparently vertical cord, the moment we look steadily at it. Thus, here this cord appears to be in those meridians which would be vertical when the eyes were looking in the direction parallel to the mean direction of vision.[1]

It must be mentioned, however, that according to VOLKMANN's experiments, which I myself have verified, the meridians that appear to be normal to the retinal horizon appear also to be absolutely vertical when the observation is made with one eye and there is no torsional rotation. In binocular vision, on the other hand, the apparent normal must correspond to both of the meridians that are absolutely perpendicular to the visual plane. Thus in binocular vision the opposing effects in the two eyes due to the inclinations of the apparently vertical meridians tend to counteract each other when we try to judge the position of the vertical. There is no difficulty in understanding how this happens in the case of inclinations to the right or left; but it should be remarked that the deviation of the apparently vertical meridian has nothing to do with our judgment as to whether the observed line is inclined toward us or away from us. In the next chapter we shall see that this deviation probably originated in the apperception of horizontal lines, and that will explain then why it does not deceive us about vertical lines.

A similar mistake in regard to the depth-dimension is apt to be made not only in the case of lines that pass through the point of

[1] Mr. E. HERING has tried to connect these phenomena with the theory of the horopter, which will be discussed in the next chapter. I may state that, so far as my own observation goes, the lines that appear to be perpendicular to the visual plane *never* do lie on the horopter, but are seen always in crossed double images. Since in the case of Mr. HERING's eyes there is no difference, or at least a very slight difference, between the meridians that are really perpendicular to the retinal horizon and those that are apparently so, the rule he gives is specifically correct for his eye, at least for the median positions he is considering.

fixation and lie in the median plane, but also in the case of lines that pass through this same point in other directions and are only approximately perpendicular to the mean direction of vision. The apparent position of such lines is given by the law stated above; they are interpreted as if the same retinal images had been obtained with the eyes in a position parallel to the mean direction of vision.

In this connection, Recklinghausen has shown that if a star composed of a number of lines meeting in a point is drawn on a flat surface, and if the eyes are turned upward and focused steadily on this central point, the rays of the star above the horizontal will appear to lie on a concave cone, and those below the horizontal on a convex cone. Opposite curvatures will be observed on gazing at the centre of the star with downcast eyes. I find that the illusion becomes more pronounced when the rays near the horizontal position are omitted, and when fine smooth wires stuck in a cork, lying all in one plane and diverging from a point, are used instead of lines drawn on paper.

According to the theory that has just been developed, these lines must all appear to lie in a conical surface of the second degree whose apex is at the point of fixation. Moreover, it contains both lines of fixation, and its intersection with the plane through the centres of the two eyes perpendicular to the visual plane will be an ellipse whose vertical axis is rather longer than its horizontal axis.

Recklinghausen made experiments also to find out the positions of those lines that appear normal to the mean direction of vision when the eyes are raised or lowered. For this purpose he used a fine smooth wire which could be adjusted in the middle by a little joint so as to give it a different inclination to the mean direction of vision (which is the line bisecting the angle of convergence). The joint which supported the wire was attached to a round iron rod, which was in the prolongation of the mean direction of vision and which could be turned around its long axis. Thus the plane in which the wire moved could be adjusted at different angles to the visual plane, and for each position of this plane the position of the wire could be found for which its upper and lower extremities appeared to be equally far from the observer.

For these positions of the wire the theory requires that the surface shall be also a cone of the second degree, passing through the point of fixation and the two lines of fixation. Recklinghausen's measurements were found to agree very satisfactorily with this theoretical result. He called this conical surface the *normal surface* because it contains all the apparent normals to the mean direction of vision.

For eyes in which there is no deviation of the apparent vertical meridian this normal surface would coincide with the horopter surface (the theory of which will be given in the next chapter) for lines going

through the point of fixation. But the two surfaces are not identical in the case of eyes for which the apparent vertical meridians are not the same as the true vertical meridians,[1] as will be shown in the next chapter.

If a system of concentric circles is drawn on a piece of paper and if then the eyes are converged on the centre of the figure, the plane of fixation being oblique, these circles likewise will undergo a small apparent rotation about their horizontal axis, in the same direction as the rotation of vertical lines, but not to the same extent. Now draw a vertical diameter of the system of circles, and it will be rotated more than the circles themselves, and so will apparently be separated from them. If the plane of fixation is elevated, the upper end of the diameter seems to be nearer the spectator, and the lower end farther away, than the plane of the circles. It is just the reverse when the plane of fixation is lowered.

Since the horizontal portions of the circles do not produce any definite binocular apperception, sometimes they will appear to be bent at an angle, as if they were trying to cling to the diameter.

This experiment too succeeds better when the figure consisting of circles and diameter is made with very fine wires. The illusion obtained then necessitates the observer's not being able to tell by the figure that his eyes have undergone a rotation. Usually on a sheet of paper there are enough noteworthy points to enable the observer to recognize that he has two images of the same object before him and that they have been rotated in opposite ways. The only objects that are suitable for these experiments are such as will admit of a real interpretation, even in case their retinal images have been slightly rotated. We saw above that a similar relation was needed in order to tell by certain peculiarities of the image that the eyes were convergent.[2]

Laws of Stereoscopic Projection

The plane of the adjoining diagram (Fig. 55) is supposed to represent the visual plane; the centres of the two eyes being at the points designated by P and Q. The straight line AB represents the section of a stereoscopic drawing in a plane which is perpendicular to the visual plane and also to the median plane of the head; corresponding to the usual position in which stereoscopic pictures are observed.

[1] Recklinghausen himself did not make this distinction, for although he discovered the deviation of the apparent vertical meridian, he did not know its effect on the position of the identical places in the images.

[2] Much recent work has had to do with the conditions for estimating distances and absolute dimensions by the eye. As to these investigations, see Note 9 at the end of the chapter.—K.

Let CD be the line of intersection of the median plane with the visual plane. The point designated by S is some point in space which is to be reproduced in the stereoscopic picture. If this point does not happen to be in the visual plane, then S designates the foot of the perpendicular let fall from it on to this plane. To find the projections of the point S in the two pictures, draw the straight lines SP and SQ meeting AB in R and T, respectively. Then the point R will be the representative of S for the eye at P, and the point T will represent the same point for the other eye at Q. The positions of these points may be given with reference to a system of rectangular coördinates whose origin is at the point O, which is the point of intersection of the three mutually perpendicular planes, namely, the visual plane, the median plane and the plane of the stereoscopic projection. Let us suppose that OA is the positive direction of the x-axis and OD that of the z-axis; the y-axis being taken perpendicular to the plane of the paper. Thus, the coördinates of the point P will be $x = +a$, $y = 0$, $z = -b$; those of the point Q, $x = -a$, $y = 0$, $z = -b$; those of the point S, $x = \alpha$, $y = \beta$, $z = \gamma$; those of the point R, $x = \xi_0$, $y = v_0$, $z = 0$; and those of the point T, $x = \xi_1$, $y = v_1$, $z = 0$. Then the conditions that the three points P, R and S shall all be in one straight line are:

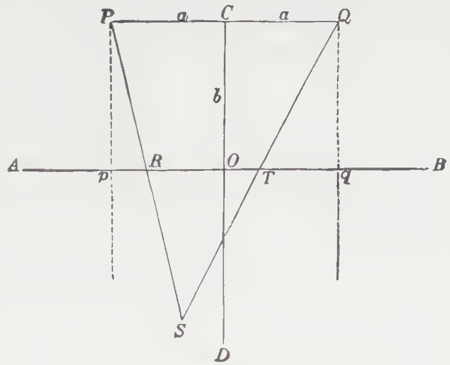

Fig. 55.

$$\frac{\alpha - a}{\alpha - \xi_0} = \frac{\beta}{\beta - v_0} = \frac{\gamma + b}{\gamma} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Similarly, if the points Q, T and S are all in a straight line, then

$$\frac{\alpha + a}{\alpha - \xi_1} = \frac{\beta}{\beta - v'} = \frac{\gamma + b}{\gamma} \quad . \quad . \quad . \quad . \quad . \quad (2$$

Evidently, therefore, we must have:

$$v = v_1 = \frac{\beta b}{\gamma + b}, \quad . \quad . \quad . \quad . \quad . \quad (1a)$$

which implies simply that the heights of corresponding points in both pictures above the horizontal line AB must be the same. Moreover,

$$\xi_0 = a - \frac{\gamma(a-\alpha)}{\gamma+b} = \frac{ab+\gamma\alpha}{b+\gamma},$$

$$\xi_1 = a - \frac{\gamma(a+\alpha)}{\gamma+b} = \frac{ab-\gamma\alpha}{b+\gamma},$$

and the difference between these two abscissae is:

$$\epsilon = \xi_0 - \xi_1 = \frac{2\gamma a}{b+\gamma}. \qquad \text{(1b)}$$

This difference being independent of the values of both α and β will be the same therefore for all points of the object that happen to lie in a plane parallel to the plane of the pictures. It is a measure of the amount by which the points in one of the pictures have been shifted to one side or the other with respect to the corresponding points in the other picture; the supposition being that the two pictures are so adjusted with reference to each other that points considered as being themselves in the plane of the pictures will be fused together; as is the case, for instance, with the lines that form the frames of the pictures. But in many cases it is better to adjust the two pictures so that two points representing an infinitely distant point will be fused; as, for example, the points p and q in the diagram where the lines of fixation Pp and Qq that are parallel to CD meet AB. If we put $\gamma = \infty$ in equation (1b), then

$$\epsilon_\infty = 2a,$$

and if

$$e = \epsilon_\infty - \epsilon$$

and

$$b + \gamma = \rho,$$

we obtain:

$$e = \frac{2ab}{\rho} \qquad \text{(1c)}$$

Now in this equation $2a$ denotes the distance between the centres of the eyes, b denotes the distance of the plane of the stereoscopic pictures, and ρ denotes the distance between the object and a plane through the centres of the two eyes perpendicular to the visual plane.[1] For all

[1] ¶ Equation (1c) may also be derived very simply from the similarity of the triangles PQS and RTS in Fig. 55. Since the altitudes of these triangles are in the same ratio as the corresponding sides, evidently,

$$\frac{\rho-b}{\rho} = \frac{RT}{PQ} = \frac{2a-e}{2a} \quad \text{or} \quad \frac{b}{\rho} = \frac{e}{2a};$$

real points in front of the eyes, the magnitude denoted by e will always be positive, because $2a$, b and ρ are always positive. Thus in the picture intended for the right eye any nearer point will lie more to the left than in the picture intended for the other eye. At the same time equation (1c) shows that the *stereoscopic difference* (e) will be very small when the distance ρ is very great, and that it does not amount to much until this distance gets to be small.

The fact that the magnitude denoted by e has the same value for objects which were all in one plane parallel to the plane of the pictures, was utilized by O. N. Rood[1] in the construction of an instrument for making a pair of corresponding stereoscopic pictures by copying them from separate perspective drawings of any object. The original, made transparent with oil, is fastened on a horizontal glass plate and illuminated from beneath. A flat rectangular frame is placed on it, and a sheet of drawing paper inserted underneath it. This frame can be shifted slightly to one side or the other by means of a thumb screw. At first a drawing is made without changing the position of the frame at all. In the second drawing the lines in the extreme foreground are copied first, then those a little farther away, etc.; but whenever a more distant set of lines is about to be drawn, the frame holding the copy is shifted a trifle, depending on the depth-difference. In this way two drawings may be obtained which will show relief when they are combined stereoscopically.

If two points at different distances $\rho_{,}$ and $\rho_{,,}$ are projected stereoscopically, the corresponding stereoscopic differences being denoted by $e_{,}$ and $e_{,,}$, then

$$e_{,}-e_{,,}=2ab\left(\frac{1}{\rho_{,}}-\frac{1}{\rho_{,,}}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2a)$$

Now suppose $(e_{,}-e_{,,})$ here is the smallest distance on the drawing that can be perceived; then the corresponding values of the distances $\rho_{,}$ and $\rho_{,,}$ will be the distances of the ends of the interval that can just be distinguished. By way of abbreviation, put

$$\frac{2ab}{e_{,}-e_{,,}}=f.$$

---

that is,

$$e=\frac{2ab}{\rho}.$$

The greatest value of the stereoscopic difference is $e=2a$, in which case $RT=0$, $\rho=b$, and the point S will lie in the plane AB. On the other hand, the least value ($e=0$) will be obtained when $RT=2a$, $\rho=\infty$, and PS and QS are parallel lines. (J. P. C. S.)

[1] *American Journal of Science and Arts.* Vol. XXXI. p. 71. Jan. 1861.

Then equation (2a) can be written:

$$\frac{1}{f}=\frac{1}{\rho_{,}}-\frac{1}{\rho_{,,}}\,.$$

which is the formula previously given for this case. If r denotes the geometric mean between $\rho_{,}$ and $\rho_{,,}$ the last formula may also be written:

$$\rho_{,,}-\rho_{,}=\frac{r^2}{f}\,.$$

That is, *the depth-intervals that can be stereoscopically perceived increase in proportion to the square of the mean distance* (r).[1]

In order to obtain an idea of the variations of the stereoscopic relief produced by shifting the two pictures with respect to each other, the coördinates α, β, γ of the apparent point P of the object must be expressed in terms of the coördinates ξ_0, ξ_1, η of its two images. From equations (1) and (2) above, we have:

$$\frac{a-\alpha}{\alpha-\xi_0}=\frac{a+\alpha}{\alpha-\xi_1}\,.$$

---

[1] ¶According to the formula $\frac{1}{f}=\frac{1}{\rho_{,}}-\frac{1}{\rho_{,,}}\,,$

we obtain:

$$\rho_{,,}-\rho_{,}=\frac{\rho_{,}^2}{f-\rho_{,}}\,,\ (\rho_{,,}>\rho_{,}).$$

For example, suppose that the radius of stereoscopic vision is $f=450$ m: how far must one point be beyond another given point before we can discern that it is farther? If the distance of the nearer point is 200 m, the distance of the farther point will have to be at least 160 m more; or if the distance of the nearer point is 400 m, that of the farther point will have to be at least 3600 m. Finally, if the nearer point is 450 m away, the farther point will have to be infinitely far away.

Or, again, how much nearer must an object be than another given object in order to be just able to tell that it is nearer?

In this case $$\frac{1}{f}=\frac{1}{\rho_{,,}}-\frac{1}{\rho_{,}}\,,$$

and hence $$\rho_{,}-\rho_{,,}=\frac{\rho_{,}^2}{f+\rho_{,}}\,,\ (\rho_{,,}<\rho_{,}).$$

Thus, for $f=450$ m and $\rho_{,}=400$ m, we find $\rho_{,}-\rho_{,,}=153$ m; that is, an object must be 153 m nearer the eye than one 400 m away, before we can discern that it is nearer. If an object was a little over 2 metres away, a second object would have to be about a centimetre closer in order to see that it was closer. (J. P. C. S.)

Hence,

$$\alpha = \frac{a(\xi_1+\xi_0)}{2a+\xi_1-\xi_0},$$

$$\beta = \frac{2va}{2a+\xi_1-\xi_0},$$

$$\gamma = \frac{b(\xi_0-\xi_1)}{2a+\xi_1-\xi_0};$$

or if, as before, we substitute here the stereoscopic difference

$$2a+\xi_1-\xi_0=e$$

and let x denote the geometric mean between ξ_0 and ξ_1, then

$$\left.\begin{aligned} \alpha &= \mathrm{x}\frac{2a}{e} \\ \beta &= v\frac{2a}{e} \\ \rho = \gamma + b &= b\frac{2a}{e} \end{aligned}\right\} \qquad \text{(3a)}$$

If both stereoscopic pictures are shifted equally to one side, that is, if x is increased without changing e, v and b, the values of α will be increased without changing β and ρ. But α will increase $2a/e$ times as much as x does. Eliminating the stereoscopic difference (e) from the first and third of equations (3a), we obtain:

$$\alpha = \rho \cdot \frac{\mathrm{x}}{b}.$$

Accordingly, the increase of α is proportional to the apparent distance of the point P. In other words, points which for the original position of the stereogram were apparently one directly behind the other, so that the values of x were the same for them both, after the stereogram has been shifted, will lie in a straight line going through the point midway between the two eyes.

If the stereogram is moved away from the eyes, thereby increasing the value of b, without changing the values of x, v, e and a, α and β will remain constant, but the distance ρ will increase in the same proportion as b. Indeed, this can easily be observed by fusing a pair of stereoscopic pictures with the visual axes of the eyes parallel; the farther the stereogram is from the eyes, the more pronounced the relief will be.

Lastly, in order to discuss the changes that occur when the two pictures are shifted towards each other or farther apart, let us write equations (3a) in the following form:

$$\left.\begin{aligned} \frac{a}{\rho} &= \frac{\mathrm{x}}{b} \\ \frac{\beta}{\rho} &= \frac{v}{b} \\ \frac{1}{\rho} &= \frac{e}{2ab} \end{aligned}\right\} \quad \ldots \ldots \ldots \ldots \quad (3b)$$

in which it should be noted that $2\mathrm{x} = \xi_0 + \xi_1$ and $e = 2a + \xi_1 - \xi_0$. Now if the two pictures are shifted in opposite directions, the one on the right to the left and the one on the left to the right, each through a distance η, the result will be to decrease ξ_0 and increase ξ_1 by this same amount, and, consequently, the value of e will be increased by 2η, while both x and v remain the same. If the values of a, β and ρ after shifting the pictures in this way are denoted by a_1, β_1 and ρ_1 then equations (3b) become

$$\frac{a_1}{\rho_1} = \frac{\mathrm{x}}{b}, \qquad \frac{\beta_1}{\rho_1} = \frac{v}{b}$$
$$\frac{1}{\rho_1} = \frac{e+2\eta}{2ab}.$$

And so, finally, after substituting the values of x, v and e as given by equations (3b), we obtain:

$$\left.\begin{aligned} \frac{a_1}{\rho_1} = \frac{a}{\rho}, \qquad \frac{\beta_1}{\rho_1} = \frac{\beta}{\rho} \\ \frac{1}{\rho_1} = \frac{1}{\rho} + \frac{\eta}{ab} \end{aligned}\right\} \quad \ldots \ldots \ldots \quad (4)$$

Here a, β, ρ denote the original coördinates of the given point of the object with reference to a system of axes whose origin lies at a point midway between the centres of the two eyes (the so-called *point of view*) and a_1, β_1, ρ_1 denote the corresponding coördinates of the apparent position which this point will have when the two correct stereoscopic projections have been shifted equally toward each other. The position of the image of any point of the object after the pictures have been shifted in this manner may be found by equations (4). The first two equations amount to saying that the true and apparent positions of the point are both in the straight line drawn through the

point of view. The third equation shows that its distance from the vertical plane passing through the two eyes has been changed, this distance being diminished when the value of η is positive. Putting $\frac{ab}{\eta} = p$, we may write this last equation as follows:

$$\frac{1}{\rho_1} = \frac{1}{\rho} + \frac{1}{p}, \quad \ldots \ldots \quad (4a)$$

which is seen to be identical in form with the formula for a concave lens of focal length p, where ρ denotes the distance of the object and ρ_1 that of the corresponding image. When the point is infinitely far away, then $\rho = \infty$ and $\rho_1 = p$. Thus p denotes the distance of the plane on which all the infinitely distant points of the original object will be represented as lying, the so-called *principal plane* as Breysig has termed it.

If α, β, ρ are the coördinates of any point in some definite plane, that is, if they are connected by an equation of the form

$$A\alpha + B\beta + C\rho + D = 0 , \quad \ldots \quad (5)$$

then, according to equations (4) and (4a):

$$A\alpha_1 + B\beta_1 + \left(C - \frac{D}{p}\right)\rho_1 + D = 0 \quad \ldots \quad (5a)$$

Hence, the corresponding image-points will also be all in one plane; and if $A = B = 0$, that is, if the given plane of the original object is parallel to the vertical plane ($\rho = 0$) passing through both eyes, then the image-plane will be parallel to this plane too and therefore parallel to the plane in the original object. On the other hand, if $D = 0$, that is, if the given plane in the original object passes through the origin of coördinates or the *point of view*, the image-plane will be absolutely coincident with the original plane.

Consider a set of parallel planes in the original, given by an equation of the same form as equation (5), where D denotes the parameter which varies from one plane to another. If we put $\rho_1 = p$ in equation (5a), which represents the image-planes, this equation will become:

$$A\alpha_1 + B\beta_1 + Cp = 0 , \quad \ldots \ldots \quad (4c)$$

which is independent of D. In other words, the reproductions of this set of parallel planes will all intersect the principal plane in a straight line whose equation is given by (4c).

Thus the images of a set of parallel planes in the original either do not intersect each other or the principal plane at all, or they all intersect each other and the principal plane in a single straight line, the

so-called *vanishing line* (*Fluchtlinie*). Since, as has been noted, the particular member of any family of parallel planes that passes through the point of view will coincide with its own image-plane, this plane also must intersect the principal plane in the vanishing line. Hence, in order to find the vanishing line of a set of parallel planes, all we have to do is to pass a plane parallel to them through the point of view, and it will intersect the principal plane in the required line.

Moreover, if equations (4) are written as follows:

$$a_1 - a + \frac{a\rho_1}{p} = 0\,, \qquad \beta_1 - \beta + \frac{\beta\rho_1}{p} = 0\,,$$

$$\rho_1 = \frac{\rho a b}{ab + \rho\eta}\,,$$

then for $\rho = 0$, we find:

$$\rho_1 = \rho = 0\,, \quad a_1 = a\,, \quad \beta_1 = \beta\,,$$

which shows that for every point in the plane $\rho = 0$ the image is the same as the original. This plane (BREYSIG's "*Bildebene*") will be called here the *plane of congruence*. Accordingly, the image of any plane A in the original can be constructed by passing a plane through A's vanishing line and the line in which A intersects the plane of congruence.

Every straight line in the original is to be regarded as the line of intersection of a pair of planes; and, similarly, its image must be the line of intersection of the copies of two planes, that is, it must be a straight line too. A bundle of parallel lines may be regarded as the system of lines of intersection of two sets of parallel planes. The images of these planes must intersect the principal plane in the corresponding vanishing lines; and the lines of intersections of these image-planes, or the images of the parallel lines in the original, will necessarily go through the point of intersection of the two vanishing lines. The only exception is that of a set of parallel lines which is parallel to the principal plane and the plane of congruence, as the vanishing lines in this case will not intersect. Reproductions of parallel straight lines that are not parallel to the principal plane will meet this plane therefore in a single point, the so-called *vanishing point* (*Fluchtpunkte*).

This vanishing point for a straight line in the original which is not parallel to the principal plane may be located by drawing a straight line parallel to the given line through the point of view, which will meet the principal plane in the required point. The image of a straight line in the original may be found by drawing a straight line through its vanishing point and the point where the given line crosses the *plane of congruence*.

Evidently, these are precisely the same rules as those given previously for the construction of reliefs, with the one exception that in the case of reliefs the plane in which the points were coincident with their images does not necessarily pass through the eyes themselves. This latter condition does not have to be fulfilled unless the object is to be represented by the relief in its natural size.

Thus, suppose that the coördinates of all the points in the original are increased or reduced in the same ratio; which means simply substituting $n\alpha$, $n\beta$, $n\rho$ in equations (4) in place of α, β, ρ, respectively; then these equations will become:

$$\left.\begin{array}{c} \dfrac{\alpha_1}{\rho_1}=\dfrac{\alpha}{\rho}\,, \qquad \dfrac{\beta_1}{\rho_1}=\dfrac{\beta}{\rho}\,, \\ \dfrac{1}{\rho_1}=\dfrac{1}{n\rho}+\dfrac{1}{p}\,. \end{array}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; (6)$$

When ρ is infinite, then $\rho_1=p$, that is, the plane $\rho_1=p$ will be the *principal plane* containing the projections of all infinitely distant points of the original.

If in the original there is a plane

$$A\alpha+B\beta+C\rho+D=0\,, \quad . \; . \; . \; . \; . \; . \; . \; (5)$$

its projection as obtained by equations (6) will be:

$$A\alpha_1+B\beta_1+\left(C-D\frac{n}{p}\right)\rho_1+Dn=0 \quad . \; . \; . \; . \; . \; (5b)$$

And if $D=0$, the second of these equations will be the same as the first, and the original plane will coincide with its projection. This condition is satisfied by planes passing through the point $\alpha=\beta=\rho=0$, and hence this point has the same significance as the *point of view*.

And, finally, the planes (5) and (5b) intersect where

$$\left.\begin{array}{l} D=Dn-Dn\dfrac{\rho_1}{p}\,, \\ \text{or} \\ \rho_1=p\cdot\dfrac{n-1}{n}\,. \end{array}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; (5c)$$

The plane given by this equation does *not* contain the point of view, and hence it is the plane of congruence. If, therefore, the relief has been constructed according to the usual rules, and the point of view does not lie in the plane of congruence, then when it is viewed from the proper point, it will represent a reduced or enlarged model of the original, in which the point of view of the observer has kept its relative

position. Under these conditions the angle subtended at the eye by the model will be the same as that subtended by the original. If the plane of congruence lies betwen the observer and the model, the relief will appear larger than the original; whereas if this plane lies behind the observer, it will appear smaller.

When the plane of congruence and the principal plane are exceedingly near together ($n = \infty$), the model becomes ultimately a plane perspective drawing.

The modifications that apparently take place when two correct stereoscopic drawings of an object are shifted toward each other in their own plane, or separated farther apart, are thus seen to be of the same character as those that can be produced by the construction of models of the object. The effect may readily be observed in stereoscopic pictures by moving them as described; and in this way a person can easily obtain the correct apperception of depth which he wishes to have. However, it should be stated that, so long as the objects are familiar ones, the correct apperceptions of depth are apt to be formed, because we are not very sensitive as to the absolute amount of convergence of the visual axes of our eyes, and for that very reason, in the absence of other points of comparison, our judgments are apt to be formed as if the lines of fixation were converged at the proper angle to give a correct apperception of the depth of the object.

We ought to add here that when stereoscopic pictures are shifted in this way, the convergence of the visual axes is not the only factor that is altered. The actual view of the pictures is altered also, because if we continue to look steadily at the same point, supposing the visual axes were normal to the surface of the pictures before, they will not remain so after the pictures have been shifted; the result being that the projection of the image on the retina will be a little different. Still it can readily be seen that, if we tried to turn the pictures themselves so as to keep the same image on the retina, many of the straight lines drawn to corresponding points of the pictures would no longer intersect, and so there would be no real point in space that would correspond to such a pair of points in the pictures. How the pictures are projected under these conditions, cannot be explained until we take up the theory of the horopter in the next chapter.

When a stereogram is viewed through a pair of convex or concave lenses, one close in front of each eye, the interval between their centres being the same as the observer's interpupillary distance, the magnitudes denoted by c, x and r in equations (3a) will all be increased in the same ratio as the apparent distance of the image (b); and consequently the magnitudes α, β and ρ will remain the same. And so

the apparent position and dimensions of the stereoscopic relief will not be modified at all by using these lenses. It is well to keep this in mind so far as wearing spectacles is concerned. If the latter are properly adjusted, the apparent size of the resultant stereoscopic image will not be affected at all, in spite of the fact that the image for each eye is actually enlarged or reduced by the lenses.

However, if the sizes and distances of objects are to be shown correctly, it is essential that the interval between the optical centres of the glasses shall be exactly the same as the distance between the centres of rotation of the two eyes. In Fig. 56 suppose that a_0 is the optical centre of a concave spectacle lens, and that the object is at b; then if the straight line a_0f_0 represents the optical axis of the lens, the image of b in the lens will be at the point β_0 on the straight line a_0b. Draw bf_0 and $\beta_0\varphi_0$ perpendicular to the optical axis. If the focal length is denoted by p, and if we put

$$a_0f_0=r\ , \qquad a_0\varphi_0=s\ ,$$

then (see Vol. I, p. 89):

$$\frac{1}{r}-\frac{1}{s}=-\frac{1}{p}\ ;$$

which enables us to find the position of β_0. Now suppose the lens is shifted at right angles to its axis until the optical centre is at the point a_1 and its axis is in the line a_1f_1; then the image of the point b will be at β_1 where the straight line a_1b crosses the perpendicular line $\varphi_0\beta_0$. Hence, the shift of the image will be:

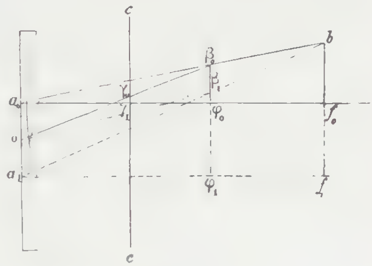

Fig. 56.

$$\beta_0\beta_1=a_0a_1\left(\frac{\varphi_0f_0}{a_0f_0}\right)=a\cdot\frac{r-s}{r}\ ,$$

where the displacement of the lens a_0a_1 is denoted by a. Thus by means of the lens formula above, we derive:

$$\beta_0\beta_1=a\frac{s}{p}=a\frac{r}{r+p}\ .$$

Now suppose that there is an eye at o just behind the glass looking at the images β_0 and β_1 and projecting them on a plane cc in the points γ_0 and γ_1. If the distance of this plane from a_0 is denoted by A, then

$$\gamma_0\gamma_1=\beta_0\beta_1\cdot\frac{A}{s}=\frac{aA}{p}\ ,$$

and hence the apparent shift of the projection of the image in the lens on the plane *cc* will be independent of the position of the object b. Thus as the lens is shifted from a_0 to a_1, the displacement of the image in it is just the same as if a perspective drawing of the object on the plane *cc* had been shifted by an amount equal to $\gamma_0\gamma_1$. Suppose the plane *cc* on which the image is projected is the focal plane of the lens, that is, suppose $A = p$; then $\gamma_0\gamma_1 = a = a_0a_1$, and then the shift of the image will be just the same as that of the lens.

The phenomena that occur when spectacle lenses are decentered laterally with respect to the eyes are, therefore, identical with those that occur when two stereoscopic pictures are shifted toward or away from each other. This theoretical conclusion is verified completely by experiment. When the interval between the centres of concave spectacle lenses is less than the interpupillary distance, objects appear too close; and in the opposite case, they appear too far. It is just the reverse with convex spectacle glasses, because then p has the opposite sign.

In making spectacles this effect should be kept in mind,[1] especially too because headaches and pains in the eye may be easily produced by keeping the eye strained for a long time. When the optical centres of a pair of spectacles are not far enough apart, they force the eyes to converge continuously; and, on the contrary, when they are too far apart, they make the eyes divergent. It is worst of all when the centre of one lens is higher than that of the other. The *pince-nez* is especially apt to be faulty in construction. If the optical centres of the lenses are at their geometrical centres, they will be too near together, thus making the eyes converge. And, besides, eye-glasses clamped on the nose are almost never in a horizontal line; and that is apt to cause variations of level.

In looking at real objects through a pair of parallel telescopes or binocular field glasses, the same effect is obtained as when stereoscopic pictures are viewed from a nearer distance; that is, the angles subtended at the eye by all the parts of the picture will be uniformly enlarged. Now, as we saw above, this is equivalent to bringing the object closer and reducing its depth-dimensions without altering the dimensions at right angles to the line of sight. Accordingly, objects seen through binocular glasses are made to appear nearer, neither smaller nor larger, but flattened as in low relief. This is especially noticeable in the case of human faces, which always assume an unnatural aspect, almost as though they were pictures.

[1] The stereoscopic effects caused by spectacle lenses have been examined in detail by F. C. DONDERS in his work on *Anomalies of accommodation and refraction*, London, 1864. pp. 152–169.

The *theory of the telestereoscope* (p. 310) is very simple, provided we reflect that, except for the symmetrical reversal of right left, the images in a plane mirror are seen just as the observer's own image would see the actual objects themselves through a hole where the mirror is.

In Fig. 57 the two parallel plane mirrors are represented by AA and BB. The observer with his eye at C sees objects reflected in the first mirror BB just as the image of his eye at D would see them through BB; the distances Cb and Db being equal, of course. Similarly, an eye at D would see objects reflected in the second mirror AA just as an eye at E, which is the image of D in this mirror, would see them through AA. Here too, of course, the distances Ea and Da are equal. Accordingly, as above stated, the pair of mirrors enables the observer to place his eye at C and thus to view the landscape just as if his eye were really at E.

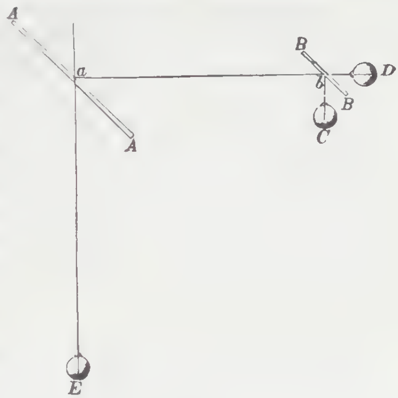

Fig. 57.

Now, according to equation (1c), the stereoscopic difference (e) between the projections of two images of the same object on a plane whose distance from the eyes is denoted by b will be

$$e = \frac{2Ab}{r},$$

where $2A$ denotes the distance between the two points of view, and r denotes the distance of the object from the vertical plane passed through the centres of the two eyes. In the case of the telestereoscope this interval $2A$ is the distance between the images of the observer's eyes in the two pairs of mirrors, that is, it is the distance $r_1\rho_1$ in Fig. 53. Now if this value of e is substituted in equations (3a), then for infinitely distant objects as seen with the visual axes parallel we shall have:

$$\alpha = x\frac{a\ r}{A\ b} = x\frac{\rho}{b},$$

$$\beta = v\frac{a\ r}{A\ b} = v\frac{\rho}{b},$$

$$\rho = l\frac{a\ r}{A\ b}.$$

Accordingly, therefore, the magnitudes denoted by α, β, ρ are related to each other in the same way as x, v, b respectively; where the latter magnitudes may be regarded as the real distances, although the apparent distance ρ is less than r in the ratio of a to A, and hence the other apparent dimensions will be reduced in the same ratio also. Thus the landscape will appear in this case as a correctly executed reduced model.

The same thing is true also in the case of stereoscopic photographs of landscapes, provided the interval denoted by $2A$ is considered as being the distance between the two positions of the centre of the photographic lens with which the pictures were taken. In adjusting the stereoscope, care should be taken to see that infinitely distant points in the photographs are fused with the visual axes of the eyes parallel, and that the distance of the stereogram from the eyes or from the lenses of the stereoscope is the same as the interval was between the sensitive plate and the camera lens. Unless these conditions are satisfied, a false relief will be obtained. Ordinarily, these two requirements are not fulfilled with the stereoscopes and stereograms purchased in the shops.

RECKLINGHAUSEN'S *Normal Surface*. Consider a system of rectangular coördinates with its origin at the point of fixation, the xy-plane being in the visual plane, and the zx-plane in the median plane of the body. Let

$$x=a\,, \qquad y=b\,, \qquad z=0$$

denote the coördinates of the right eye, and

$$x=a\,, \qquad y=-b\,, \qquad z=0$$

those of the left eye. Thus the interpupillary distance will be equal to $2b$, and the distance of the point of fixation from the line joining the centres of the eyes will be equal to a.

The equations of the line of fixation of the right eye will be:

$$\frac{x}{a}-\frac{y}{b}=0 \quad \text{and} \quad z=0 \; . \; . \; . \; . \; . \; . \; . \; . \qquad (1)$$

and those of the line of fixation of the left eye will be:

$$\frac{x}{a}+\frac{y}{b}=0 \quad \text{and} \quad z=0 \; . \; . \; . \; . \; . \; . \; . \; . \qquad (1a)$$

Adding equations (1), after multiplying the first one by a certain factor p, we obtain:

$$p\left(\frac{x}{a}-\frac{y}{b}\right)+z=0 \; . \; . \; . \; . \; . \; . \; . \; . \qquad (1b)$$

This is the equation of a plane passing through the line of fixation of the right eye, since equations (1) from which this equation was derived are satisfied by the coördinates of any point on this line. If the angle between the normal to this plane and the z-axis (or the angle between this plane itself and the visual plane $z=0$) is denoted by α, then

$$\cos\alpha = \frac{1}{\sqrt{1+\dfrac{p^2}{a^2}+\dfrac{p^2}{b^2}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1c)$$

Similarly, from equations (1a) we obtain:

$$-p\left(\frac{x}{a}+\frac{y}{b}\right)+z=0\,, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1d)$$

which is the equation of a plane passing through the line of fixation of the left eye; and the value of $\cos\alpha$ for this plane is the same as that given by formula (1c). From the latter formula we derive the following value of p:

$$p = \frac{\tan\alpha}{\sqrt{\dfrac{1}{a^2}+\dfrac{1}{b^2}}}\,.$$

Or, if γ is one-half the angle of convergence, and if r denotes the distance of each eye from the point of fixation, then

$$a=r\cos\gamma\,, \qquad b=r\sin\gamma\,,$$

and

$$p=r\tan\alpha\sin\gamma\cos\gamma\;;$$

hence equations (1b) and (1d) become:

$$(x\sin\gamma-y\cos\gamma)\tan\alpha+z=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1b)$$

$$-(x\sin\gamma+y\cos\gamma)\tan\alpha+z=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1d)$$

Subtracting the second from the first, we obtain:

$$x\sin\gamma=0,$$

and therefore the line of intersection of the two planes (1b) and (1d) lies in the plane $x=0$, which is perpendicular to the visual plane and the median plane and passes through the point of fixation, no matter what value the angle α may have. Suppose now that this line is a line in the object which is being observed; then the planes (1b) and (1d) contain the direction rays [Vol. I, p. 97].

On the supposition that thus far the eyes have had no rolling motion, we can impose now a rotation of this sort by assuming that the

angle in equation (1b) is increased by an amount δ, whereas the angle α in equation (1d) is decreased by this same amount. Then the new positions of the two planes will be

$$\tan(\alpha+\delta) = \frac{z}{y\cos\gamma - x\sin\gamma},$$

$$\tan(\alpha-\delta) = \frac{z}{y\cos\gamma + x\sin\gamma}.$$

Now the tangent of the difference of these two angles is given by the following expression:

$$\tan 2\delta = \frac{2zx\sin\gamma}{y^2\cos^2\gamma - x^2\sin^2\gamma + z^2}$$

or

$$z^2 + y^2\cos^2\gamma - x^2\sin^2\gamma - 2zx\sin\gamma \cdot \cot 2\delta = 0 \quad . \quad . \quad . \quad . \quad (2)$$

If this equation is satisfied by the coördinates x, y, z, obviously it will be satisfied also by the coördinates nx, ny, nz; and, consequently, any straight line drawn from the origin to a point on the surface represented by equation (2) will lie on this surface; in other words, equation (2) is the equation of a cone with its vertex at the origin. Moreover, the same values of x, y, and z that satisfy equations (1) and (1a) will also satisfy (2); and hence the conical surface passes through the lines of fixation of the two eyes.

According to the fundamental principles established above, the images seen when the point of fixation lies in the median plane may be explained without assuming any rolling motion of the eyes. And, therefore, the pencil of rays which was in the plane $x=0$ before this rotation cannot be distinguished from the rays on the surface of the cone represented by equation (2). In other words, the bundle of rays will appear plane or conical, according as the retinal horizons coincide with the visual plane when the eyes are in the first position or the second position, respectively.

Here it should be noted that those elements of the cone which happen to be very close to the lines of fixation, and which therefore would necessarily seem to be directed toward the eyes of the observer himself, produce too sharp a relief to be likely to be right; and hence it is better to avoid them. And, moreover, those elements of the cone that come in between the two eyes belong to images on the two retinas that are exactly opposite in direction; and hence they should not be considered.

In order to find the apparent position of circles whose centres correspond to the points of fixation and whose planes are perpendicular to the bisector of the angle of convergence, we may utilize the fact that when the equation of a plane is given in the normal form

$$U=ax+by+cz+d\ ,$$

where

$$a^2+b^2+c^2=1\ ,$$

the magnitude denoted by U is the distance of the point (x, y, z) from the plane $U=0$, and d is the distance of the origin from this plane. Putting equation (1b) in the form

$$x \sin\gamma \sin\alpha - y \cos\gamma \sin\alpha + z \cos\alpha = U\ , \quad \ldots \quad (3)$$

let us consider another plane which also goes through the line of fixation, but which is perpendicular to the first plane, so that for it the value of α is $(\alpha+90°)$. Its equation, therefore, will be

$$x \sin\gamma \cos\alpha - y \cos\gamma \cos\alpha - z \sin\alpha = V \quad \ldots \quad (3a)$$

And, finally, let

$$x \cos\gamma + y \sin\gamma - r = W \quad \ldots \quad (3b)$$

be the equation of a third plane perpendicular to the line of fixation. Then U, V, W will be the coördinates of the point (x, y, z) referred to this system of three mutually rectangular planes, and

$$\frac{1}{m^2}U^2+\frac{1}{n^2}V^2=W^2 \quad \ldots \quad (3c)$$

will be the equation of a cone of the second degree whose vertex is at the centre of the right eye, and whose three principal axes will be formed by the lines of intersection of the planes

$$U=0\ , \qquad V=0\ , \qquad W=0\ .$$

This cone will intersect the plane $x=0$ in a curve whose equation is:

$$y^2\cos^2\gamma\left\{\frac{\sin^2\alpha}{m^2}+\frac{\cos^2\alpha}{n^2}\right\}+z^2\left\{\frac{\cos^2\alpha}{m^2}+\frac{\sin^2\alpha}{n^2}\right\}$$
$$+2yz\cos\gamma\cos\alpha\sin\alpha\left(\frac{1}{n^2}-\frac{1}{m^2}\right)$$
$$=y^2\sin^2\gamma-2ry\sin\gamma+r^2\ .$$

Now in order that this curve shall be a circle when the rolling motion of the eye is such that $\alpha=0$, we must have:

$$\frac{\cos^2\gamma}{n^2}-\sin^2\gamma=\frac{1}{m^2} \quad \ldots \quad (3d)$$

For symmetrical positions of the other eye, γ and α must both be taken negative at the same time. Thus putting

$$\begin{aligned} x \sin\gamma \sin\alpha + y \cos\gamma \sin\alpha + z \cos\alpha &= U' , \\ -x \sin\gamma \cos\alpha - y \cos\gamma \cos\alpha + z \sin\alpha &= V' , \\ x \cos\gamma \quad - y \sin\gamma \quad - r &= W' , \end{aligned}$$

we obtain:

$$\frac{1}{m^2} U'^2 + \frac{1}{n^2} V'^2 = W'^2 \; ; \qquad \ldots \ldots . (3e)$$

which is the equation of a corresponding cone whose axis is the line of fixation of the left eye, whose vertex is at the centre of this eye, and which for $\alpha = 0$ intersects the plane $x = 0$ and any plane parallel to it in a circle, as was the case with the cone given by the equation (3c).

If the position of the eyes for which $\alpha = 0$ involves a rolling motion, and if the curve in which the two cones intersect each other is a circle objectively present, then by the rules given above, the retinal image will be interpreted as if these retinal images had been obtained without such a rotation. Thus the apparent object will be the intersection of cones (3c) and (3e). Subtracting one of these equations from the other, we have left only those terms with opposite signs in the two equations, namely:

$$\begin{aligned} &-\frac{1}{m^2} y \cos\gamma \sin\alpha (x \sin\gamma \sin\alpha + z \cos\alpha) \\ &-\frac{1}{n^2} y \cos\gamma \cos\alpha (x \sin\gamma \cos\alpha - z \sin\alpha) \\ &= y \sin\gamma (x \cos\gamma - r) . \end{aligned}$$

This equation will be satisfied either when

$$y = 0$$

or when

$$x \sin\gamma \cos\gamma \left[\frac{\sin^2\alpha}{m^2} + \frac{\cos^2\alpha}{n^2} + 1\right] + z \cos\gamma \cos\alpha \sin\alpha \left[\frac{1}{m^2} - \frac{1}{n^2}\right] = r \sin\gamma .$$

Thus the first line of intersection would be in the median plane, and cannot be easily represented as an object. Taking account of equation (3d) we may write the equation of the plane containing the other line of intersection as follows:

$$x(1 - \sin^2\alpha \sin^2\gamma) - z \sin\gamma \sin\alpha \cos\alpha = \frac{rn^2}{(n^2+1)\cos\gamma} \quad . \quad . \; (3f)$$

For the case when $\alpha=0$, this equation becomes:

$$x = \frac{rn^2}{(n^2+1)\cos\gamma} = x_0 \, .$$

Hence the curve in which the two cones cut each other in this case will be a circle in a plane parallel to the plane $x=0$ and at a distance from it equal to x_0. When α is not equal to zero, the plane of the curve will be inclined at an angle η to the plane $x=0$, such that

$$\tan\eta = \frac{\sin\gamma\sin\alpha\cos\alpha}{1-\sin^2\gamma\sin^2\alpha} \, .$$

This plane intersects the visual plane $z=0$ in the line

$$x = \frac{x_0}{1-\sin^2\alpha\sin^2\gamma} \, ,$$

that is, a little farther from the eye than before. In this case the curve will be an ellipse.

The nearly vertical axial planes of the two cones $V=0$ and $V'=0$ intersect in the straight line whose equations are

$$\left.\begin{aligned} x\sin\gamma &= y\tan\alpha \\ y&=0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

For $\alpha=0$, the equations of this line become

$$x=0 \, , \quad z=0 \, .$$

Accordingly, when the two eyes are rolled through an angle α, a line perpendicular to the visual plane will appear to be inclined at an angle η' to the plane $x=0$, such that

$$\tan\eta' = \frac{\sin\alpha}{\cos\alpha\sin\gamma} \, .$$

Now when α and γ are both small, as is always the case in actual tests, we have

$$\tan\ \eta' > \tan\ \eta \, .$$

Thus the vertical diameter of the circle appears to be more inclined to the plane $x=0$ than does the plane of the circle, and that is why it seems to come out from the circle, as Recklinghausen noticed. Since the horizontal portions of the circle give only a very indefinite binocular localization, the circle may also appear bent where it is intersected by the diameter, without being separated from it.

When an ellipse is viewed instead of a circle, equation (3d) will not occur, and then it is found that an ellipse with its longer axis vertical will have to be inclined in the same way as a vertical straight

line has to be inclined, the narrower the ellipse the greater being the straight line inclination. On the other hand, if the longer axis of the ellipse is horizontal, it has to be inclined the opposite way, the amount of inclination again depending on how narrow the ellipse is.

Helmholtz's *Modification of* Brewster's *Stereoscope.*—In the ordinary photographic stereograms the distance between corresponding points is not always equal to the interpupillary distance, and sometimes too these points are at different levels above the base line. Consequently, there should be some device for adjusting the instrument for each picture so as to obtain as natural a projection of the object as possible. In a stereoscope which I got Oertling in Berlin to make for me, this adjustment was accomplished very simply by mounting each of two prismatic lenses in a cylindrical tube which could be turned around its axis. Thus the refracting edges of the prisms could be turned more inwards or outwards, so as to cause the eyes to converge more or less, and also to correct for differences of level. The same result was accomplished in another way by means of the instrument shown in perspective in Fig. 58, and in section in Fig. 59, which is four-tenths the actual size. Not only is the adjustment simpler in this case, but the irregularities of refraction in prismatic lenses are reduced as much as possible. A special advantage of this type of instrument is that it enables us to use higher magnifications than can be obtained with ordinary stereoscopes and therefore to obtain a still more natural impression. But it must be mentioned that such additional magnification is hardly ever feasible except with glass transparencies. The box is similar to that of Brewster's stereoscope with prismatic lenses. The stereogram is inserted through the slit in a direction parallel to the ground glass plate AA. The spectator looks through the two tubes B_0B_1 which contain only centered convex lenses, not prisms.[1] There are two lenses in each tube, the one next the eye having a focal length of 12 cm, and the other one a focal length of 18 cm. When the ordinary magnification is sufficient, the latter lens can be removed, but then the landscape views are apt to appear smaller than the actual object would appear as seen from the same position by the naked eye. By turning the screw C_0 the tube B_0 can be moved up and down (supposing that the spectator is gazing down in the instrument); and by turning the screw C_1 the tube B_1 can be moved sideways horizontally, toward B_0 or away from it. The details of this mechanism are shown in Fig. 58, where it can be seen how C_1 acts directly on the tube-carriage, whereas C_2 operates by means of a bent lever.

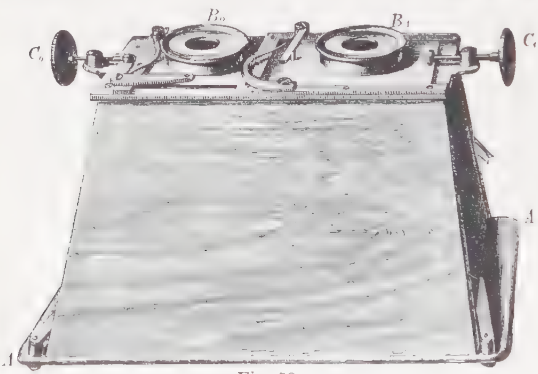

Fig. 58.

[1] Mr. Claudet too has noticed (*Proc. Roy. Soc.* VIII, 104–110) that more correct and truer pictures of landscapes can be obtained by fusing the images made by lenses with the visual axes parallel.

The way I generally make the adjustments is to pull out the tubes first until the stereogram is in the focal plane of the combination of lenses. It is easy to tell when this is the case by looking at the ground glass plate and focusing the image of some distant bright object on it. If the observer happens to be near-sighted, I think it is better for him to wear his ordinary glasses in getting this focus. Two advantages are obtained by focusing the image in this way in the focal plane of the system of lenses. In the first place, the stereoscopic image continues to look like an infinitely distant object even when the head is moved in front of the glasses; and in the second place, the fusion of the two images will not be disturbed even if the head is inclined to one side. In particular, if the observer is asked to look through a rigidly mounted stereoscope, he will obtain, as far as shape is concerned, the same optical impression in every respect as if he were looking at the distant real object itself. The next thing to be done is to correct the positions of the two images by means of the screws C_0 and C_1. By converging my eyes slightly, I produce a double image of some prominent bright object, and note whether the two components are on the same level with each other. If not, I adjust the screw C_0 until this is the case. The focusing in the focal plane can be made still more accurately by tilting the head first to one side and then to the other. To make the convergence approximately right, I move my head back a little away from the lenses and look over the top of the stereoscope at actual objects and compare their distances from me with the apparent distances of objects seen in the stereoscope. The proper correction can then easily be made by turning the screw C_1.

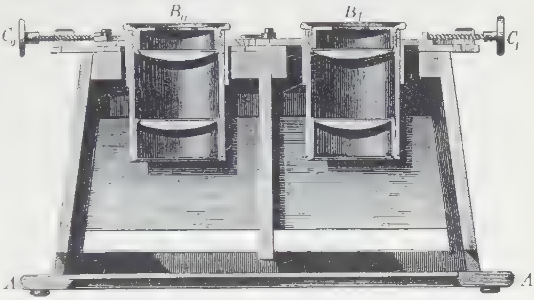

Fig. 59.

When this instrument is properly adjusted, objects seen through it will appear not only much larger and much farther but also more substantial than they do in an ordinary form of stereoscope, which almost always requires too much convergence and so makes the objects appear in low relief. Another very great advantage in this improved apparatus is the complete absence of fatigue and smarting of the eyes which are so apt to occur otherwise.

Without using more or less complicated contrivances, such as WHEATSTONE's *mirror stereoscope*, or BREWSTER's *lens stereoscope* in some of its various forms, or the *pseudoscope* (which likewise enables us to fuse a pair of pictures), stereoscopic effects may also be produced with only a picture and a prism.[1] Thus if the picture represents an object which is formed symmetrically with respect to the observer's median plane as seen by his right eye, then the corresponding view for the left eye would be symmetrical with it, or congruent with the image of it in a plane mirror. Thus instead of having a second drawing, all that is necessary is to have an actual image of the first one in a plane mirror, which can be obtained simply by looking through a right-angle prism in a direction parallel to the hypothenuse face; because, as has been repeatedly stated, the observer will see an image of the object produced by total reflection in this face. Meanwhile the right eye will be gazing directly

[1] DOVE, POGG. *Ann.* LXXXIII. 183. Also, *Berliner Monatsberichte.* 1850. p. 152.—BREWSTER, *Phil. Mag.* (4) III. 16–26. Also, *Rep. of Brit. Assoc.* 1849, 2, p. 5.

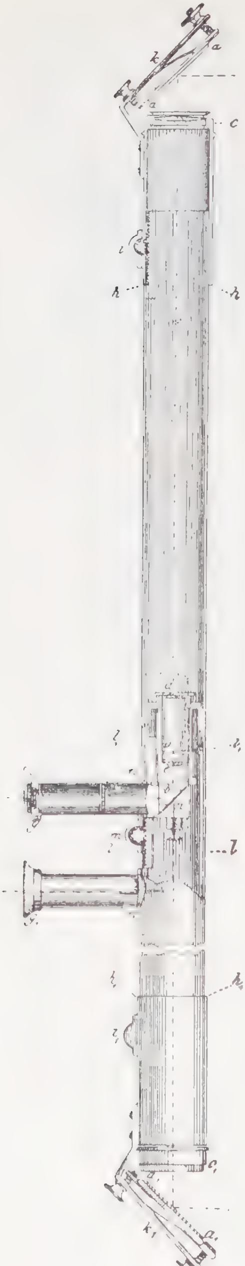

Fig. 60.

at the drawing itself. The impression of stereoscopic relief is obtained by fusing these two images. By putting the prism in front of the left eye, the relief will be reversed. In this way pictures can often be made to give stereoscopic effects which were not intended to do so; as, for example, in the case of the photograph of a person which was taken with a camera pointed almost at right angles to his face.

Dove[1] obtained similar stereoscopic effects by looking at a suitable drawing through two telescopes of equal magnifying power, one of which was an astronomical telescope and the other a Dutch telescope. The picture is inverted by the former but not by the latter. The same pictures can be used for this purpose as in the case of the simple prism stereoscope; only it is necessary that the upper half of the body to be represented shall be symmetrical with its lower half.

The simple form of *telestereoscope* in which there is no magnification has been described above. I have had a similar instrument constructed with two telescopes whereby distant objects can be seen stereoscopically in their bodily form. The optical part of the instrument is represented in Fig. 60. The light coming from the objects falls first on the two plane mirrors aa and a_1a_1. These mirrors, however, must be as nearly perfect as possible; otherwise, all the irregularities in the images in the mirrors will be magnified by the telescopes. The mirrors are fastened to the plates k and k' by three screws, and there are springs inserted between the plates and the mirrors separating them as much as the screws will allow. By means of these screws the mirrors can be adjusted until the two images coincide. The object-glasses of the two telescopes are at c and c_1. They are inserted in tubes, and can be focused by means of rack-and-pinions h, i and h_1, i_1 so as to regulate the focal planes. The two lenses of a terrestrial ocular are at d and e. After passing through the telescope the light falls on the prism b, by which it is reflected into the eye-tube where the two other lenses (g) of the ocular are inserted. The prism is mounted on a metal block (p) which can be moved along the telescope tube by means of a micrometer screw in order to align the optical axes of the two parts of the telescope. And, finally, a rack-and-pinion at m enables us to adjust the two telescopes with respect to each other so that the distance between the centres of the ocular tubes is the same as the observer's interpupillary distance.

The interval between the two is 108 cm, that is, it is 16 times as much as the ordinary interpupillary distance, so that the stereoscopic differences will be 16 times as great as for the unaided eyes. As the magnifying power is also 16, the

[1] Pogg. *Ann.* LXXX, 446.—*Berliner Monatsberichte*, 1850. p. 152.

effect of using the instrument is equivalent to diminishing the distance of the object from the naked eyes to one-sixteenth of the actual distance.[1]

According to a statement made by Oppel,[2] an effect just opposite to that obtained with the telestereoscope will be produced by adjusting two congruent bodies both turned the same way at a distance apart equal to the interpupillary distance and then viewing them with the visual axes parallel.

Stereoscopic Microscope.[3] A recent model of Nachet's stereoscopic microscope is represented in Fig. 61. After passing through the objective at a, half of the light goes past the small reflecting prism at b and through the tube E, where it emerges through the ocular e into one of the observer's eyes. The other half of the bundle of rays enters the prism, which is almost a right-angle prism, and is reflected from its hypothenuse face to a second prism (c), where it is again reflected so as to traverse the tube F and enter the observer's other eye through the ocular at f. This entire tube with the prism at c can be adjusted by the screw at g, so as to accommodate the instrument to the observer's interpupillary distance. This adjustment must be accurately made because the beams issuing from the oculars are very narrow. In English instruments of this kind, the two tubes are rigidly connected, and the adjustment for interpupillary distance is made by drawing out the ocular tubes more or less.

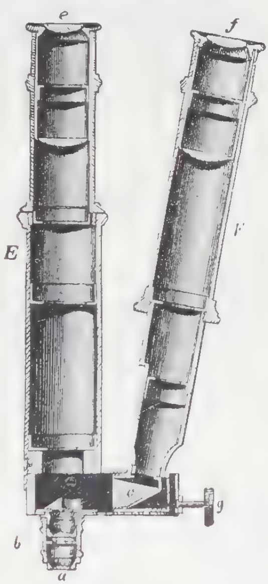

Fig. 61.

The stereoscopic effect with an instrument of this sort is very impressive, and is an immense help in studying objects of complicated shape. The means employed are entirely different from those in other stereoscopic instruments. Thus in this case we do not have two pictures of the object taken from different standpoints, because the images for the two eyes are formed by a single objective, half of the rays going to one eye and half of them to the other eye. The only reason why there is stereoscopic effect here is because a punctual image is formed only by those

[1] ¶In a so-called telestereoscope (or in any similar instrument such as a pair of binocular field glasses) the "plastic" effect (P) is measured by the product of the magnifying power (M) and the ratio of the distance (a) between the centres of the two object-glasses to the interpupillary distance (b); that is,

$$P = M\,\frac{a}{b}$$

Thus, for example, suppose $M = 10$, $a = 325$ mm, $b = 65$ mm; then $P = 50$, which means that if the radius of stereoscopic vision with the naked eye were 450 m, the radius with this instrument would be 50 times as much or 22.5 Km. (J. P. C. S.)

[2] *Jahresbericht des Frankfurter Vereins 1858–59.* pp. 64–75.

[3] ¶The images of minute objects as seen through a binocular magnifying instrument are generally obliged to be focused comparatively too near the eyes, the result being that the images on the two retinas are so different that the impression of the object is apt to be more or less unnatural. This difficulty may be obviated in some measure by making the interval between the centres of the object-glasses less than the interpupillary distance

points that are in the focal plane of the microscope, whereas the images of all other points on one side or the other of this plane are small blur circles, and, owing to the division of the beam of light, one half of each circle falls on the right eye, and the other half on the left eye. Since the two halves of each circle are in a different position from each other, a stereoscopic effect is produced.

The principal points and focal points of the entire optical system of a microscope can be easily found by the rules given in Vol. I, pp. 81-84. Both the first principal point and the first focal point lie below the objective, the latter, however, being the nearer of the two. The second principal point and the second focal point lie above the ocular, the focal point being nearer to it. The observer's eye may be regarded as being at the second focal point. If the focal length of the entire system is denoted by p, and if f denotes the distance between the object and the first focal point measured upwards, and φ the distance between the image and the second focal point measured downwards, then by equation (7b) in §9 (Vol. I, p. 73):

$$\varphi = \frac{p^2}{f} \cdot$$

Let b denote the size of the object, and β that of its image; then

$$\frac{\beta}{b} = \frac{p-\varphi}{f-p} = \frac{p}{f} = \frac{\varphi}{p} \cdot$$

Now suppose that the eye is accommodated for the image β, and that there is another object b' either in front of b or behind it, which can be seen along with it, because b is transparent. If the distance of b' from the focal point is denoted by f', the distance between its image and the second focal point where the eye is will be

$$\varphi' = \frac{p^2}{f'} ,$$

and hence

$$\varphi' - \varphi = p^2 \cdot \frac{f-f'}{ff'} \cdot$$

Let a denote the angle of divergence of the bundle of rays coming from the object (b) and falling on the objective; and let α denote the corresponding angle at the image β; then by equations (7d) and (9) in §9 (Vol. I, pages 74 and 79):

$$b \tan a = \beta \tan \alpha$$

or

$$\tan \alpha = \frac{f}{p} \tan a \,.$$

---

and that is the fundamental principle of such optical devices as the binocular microscope. Incidentally, see description of KREIDL's binocular "relief" magnifying glass in *Zft. f. wissenschaftliche Mikroskopie*, Bd. 18. (J. P. C. S.)

Similarly, the corresponding angles a' and α' for b' and β' are connected by the formula:

$$\tan a' = \frac{f'}{p} \tan \alpha' .$$

Evidently, the radius of the blur circle in the plane of the image β for which the eye is accommodated will be

$$\rho = (\varphi' - \varphi) \tan \alpha' = \frac{p}{f}(f - f') \tan a' .$$

Since objects cannot be observed unless their blur circles are very minute, that is, unless $(\varphi' - \varphi)$ and $(f' - f)$ are very small, the change in angle a' for the various objects visible may be neglected, and the difference between a and a' may also be neglected. Accordingly, the last equation can be written

$$\rho = \frac{p \tan a}{f} \cdot (f - f') .$$

Now in the construction described above one half of this blur circle falls in the right eye and the other half in the left eye. Thus every line of the image that is perpendicular to the visual plane, no matter whether it is isolated or

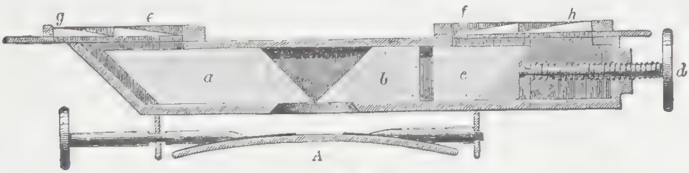

Fig. 62.

whether it is part of a uniformly coloured surface, will be transformed into a band of width ρ, where the broadening in one image is to the right and in the other image to the left. Thus, as compared with points in the focal plane, for a pair of bands of this sort there will be a stereoscopic parallax equal to ρ in the two images.

Suppose f' is less than f, that is, suppose the distance between the object and the objective is greater than the distance of those points from the objective for whose images the eye is accommodated, then φ' will be greater than φ, that is, the image of b' will be below the image of b, and hence when the rays coming from b' get to the plane where b is they will already have crossed each other. In this case the right half of the blur circle wil fall on the observer's right eye, and the left half on his left eye; and hence the stereoscopic parallax will be negative as compared with that of the object b, thus causing b_1 to appear to lie behind b, as it really does. Here one half of the blur circle goes to one eye by a double reflection and appears therefore in its natural position and not reversed as to right and left.

When the object b' is above b, everything is just the other way.

In Nachet's instruments the slider which regulates the positions of the prisms can be drawn out so far that the little prism b (Fig. 61) will be in front of the other (right-hand) half of the opening; and then a pseudoscopic effect will be obtained, so that objects or details that are really lower down will appear to be higher up; in other words, the relief will be reversed.

Nachet's design of the *binocular ophthalmoscope* is shown in Fig. 62. The principle is similar to that of the stereoscopic microscope. The silvering

has been removed from the middle of the concave mirror A. The two surfaces of the glass being of the same curvature, the rays traverse it without being refracted. The mirror is intended to illuminate the eye that 's to be observed. The observer views the real inverted image of the fundus of the patient's eye as produced by a convex lens which is between the mirror and the eye; as represented in the diagram Fig. 103, Vol. I, p. 243. After the rays from the patient's eye have traversed the aperture in the mirror, they fall on the two reflecting prisms a and b, where they are divided. The cross section of prism a has the form of a parallelogram with two 45° angles. Prisms b and c when united make a prism exactly like a, except that it is cut in two; so that the portion c can be moved one way or the other with respect to the other portion b, by means of the screw d. In this way the instrument can be adapted to the observer's interpupillary distance. The rays coming through the aperture and falling normally on the first face of prism a are reflected from the sloping face over to the opposite side of the prism, where they are again reflected to the opening at e; through which they pass into one of the observer's eyes. The other half of the beam enters prism b and traverses it and c in the same way, so as to issue finally through h into the observer's other eye. Weak refracting prisms are inserted in the openings at e and h to enable the observer to view the two images together without having to converge the lines of fixation of his eyes more than necessary. These prisms are mounted on a pair of sliders, each of which carries also another prism with a convex surface, which has therefore a slight magnifying action, and can be used or not as desired.

The best position of the convex lens between the mirror and the patient's eye is that in which it projects an image of the pupil in the hole in the mirror, as explained in Vol. I, pp. 243-246. Under these circumstances the light from the right half of the pupil enters the left-hand prism a, and that from the left half enters the right-hand prism b. Thus, the observer's right eye sees the fundus of the patient's eye as it appears from the left side of the pupil; while his left eye sees it as it appears from the right side. Incidentally, as the image is inverted also, this helps to give a correct stereoscopic effect, which is very noticeable as well as very useful in the clinical examination of the fundus.

Finally, I wish to mention here a peculiar stereoscopic method due to ROLLMANN.[1] He draws both projections on the same black chart, one in red lines, and the other in blue; and then putting on goggles with a red glass in front of one eye and a blue glass in front of the other eye, so that he can see only the red lines through the red glass and the blue lines through the blue glass, he uses them in a stereoscopic relief. By distributing spectacles of this kind among a number of persons in a theatre, a stereoscopic picture can be exhibited to them all at the same time. Mr. J. C. D'ALMEIDA projects the pictures on a screen by means of a pair of lenses with a red glass in front of one of them and a green glass in front of the other.[2]

Incidentally, the most various combinations of mirrors, lenses, etc. can be used to produce the necessary shifting of the images to obtain the stereoscopic effect. Sometimes only one picture is shifted, sometimes both. WHEAT-

[1] POGG. *Ann.* XC, 186–187.

[2] ¶Stereoscopic pictures recently exhibited in this way in a New York theatre excited much interest and curiosity, as though they were something very novel and marvellous. ROLLMANN's beautiful method is more accurately described and more fully explained by Professor v. KRIES in Note 10 at the end of this chapter.

See also: K. GENTIL, Die stereoskopische Projektion nach dem Verhafren von ROLLMANN und D'ALMEIDA. *Deutsch. opt. Woch.*, 7 (1921), 35-36. (J.P.C.S.)

STONE originally used two plane mirrors. BREWSTER[1] described a similar arrangement with two mirrors and another one with only one mirror. In the latter instrument either one picture or two pictures could be used. DOVE[2] and BREWSTER suggested the use of totally reflecting prisms instead of the mirrors. Some stereoscopes have one prism, others two. In the latter form a prism is placed in front of each eye; or the two prisms are united into a single so-called *reversion prism* and placed in front of one eye. A thin refracting prism will suffice also to shift one of the images so that it can be fused with the other. E. WILDE[3] used the double reflecting prism of a *camera lucida* for this same purpose.

In order to combine stereoscopic pictures without deviating the rays of light, BREWSTER proposed holding a glass plate at a proper distance in front of them with a little black spot on it as a mark of fixation. Mr. FAYE[4] used a screen with two holes in it to enable each eye to see simply the picture that was intended for it. Mr. ELLIOT[5] used two crossed tubes by which the right eye could see the picture on the left, and *vice versa*. It may be noted here that, owing to the difficulty in getting the proper accommodation, it is easier for a far-sighted person to fuse the images when the visual axes are crossed, whereas in the case of a near-sighted person it is easier when the visual axes are not crossed.

J. DUBOSCQ[6] inserted prismatic lenses in the frame of a pair of opera glasses, and then looked through them at stereoscopic pictures tacked on a wall. By adjusting the lenses one way or the other, the convergence of the axes of the eyes could be altered, thus causing the relief to be augmented or diminished.—In order to fuse pictures of any size, he put them in his panorama stereoscope, one above the other, opposite two plane mirrors standing side by side, which could be turned about a horizontal axis. The observer looks, between the pictures or underneath them, at the mirrors, the latter being so adjusted that the corresponding portions of the images overlap each other. It does not make any difference how wide the pictures are; and they may slide past in front of the observer's eyes. Subsequently, DUBOSCQ[7] described another form of instrument for combining large pictures which is more like BREWSTER's stereoscope and in which achromatic prisms with plane faces and separate lenses are used, both being adjustable in order to make the proper corrections in the image.

Revolving stroboscopic discs may also be inserted in the panorama stereoscope instead of the pictures, and then the moving figures can be seen in their bodily forms. This contrivance is known as the *stereophantascope* or *bioscope*. Mr. CZERMAK[8] has described another instrument called the *stereophoroscope*, which produces the same effect. He selected the ordinary lens stereoscope in which both pictures are mounted side by side on the same cardboard. These cardboards were attached to the plane faces of a polygonal wooden prism, which could be revolved around a horizontal axis. Surrounding this prism, and at a distance of several inches from the pictures, there was a cardboard cylinder with slits made in it at such intervals as to allow the

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1 *Phil. Mag.* (4) III, 16–26.

2 POGG. *Ann.* LXXXVIII, 183.

3 POGG. *Ann.* LXXXV, 63–67.

4 *Comptes rendus.* XLIII, 673–674.–POGG. *Ann.* XCIX, 641–642.

5 *Phil. Mag.* (4) XIII, 78.

6 *Cosmos* I, 97—104; 703–705.

7 *Comptes rendus.* XLIV, 148–150.

8 *Wiener Berichte*, XV, pp. 463–466.—See description of another similar instrument, so-called *stereotrope*, by SHAW in *Proc. Royal Soc.* XI, 70–73.

pictures to be seen at the proper instants. Beyond this cylinder there was the optical arrangement in BREWSTER's stereoscope to enable the observer to look through it and view the images through the slits as they passed by.[1]

Mr. C. C. CLARKE[2] has provided BREWSTER's stereoscope with a stand. Mr. KILBARN[3] made it so that it could be folded up compactly. SMITH and BECK[4] have added a stand, a firmer guide for the pictures, better illumination from all sides, and achromatic lenses. SAMUEL[5] has provided a device for adjusting the distance of the pictures from the lenses to the observer's distance of distinct vision.

CLAUDET's *stereomonoscope*[6] is original in construction. He noticed that the image in a *camera obscura* projected on the ground glass plate showed some stereoscopic relief when it was viewed with both eyes. The phenomenon is due to the fact that each eye sees best those rays that fall on the ground glass in the direction of its own visual axis. Accordingly, he constructed the stereomonoscope in which two corresponding stereoscopic images are projected by two lenses at the same place on a ground glass plate. When the plate is viewed by both eyes, each eye sees simply the picture that is intended for it, and thus the impression of relief is produced.

In order to investigate the effect of changing the position of the pictures, WHEATSTONE[7] modified his mirror stereoscope by mounting the parallel walls on slides, where the pictures are. He also made the two arms of the stereoscope so that they could be turned around a fixed axis between the two mirrors, in order to enable him to change the angle of convergence of the eyes. For the similar purpose of producing pseudoscopic relief, Mr. HARDIE[8] constructed an instrument with two pairs of mirrors similar to the telestereoscope which I afterwards made and which has been already described. It enables us to show the pictures reversed or in their proper position, and to enhance the relief or reduce it or reverse it. With the same object in view, Mr. H. MEYER[9] made the pictures in WHEATSTONE's mirror stereoscope so that they could be shifted in their own planes, and added a scale for measuring their displacements. WHEATSTONE proposed a contrivance by which the pictures would be moved on the arc of a circle so as to keep their distance from the eyes constant; and perhaps this method has the advantage that the retinal images are absolutely unaltered by lateral displacements of the pictures; whereas in MEYER's arrangement small corrections must be computed to allow for the change in the distance of the pictures from the eyes as they are moved in their own planes.

ROLLET[10] adjusted a thick plate of glass with plane parallel faces at an angle in front of each eye, and produced similar variations of convergence by looking at real objects through these glasses. According as the front

[1] ¶Stereoscopic motion pictures, visible to an entire theatre audience, were exhibited in New York during the winter of 1922. Pictures from two films, one intended for each eye, were projected alternately on the screen and observed through sectored discs, one of which was attached to the arm of each seat The alternations of the projections and the motions of the sectored discs were controlled by synchronous motors so that each eye could see only the picture intended for it. (W. W.)

[2] *Cosmos*. III, 123.

[3] *Cosmos*. III, 770.

[4] *Athenaeum*. 1858, II, 269.—*London J. of Arts*. June 1860.

[5] *Rep. of Brit. Assoc.* 1858, 2. p. 19.

[6] *Proc. Royal Soc.* IX, 194–196.

[7] *Phil. Trans* 1852. pp. 1–17.

[8] *Phil. Mag.* (4) V, 442–446.

[9] POGG. *Ann.* LXXXV, 198–207.

[10] *Wiener Sitzungsber.* XLII, 488–502.

surfaces were turned toward the nose or toward the temporal side of the eye in question, the lines of fixation were made divergent or convergent, respectively. Thus the effects corresponded with those observed by WHEATSTONE.

Stereoscopic pictures may be made by photography or by drawing them in perspective and reproducing them by lithographic or copper plates.[1] By the latter method good results are obtained only in the case of non-shaded line drawings of geometrical figures, such as regular solids or crystal forms. At the same time, they afford the best illustrations of stereoscopic effects, without any coöperation on the part of illumination and shading to promote the illusion. However, they have to be made with the utmost accuracy in order to prevent the images from looking distorted, as the slightest discrepancies may result in very marked changes in the relief. Exceedingly complicated geometrical figures may be made to give a clear and correct spatial impression by this means. As such drawings can be readily purchased anywhere, it is unnecessary to give any illustrations of them here. So far attempts at shading such lithograph drawings have been rather unsuccessful, owing to the fact that it is not possible to make the gradations of shade in the two pictures sufficiently uniform. ROOD's apparatus for the construction of such figures was mentioned above on p. 333.

Stereoscopic photographs are far more perfect in their results. Such pictures were first made by Professor MOSER of Königsberg, and their manufacture has already become an important industry. Objects represented in these photographs include landscapes and buildings from all parts of the world, statues, animals, flowers, etc. At first these pictures were generally made by taking two exposures with the same camera from different positions. However, there was this disadvantage about this process, that in bright sunshine the shadows cast by objects would change during the time required to readjust the camera, the result being that a false effect would be obtained. For instance, the shadows in this case may produce an impression as if they were solid screens floating in air. I remember looking at a stereoscopic view of the city of Paris, in which there was a church steeple with a clock on it, and you could tell by the position of the minute-hand on the dial that a period of five minutes had elapsed between the times of taking the two photographs. There is also the extra trouble of having to provide two sensitive plates, etc. And so lately it has become very common to follow BREWSTER's suggestion[2] to use cameras with two photographic lenses, in which the two views are focused side by side on different parts of the same plate. The interval between the centres of the two objectives is made the same as the interpupillary distance, or perhaps a little larger, say, from 70 to 75 mm. Thus the camera itself constitutes a sort of inverted stereoscope. These instruments are very convenient for making pictures of near objects, and they reproduce immediately the same view of the object which an observer would have had by occupying the place where the camera was. A special advantage about them is that by making an instantaneous exposure in bright sunshine it is possible to obtain good pictures of things in motion such as people, animals, ships, and indeed realy splendid views of waves on the surface of the sea. But in the case of landscapes in which there are distant objects they are not altogether satisfactory, because the distance between the two lenses is too small to obtain sufficiently large parallaxes; and usually, therefore, the farther parts of the landscape look absolutely flat.[3] For such scenes it is

[1] Mr. HESSEMER made very good pictures in this way and has explained the process in DINGLERS *polytechn. Journ.* 89, pp. 111–121.

[2] *Phil. Mag.* (4) III, 26–30; 1852.—*Rep. of Brit. Assoc.* 1849, 2. p. 5.

[3] As to the choice of the angle, see CLAUDET in *Cosmos*, IV. pp. 65—67 and 147; also SUTTON, *ibid.*, IX, pp. 313–319.

better to get a sort of telestereoscopic effect by taking two photographs from stations farther apart. For example, in the very fine photographic pictures of landscapes made by Braun of Dornach, I found several pairs of stereoscopic views of the Wetterhorn, each of which had been photographed from different points in Grindelwald, and another pair of views of the same mountain taken from different places of the Bachalp, and also some pictures of the Jungfrau taken from Mürren. By cutting the pictures apart and recombining them in pairs so as to obtain stereoscopic views of the same landscape from two much farther separated points, extremely beautiful results could be obtained in the stereoscope, bringing out clearly the outlines and forms of the mountains. The original pictures did not give any better idea of the scene than a spectator could get by standing still and looking at the thing itself. But when the pictures were interchanged as above described, it was possible to make out better the real topography, somewhat in the same way as an observer might do by walking about and comparing the different views which he got from different places.

Very beautiful stereoscopic views of microscopic objects have been made by Babo.[1] The two pictures were obtained by altering the inclination of the stage of the microscope to the axis of the instrument, thereby producing stereoscopic parallax.

Mr. J. G. Halske made moving stereoscopic pictures. The first one consisted of a pair of pictures representing a frustum of a cone with the central small circles movable in a horizontal direction. But the most beautiful effect was obtained with a round black disc about 3 inches in diameter, which was mounted horizontally so that it could spin smoothly on its axis, continuing for quite a long time when once the motion got started. A smaller white disc or wafer was laid on top of it. A totally reflecting right-angle prism was properly adjusted in front of one of the observer's eyes; his other eye being free. When, as the large disc turned, the small one happened to be to the right of the centre, the free eye would see it over there, whereas the other eye would see it on the opposite side of the centre owing to the reflection in the prism; and so there would be a stereoscopic parallax. Thus the small disc appeared to pass through the larger one, alternately rising and falling.[2]

Historical. The earlier discussions of the subject of depth-perception were connected at first with the differences in the apparent size of the moon. Ptolemy (150 a. d.) states that the human mind forms an opinion of the size of an object after having made a preliminary estimate of its distance. Distances appear larger when there are many objects between the eye and the thing that is being observed, as is the case when the heavenly bodies are near the horizon.[3] In another place, to be sure, he ascribes the magnification to the refraction of the rays of light by the vapour in the atmosphere.[4] Alhazen[5]

[1] *Bericht der Freiburger Ges.* II, 312–314.

[2] Some other studies of stereoscopic vision and particularly of the conditions required for it will be discussed in Note 11 at the end of this chapter. Recent developments in the construction of binocular instruments are so important that it would seem desirable to go more fully into this subject here. And so the second division of the Appendix will be devoted to it.—K.

[3] Montucla, *Hist. des Mathém.* Vol. I, p. 309.—Rogeri Baconis *Perspect.*, p. 118.—Priestley, *Geschichte der Optik*, Klügel's translation, pp. 11–12.—Gregory, *Geometria. Pars univers.*, p. 141.—Malebranche, *Recherche de la verité.* P. 1.—Huygens in Smith, *Opticks.* Art. 586.—Logan in *Phil. Trans.* XXXIX, 404.

[4] *Almagest*, Lib. III, c. 3. Also Strabo in *Geogr.* I, 3.

[5] Alhazen, lib. VII, pp. 53–54.

(in the tenth century)[1] refutes this latter view and adopts the former. Roger Bacon followed Alhazen's opinion, but Porta[2] did not. Vitellio[3] (1270) likewise adopted Alhazen's opinion, and called attention to the fact that the arch of the sky appeared somehow to be farther away at the horizon than at the zenith. Kepler[4] whose view was practically that of Descartes[5] also, speaking of the judgement of distance, states plainly that the interval between the eyes is the base-line which is used in measuring the distances of observed objects. And since it is possible for one eye to learn to make such estimates, he concludes that for small distances the width of the pupil might serve as base-line. He also observes that the different degrees of brightness can be estimated even with one eye, and that we learn by experience to compare the size of an object with its distance, by finding out how far the hand has to be stretched forth to touch it, or how far one must walk to reach it. Thus, except for the difference between the images in the two eyes, he was aware of the main factors in making this estimate.

But Gassendi[6] was able to state that the reason why the moon looks bigger near the horizon was because then the light entering the eye was less bright and therefore the pupil was dilated. Hobbes[7] returned to the explanations that had been given by the ancients, and assumed the apparent form of the arch of the sky to be that of a portion of a spherical surface. On the other hand, Father Gouye,[8] Molyneux,[9] and Samuel Dunn[10] observed that it was not necessary for objects to be between the moon and the eye in order to get the illusion, and that sometimes at any rate it could be produced without intervening objects. Desaguliers[11] contrived experiments by which the observer was induced to make false estimates of the distance and therefore also of the size. Berkeley[12] insisted on the hazy appearance of the moon near the horizon and on its low luminosity there; and undoubtedly these circumstances do have a decided influence. Robert Smith also (in his *Compleat System of Opticks*, §162, foll.) investigated the "Concavity of the sky." He made a series of estimates of apparently equal distances, some lying near the zenith and others near the horizon, and found that the horizon is apparently three or four times as far away as the zenith. Lambert[13] compared the section of the celestial vault with that of a line on a shell. The form and width of the rainbow is also changed by it, being flattened like an ellipse, and being narrower in the middle than at its ends. Solar halos and star distances are apparently altered in the same way. The following beautiful experiment was given by Smith. When a little round wafer is placed in the focus of a convex lens, its image in the lens always subtends the same angle at the eye, no matter how far away the observer may be, provided that the outline of the image is still visible through the lens. Apparently, however,

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1 ¶He is said to have died in Cairo about 1100 A.D. (J. P. C. S.)

2 *De refractione*, pp. 24, 128.

3 *Optica*, Risner's edition, p. 412. Basel 1572.

4 *Paralipomena*, pp. 62–66. 1604.

5 *Dioptr.* p. 68.—*De homine*, pp. 66–71.

6 Gassendi, *Opera.* Vol. II, p. 325.

7 Robin's *Tracts.* Vol. II, pp. 241–244.

8 *Mém. de l'Acad. de Paris.* 1700, p. 11.

9 *Phil. Trans.* Vol. I, p. 221.

10 *Phil. Trans.* Vol. LII, p. 462.

11 *Phil. Trans.* Vol. VIII, p. 130.

12 *Essay toward a new theory of vision.* Dublin 1709, p. 30.—Robin, *Mathemat. Tracts*, II, 242.

13 *Beiträge.* I, §§60–78.

the size of the image increases enormously as the observer recedes from it, because he does not think of it as being infinitely distant, but rather as being just beyond the lens.

SMITH carried on a controversy with BERKELEY about the latter's injection of aerial perspective into the discussion, but he had to admit that the apparent size of the moon on the horizon varies from time to time. EULER[1] supported BERKELEY.

The influence of the apparent distance on the judgment of the absolute size was insisted on too by MALEBRANCHE and BOUGUER[2] in reply to VARIGNON.[3] The views of DE LA HIRE[4] and PORTERFIELD[5] as to how the distance was estimated were in conformity with the opinions mentioned above.

Reversal of the relief is a phenomenon which had been observed a long time ago. It was first noticed by JABLOT[6] and P. F. GMELIN[7] in looking through a microscope or telescope in which the image was inverted. RITTENHOUSE[8] tried to explain it as being due to a reversal of illumination. MUNCKE[9] objected to this explanation because the phenomenon can be seen also in a simple magnifying glass. ABAT confirmed this latter view by directing attention to a pretty experiment as follows: In looking at the inverted image in a concave mirror of a bottle half filled with water, the empty part seems to be filled and the part where the water is seems to be empty, because we naturally suppose that the water is always *below* the surface. The more recent discoveries and explanations of the inversion of relief have been given in the text.

That there must necessarily be some differences between the images of a material object as seen by the two eyes, was clearly understood by EUCLID, GALEN, PORTA and AGUILONIUS,[10] and they were conscious of the difficulties it involved. LEONARDO DA VINCI[11] insisted that it made such a difference that it was impossible for any painting to imitate the effect produced by binocular vision. SMITH describes an experiment in which he looked at a distant object through the open legs of a pair of compasses. The compasses were held at some distance in front of his face, with the interval between their points about the same as the interpupillary distance. At first he saw two images of the two prongs; but on gradually bringing them closer together, "the two inner points will come nearer to each other, and when they unite," "the two inner legs will also entirely coincide and bisect the angle under the outward ones; and will appear more vivid, thicker and longer than they do, so as to reach from your hand to the remotest object in view, even in the horizon itself, if the points be exactly coincident." (*Compleat System of Opticks*, §977). This was a stereoscopic perception.[12] Similar perceptions have been obtained by WELLS[13] with cords and rulers.

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[1] *Briefe an eine deutsche Prinzessin.* S. 317.

[2] *Mém. de l'Académie.* 1755. pp. 99 and 156.

[3] *Ibid.*, 1717.

[4] *Mém. de Paris.* 1694.

[5] *Treatise on the eye.* 1759.

[6] *Description de plusieurs nouveaux microscopes.* 1712.

[7] *Phil. Trans.* 1747.

[8] *Trans. of the American Philos. Society.* 1786. II.

[9] GEHLERS *physik. Wörterbuch*, new ed. Leipzig 1828. IV, 1455.

[10] D. BREWSTER, *The stereoscope, its history, theory and construction.* London 1856.

[11] *Trattato della pittura.*

[12] ¶F. J. CHESHIRE (Article on "Range-finder" in GLAZEBROOK'S *A Dictionary of Applied Physics*, Vol. IV, p. 636) has called special attention to this remarkable observation

Just how much this difference in the retinal images contributes to the perception of depth was not realized, however, until WHEATSTONE's important invention of the stereoscope. The first account of it was published in 1833,[1] and a more complete description of the phenomena and their theory in 1838.[2] D. BREWSTER[3] states that an Edinburgh mathematician named J. ELLIOTT had also invented it in 1834 and published an account of it in 1839. Mr. G. MAYNARD[4] is a third claimant. In any case the priority belongs to Mr. WHEATSTONE, and, moreover, his article published in 1838 not only contains a description of his mirror-stereoscope but is filled with experiments and observations exhibiting and demonstrating clearly practically all the necessary conditions. Afterwards, in 1858, a double picture was found in Lille by Dr. A. BROWN,[5] which had been made by JACOPO CHIMENTI (b. 1554; d. 1640). It represented a man sitting on a low stool holding a pair of dividers in one hand and a plumb line in the other. In a stereoscope the two pictures gave some sort of relief effect. D. BREWSTER conjectured that the pictures may have been made by CHIMENTI to test PORTA's theory, which was published in 1593. Since then photographic reproductions of these pictures have been made for sale. The two pictures of the man were certainly made from different positions, but I must admit that it seems to me very unlikely that CHIMENTI intended them for a stereoscopic experiment, because the stool, the dividers, and the plumb line, which could easily have been drawn correctly, are treated as unessentials and are drawn so irregularly and so differently that they cannot be combined. Had the artist desired to test a theory, it is more than likely that he would have drawn the easy things correctly and the difficult parts, such as the man, more inaccurately. It seems more probable to me that the artist was not quite satisfied with his first figure and did it over again from another point of view, using the same sheet of paper quite by accident.[6]

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and pointed out that it was "a very complete and interesting anticipation" of the principle of the so-called "wandering mark" employed in one of the types of stereoscopic range-finders.—See also M. v. ROHR, *Die binokularen Instrumente* (Berlin, 1907), p. 29. (J.P.C.S.)

[13] *Essay upon single vision with two eyes.* 1792. 2d ed. 1818.

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[1] In H. MAYO's *Outlines of human physiology.* p. 288.

[2] *Phil. Trans.* 1838. P. II, 371–394.

[3] *Liverpool and Manchester Photographic Journal.* 1857, January 1, pp. 4–7.—January 15, pp. 21–23.

[4] *Toronto Royal Standard.* 1836.—*Toronto Times.* 1857, October 8.

[5] *Photographic Journal*, 1860, May 15.—*Encyc. Britann.* Article on "Stereoscope."

[6] ¶ M. v. ROHR (OSTWALDS *Klass.* No. 168, p. 124) has called attention to the interesting fact that some fifteen years or more prior to WHEATSTONE's invention, in a volume entitled *Le conservateur de la vue* (3rd. ed., Paris 1815) written and published by J. G. A. CHEVALLIER (partner of CH. CHEVALIER), the word *stéréoscope* is employed to describe an instrument that used to be called a megascope, although it is generally known nowadays as episcope, which is intended for opaque projection of solid objects. The impression of solidity or depth obtained with this instrument was not so much a *depth-perception* of binocular vision (as in the case of WHEATSTONE's stereoscope) as it was a *depth-conception* such as is produced in monocular vision by tokens of depth (shadows, reflections, shades of colour, etc.). In a letter to the editor, Professor v. ROHR writes (March 16, 1925): "Ich glaube, dass danach kein Zweifel bestehen kann, dass der griechische Bestandteil stereo, etwa körperlich, zunächst von CHEVALLIER gebraucht wurde, um eine Tiefen*vorstellung* zu beschreiben, und erst mehr als 20 Jahre danach WHEATSTONE dazu diente, eine Tiefen*wahrnehmung* eben die auf den Gebrauch beider Augen zurückgehende, zu kennzeichnen."

See M. v. ROHR, *Abhandlungen zur Geschichte des Stereoskops.* Leipzig, 1908.—E. DIAZ-CANEJA, WHEATSTONE's stereoscopic experiments. *Arch. de Oft. Hisp.-Amer.*, 22 (1922), p. 297. (J. P. C. S.)

The form of lens stereoscope now in common use was described by BREWSTER in 1843. The appended bibliography will give some idea of other inventions in this field. The history of the theory of these phenomena will be given in the next chapter. Investigations with respect to the errors of pure binocular localization have been begun in the past few years by RECKLINGHAUSEN,[1] HERING,[2] J. TOWNE and myself,[3] but they require to be repeated and extended by other observers.[4]

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[2] *Beiträge zur Physiologie*. Leipzig 1864. Hefte 4 and 5.

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[4] ¶An indispensable book of reference concerning the theory and historical development of binocular instruments is M. v. ROHR'S *Die binokularen Instrumente*. 2. Aufl. Berlin, 1920. (J. P. C. S.)

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Notes on § 30 by v. Kries

1. The apparent magnification of the sun and moon near the horizon [see p. 290] has been the object of a great many recent investigations and explanations. Incidentally, attention has been called to some noteworthy references to the subject in the older literature, especially to a letter written by Gauss[1] to Bessel, in which this question was discussed. The articles referred to in the accompanying footnote[2] are the more important contributions on the subject that have appeared since 1867. The new data have been obtained mainly by performing Helmholtz's mirror experiment [p. 291] over again and doing it much more carefully.

Filehne insisted on the importance of taking special precautions with this experiment in order to see the reflected image of the moon really in the sky, so as not to localize it, as is usually done with images in a mirror, at some little distance from the mirror. By being careful to observe the requisite conditions in this respect, he found that "the sun, moon, and all constellations of stars, no matter whether they were reflected from the horizon up into the sky or from the sky down to the horizon, have the same apparent size at either place as they would have if the objects themselves had really been there." Similar results were also obtained by Zoth. But whereas Filehne attached particular

[1] Gauss, Correspondence between G. und Bessel, published by the *preuss. Akademie der Wissenschaften.* 1880. S. 498.

[2] Filehne, Pflügers *Archiv.* LIX. p. 279. 1895.—Zoth, ibid. LXXVIII. 1899. S. 363.—Idem, ibid., LXXXVIII, 1902. p. 201.—Reimann, *Zeitschr. f. Psychologie.* XXX. pp. 1 and 161. 1902; XXXVII. p. 250. 1905.—Bourdon, *La perception visuelle de l'espace.* Paris 1902. p. 392.—Guttmann, Blickrichtung und Grossenschatzung. *Zeitschr. f. Psychol.* XXXI, p. 333. 1903.—Bernstein, Das Leuchtturmphanomen und die scheinbare Gestalt des Himmelsgewolbes. *Zeitschr. f. Psychol.* XXXIV. p. 132. 1904.—Stratton, Der linearperspektivische Faktor in der Erscheinung des Himmelsgewolbes. *Zeitschr. f. Psychol.* XXVIII, p. 42. 1902.—A. Muller, Über den Einfluss der Blickrichtung auf die Gestalt des Himmelsgewolbes. *Zeitschr. f. Psychol.* XL. p. 74. 1905.—B. Mayr, Die scheinbare Vergrösserung von Sonne, Mond und Sternbildern am Horizont. Pflügers *Archiv.* CI, p. 349. 1904.—Feilchenfeld, Über die Grossenschatzung im Sehfeld. *Archiv f. Ophth.* LIII. p. 401. 1904.—Claparède, L'agrandissement et la proximité apparents de la lune à l'horizon. *Archives de psychol.* V. pp. 121. 1905.

importance to the fact that an impression of great distance is produced just at the horizon by the succession of intervening objects filling up the lower half of the field of view, ZOTH (and subsequently GUTTMANN also) argued that the principal factor was the direction of the line of sight. In fact both of these observers noted that, under conditions otherwise identical, other objects too will look smaller when they are observed with the eyes raised than they do when the eyes are directed horizontally. The differences, it is true, are not very considerable. GUTTMANN found that they were between 3 and 4 percent. In the case of the moon this is certainly far less than the difference of size which I myself notice. BOURDON was not able to detect any connection whatever between the impression of size and the direction of the line of sight. Undoubtedly, therefore, as ZOTH too insists, there must be other factors, such as aerial perspective that are also of importance. REIMANN especially takes the view that the reason why the celestial dome has the appearance of a flattened vault is connected in some way with the peculiarity of the atmosphere, and that this is responsible for the magnification near the horizon. He elaborated this idea in an original way. Thus he started out with the view, which is held by nearly everybody, and which was entertained especially by HELMHOLTZ himself, namely, that there is some direct and precise connection between the impression of larger absolute size and the impression of greater distance from the spectator, and that this latter is the cause of the former. In my opinion this assumption cannot by any means be regarded as self-evident, and as soon as its doubtful character is admitted, the whole problem may be approached from essentially different points of view. However, the reasons for this opinion cannot be given in detail until we come to the final chapter at the end of this volume, where the problem of the apparent size of the heavenly bodies will be briefly discussed again [see page 602].[1]

2. Perception of depth by means of accommodation [see p. 294] has been comparatively recently investigated by HILLEBRAND,[2]

[1] ¶ The following is a list of some of the more recent literature on this subject:

W. FILEHNE, Das WEBER-FECHNERsche Gesetz und die wechselnde scheinbare Grösse des Gestirne. *Arch. f. Physiol.*, 1912, pp. 185-187.—M. PONZO, Rapports entre quelques illusions, visuelles de contraste angulaire et l'appréciation de grandeur des astres à l'horizon. *Arch. ital. de biol.*, 58 (1913), 327-329.—H. HENNING, Die besonderen Funktionen der roten Strahlen bei der scheinbaren Grosse von Sonne und Mond am Horizont, usw. *Zft. f. Sinnesphysiol.*, 50 (1919), 275-310.—H. DEMBER and M. UIBE, Versuch einer physikalischen Losung des Problems der sichtbaren Grössenanderung von Sonne und Mond in verschiedenen Hohen uber dem Horizont. *Ann. der Physik*, 61 (1920), 353-378.—A. MÜLLER, Beitrage zum Problem der Referenzflächen des Himmels und der Gestirne. *Arch. f. d. ges. Physiol.*, 41 (1921), 47-89. (J. P. C. S.)

[2] HILLEBRAND, *Zeitschr. f. Psychol.* VII. 1894. p. 97.

DIXON,[1] ARRER,[2] and BOURDON.[3] HILLEBRAND succeeded in devising a method that was even better than the experiment with three cords for getting rid of the effect of extraneous conditions. He used a black screeen which filled one half of the observer's field of view. It was seen against a white background, and could be shifted toward the observer or away from him. The results indicated that accommodation was considerably less reliable than would be inferred from WUNDT's data as given in the text.[4]

3. Recently, numerous investigators, especially EINTHOVEN[5] and EXNER,[6] have sought to ascertain the reason why co-planar objects of different colours do not appear to lie in one plane [see p. 294]. EXNER has briefly reported some very beautiful observations of this kind. EINTHOVEN showed that the phenomenon is not due primarily to unequal accommodation for long and short wave-lengths of light, but is caused by more complicated relations of a dioptric nature (dispersion and lack of perfect centering of the refracting surfaces of the eye). As the main interest in the phenomenon is connected with these dioptric conditions, it will not be considered more fully here.

4. HELMHOLTZ has described [page 295] certain changes in the configuration of observed bodies due to motion on the part of the observer, and discussed the effect they had on perception of distance. The changes of which he speaks are such as the observer would notice if he advanced forward without changing the attitude of his head or his eyes especially. In reality the phenomena are complicated by the fact that, supposing our attention is attracted, not by some object moving along with us, but by stationary external objects, we are invariably in the habit of keeping the eyes fastened for a brief space on some definite point, by turning them so as to counteract the effect of the forward motion of the body. This accounts for the jerky move-

[1] DIXON, *Mind*. 1895. p. 195.

[2] ARRER, WUNDTS *Philos. Studien*. 1896. pp. 116, 222.

[3] BOURDON, *La perception visuelle de l'espace*. 1902.

[4] As to the nature of this connection between accommodation and perception of depth, and especially with respect to a supposed special feeling of accommodation, see my remarks on the subject in the concluding chapter.—K.

¶See also K. W. ASCHER, Zur Frage nach dem Einfluss von Akkommodation und Konvergenz auf die Tiefenlokalisation und die scheinbare Grosse der Sehdinge *Zft. f. Biol.*, 62 (1913), 508-535.—J. BAPPERT, Relation of accommodation and convergence to perception of depth *Zft. f. Psychol. d. Sinnesorg.*, 90 (1922), 167-203. (J. P. C. S.)

[5] EINTHOVEN, On the production of shadow and perspective effects by difference of colour. *Brain*. XVI, p. 191.—Idem, Stereoskopie durch Farbendifferenz. *Archiv f. Ophth.* XXXI. (3). p. 211.

[6] EXNER, Perspektivische Täuschungen an farbigen Bildern, die durch prismatische Brillen betrachtet werden. *Zentralblatt f. Physiologie*. 1906 p. 843.

ments of the eyes that may be noticed by watching a passenger on a railway train who is gazing out of the window while the train is in motion. What happens in this case is that for a brief space the image of the point of fixation for the time being remains stationary at the place where it is on the retina, while the images of objects that are nearer and farther than this point glide over the retina in opposite directions. And so the point of fixation, being perceived as stationary, serves as a point of reference; and points which are farther away appear to be advancing in the same direction as the observer, while points which are nearer appear to be receding in the opposite direction. Now these apparent motions are just as useful as those described by HELMHOLTZ for forming estimates of distance; and the probability is that both of them generally contribute to the result in some way, although it would be hard to say exactly how.

5. The stereoscopic parallax as defined by HELMHOLTZ [on page 299] is a certain length depending on a definite distance between the stereogram and the frontal plane through the observer's eyes. As the theory came to be developed further, a definition was needed for the difference of position of two points, one of which was intended for one eye and the other for the other eye, that would be independent of the somewhat arbitrary distance of the stereogram itself. Consequently, there is a tendency now to define binocular parallax in terms of certain angles that are found to be suitable for this purpose. If we stop to consider what angle should be called the binocular parallax, we can readily see that there are several different possibilities. The simplest method of all, which happens also to be most nearly in accord with the earlier definition of this term, is to define the binocular parallax of a point as the difference between the two angles made with the so-called sagittal direction by the lines joining each eye with the projection of the given point on the horizontal plane through both eyes. Thus in Fig. 63, suppose that the plane of the paper represents the horizontal plane through the two eyes A and B, and that P is the projection of the given point in this plane; and let AS and BS' drawn parallel to each other indicate the sagittal direction; then the binocular parallax of the point will be measured by the difference between the angles SAP and $S'BP$, that is, by the angle BPA. The angle thus defined is practically the same as the difference of azimuth (*Breitenwinkel*)[1] of the

Fig. 163

[1] As defined by HELMHOLTZ in the next chapter, page 418.

given point with respect to the two eyes, on the supposition that the latter are directed straight forward in parallel horizontal lines. The only reason there is any difference at all is on account of the deviation of the apparently horizontal and vertical planes from those that are actually horizontal and vertical; and for eyes in which these deviations do not exist there would be no difference.

In many cases it is of special interest to know how much the parallax of some point exceeds that of the point that happens to be the point of fixation for the time being, or is less than it. In other words, it is often convenient to regard the parallax of the point of fixation as being equal to zero. And so this positive or negative value may be called the *instantaneous parallax* of a point; whereas, to prevent confusion, the parallax as defined above may be called the *absolute parallax*. And, finally, the difference between the values of the absolute parallax of two points may be called their *relative parallax*. Evidently, when one of the two points is the point of fixation, the relative parallax will be the instantaneous parallax of the other point.[1]

A brief statement may be added here in regard to the formulae for the different kinds of parallax. Let p denote the absolute parallax of the point P in Fig. 63, and put $s =$ angle $S'BP$ and $2a = AB =$ the interpupillary distance; then if the distance of P from the frontal plane through the two eyes is denoted by F, evidently,[2]

$$2a = F\{\tan(s + p) - \tan s\}\ .$$

For points near the median plane and not too near the observer, s and p will both be small, and then approximately:

$$p = \frac{2a}{F}\ .$$

For points located like the above but at the same time near the horizontal plane through the eyes, the distance E between the given point and the eyes themselves may be substituted for F; that is, for points which are not far either from the median plane or the horizontal plane through the eyes, the binocular parallax is equal to $2a/E$.[3] The

[1] This rather involved terminology is simplified in practice, because in most cases the context indicates immediately the sense in which the term parallax is used, so that generally it is not necessary to specify whether we mean absolute, relative or instantaneous parallax.

[2] ¶This simple relation is self-evident in case the point P is very far away and not much over to one side or the other so that the angle APB is small and the distance F is practically equal to AP or BP. (J.P.C.S.)

[3] ¶Comparing this result with HELMHOLTZ's expression (1c) for the stereoscopic difference e as measured on a plane parallel to the frontal plane at a distance from the eyes equal to b, we see at once that $p = e/b$, on the assumption that the angle p is small. (J.P.C.S.)

relative parallax of two points satisfying the above conditions will be given, therefore, by the following expression:

$$\frac{2a(E_1 - E_2)}{E_1 \,.\, E_2}.$$

And supposing the depth-interval (δ) between the two points under consideration is so small compared with the distance of either one of them from the observer that we may put $E_1 \cdot E_2 = E^2$, where E denotes therefore the average distance in this sense, then the relative parallax may be calculated from the following expression:[1]

$$\frac{2a\delta}{E^2}.$$

Accordingly, the relative parallax corresponding to a certain small difference of depth will be inversely proportional to the square of the observer's mean distance from the two points that are separated from each other by that interval.

For the sake of being as precise as possible in our use of terms, it might be advisable to employ the term binocular parallax strictly in the purely geometrical sense as above defined. On the other hand, HERING especially has introduced the expression *cross-disparity* (*Querdisparation*), which has a physiological significance and is defined in terms of physiological conditions. Thus, in particular, the cross-disparity of a point is said to be zero when the places where its images fall on the retinas of the two eyes are such as to give the same impression of direction or the impressions of two directions that differ only in elevation. Whether the cross-disparity of a point is zero, will depend therefore not only on its position and the instantaneous adjustment of the eyes, but also on certain physiological conditions, namely, on the distribution of the place-values (*Ortswerte*) on the retina, as HERING would say. Wherever it seems advisable and permissible to make certain simplifying assumptions in this respect it would certainly seem best to regard cross-disparity also as being an angular magnitude having some definite connection with the binocular parallax. For instance, this is what HILLEBRAND has done in a paper[2] which will be discussed later. The fact is, indeed, that there is a certain limited region

[1] ¶This expression should be compared with HELMHOLTZ's formula (2a) on page 333, which in terms of the above notation may be written:

$$\frac{e_{,} - e_{,,}}{b} = \frac{2a\delta}{E^2},$$

where E^2 is written in place of $\rho_{,}.\rho_{,,}$. (J. P. C. S.)

[2] *Denkschriften der Wiener Akademie. Math.-naturw. Klasse.* 72. 1902.

in which we do not need to make any distinction between cross-disparity and instantaneous parallax, that is, the value of the latter as defined geometrically may be considered as being equivalent to the former.

6. Strictly speaking, the experiments described here in the text [page 306] are not to be taken as a determination of the limit of accuracy of binocular perception of depth. HELMHOLTZ was content rather simply to establish the fact that, when the parallax reaches a certain limiting value necessary for monocular discrimination, it is still possible to have a reliable perception of depth. Whether he refrained from trying to determine the real limit because he was doubtful on theoretical grounds of the possibility of a still higher degree of accuracy, or whether he did not get around to it for other reasons, it is impossible to say definitely. As a matter of fact we know now from a series of investigations that a reliable perception of depth is possible even with values of the parallax that are distinctly lower than those which HELMHOLTZ considered sufficient. In experiments of this kind HEINE[1] obtained with different individuals limiting values ranging from 6 to 13 seconds of arc. PULFRICH[2] got values of 10″ or less. And in BOURDON's experiments,[3] at least in the majority of cases, it was found to be possible to detect difference of depth correctly when the parallax amounted to only 5″. Essentially the same method as HELMHOLTZ used was employed in all these tests. Three parallel vertical rods were observed, the two outer ones being stationary in a frontal plane, whereas the central one could be shifted one way or the other in a sagittal direction. The test could be made in two ways: either by finding how far the central rod has to be from the plane of the other two rods before it could be told certainly whether it was in front of this plane or beyond it; or by finding the average error made by the observer in trying to place the central rod in the plane of the two outer ones. A screen with a suitable opening in it was interposed in front of the observer so as to make sure that all he could see was simply the middle portion of each rod and not any of the mountings, etc.; so that all other factors were eliminated that might have influenced his judgment of depth. In these experiments, as was indicated above, the parallax can be put equal to $2a\delta/E^2$, where $2a$ denotes the interpupillary distance, E the distance of the rods from the observer, and δ the amount by which the central rod is out of the plane of the

[1] HEINE, *Archiv. f. Ophth.* LI. 1900. p. 146.

[2] PULFRICH, *Physikal. Zeitschrift* 1899.—*Zeitschrift f. Instrumentenkunde.* 1901.

[3] BOURDON, *Revue philosophique.* XXV. 1900. p. 74.

other two rods.—Incidentally, I may say that it does not seem to me quite to the point to assert, on the basis of these new data, that HELMHOLTZ's general statement, namely, that the binocular power of depth-perception is about the same as the resolving power of the eye in monocular vision, has been shown to be incorrect. The facts admit of being considered rather from a different point of view, which in my judgment is of greater significance. It is just the monocular resolving power for distinguishing a pair of points or parallel lines, which are exposed simultaneously side by side, that lags so far behind the binocular power of depth-perception. On the other hand, when the tests of resolving power are made with other types of objects, it is found that angles of the same order of size (say 10″) as those obtained in measurements of binocular depth-perception occur also in determinations of the keenness of monocular vision. This is the case, for instance, in the so-called vernier method where the test consists in deciding whether a piece of one line is the exact continuation of another line or whether one is shifted so as to be parallel to the other;[1] and also when we try to detect the least perceptible motions.[2]

Accordingly, the statement appears to be justified that the relation suspected by HELMHOLTZ is actually approximately true. The only point is this, that in making comparisons between the monocular resolving power and the binocular power of depth-perception, the thing that is involved is not so much the ability of distinguishing two adjacent objects as being really separate as it is some other kind of ability such as the ability to detect a break in a line or to perceive a motion. Of course, it is perfectly obvious that, strictly speaking, this is not the same thing exactly as perception of depth; and naturally all we could expect would be some approximate agreement.[3]

7. HELMHOLTZ's criticism of WUNDT's method [p. 314] was justified. BOURDON[4] has met the objection by changing the method of the experiment. Instead of bringing the object closer or moving it farther away, he took a stationary object which was at a certain definite distance in one case or which could be adjusted closer to him or farther from him in the other case, and made it visible for brief times in an otherwise dark room; so that a short interval of time elapsed between successive exposures, during which the eyes might move as they pleased. By taking special precautions, variations of brightness and

[1] WÜLFING, *Zeitschr. für Biologie.* N. F. XI, p. 199. 1893.

[2] BASLER, PFLÜGERS *Archiv.* CV. S. 582. 1906; also CXXIV. p. 313. 1908.

[3] ¶Reference may be made here to H. GRABKE, Size of visual image together with binocular threshold space perception. *Arch. f. d. ges. Psychol.*, 47 (1924), 237-300. (J. P. C. S.)

[4] *La perception visuelle d'espace.* 1902. p. 237.

size due to variations of distance could be prevented. The object consisted of points that were illuminated for a short time.

The results obtained by this method are given in the following table.

Relative Estimate of Distance
(Point of fixation one metre away)

Distance of movable point	Relative Parallax	Answers:		
		Correct	Doubtful	Wrong
1.08	16′	5	14	1
1.12	24′	12	4	4
1.16	32′	10	7	3
1.20	38′	17	3	0
1.24	44′	19	1	0
1.28	50′	20	0	0
1.32	54′	20	0	0

The results were found to be much better when the two points were exposed not simply once but several times in succession; as is shown in the following table.

Distances in Metres	Answers:		
	Correct	Doubtful	Wrong
25—15	1	16	3
25—14	5	8	7
25—13	10	7	3
25—12	12	8	0
25—11	15	2	3
25—10	17	2	1

If the change of convergence that can be detected is expressed in angular measure, the results substantiate those obtained by WUNDT; that is, when the convergence is slight (or the distances large), smaller variations of convergence are perceptible than when the convergence is considerable. Thus, when the distance of the fixed point is 10 m, 2 m or 1 m, the minimum perceptible rotation of one eye by itself was calculated by BOURDON to be 7′, 19′ or 25′, respectively. These values are appreciably larger than those calculated from WUNDT's data as cited by HELMHOLTZ in the text; as might be expected from the difference in the method of the experiment.

8. The question was raised on p. 323 as to the apparent depth-configuration of points situated on the longitudinal horopter (*Langshoropter*), especially as to whether or not, as HERING assumed, such points appear to lie in a frontal plane perpendicular to the line of sight.

The entire basis of this discussion has been changed by questioning the validity of the fundamental assumption of the older view. Are those points lying in the retinal horizons at equal angular distances from the centres of the retinas really corresponding points? As will be seen in the next chapter, where the theory of the horopter is discussed, HERING's arguments tend to prove that this is not absolutely true. Thus from the familiar optical illusion known as WUNDT's illusion (that is, from the errors made in trying to bisect a horizontal line under monocular observation), the conclusion can be drawn that distances extending outward from the point of fixation are slightly underestimated and those extending inward toward the median plane are slightly overestimated. Another way of expressing it is by saying that of two points lying on the retina at equal distances to the right and left of the centre, the one on the nasal side (corresponding to the outer half of the field of view) has a rather smaller "azimuth value" than the temporal one (corresponding to the median half of the field of view). Starting with this fact, it would seem reasonable to suppose also that objects will appear to be in a plane perpendicular to the line of sight when they lie, not on the mathematical horopter, but on the physiological horopter, which is different from the former owing to the asymmetry mentioned above.

This assumption is supported by the quantitative results obtained by FRANK.[1] He found the results of monocular bisection experiments to be in satisfactory agreement with those that were to be expected from "horopter determinations" in the above sense, that is, from determinations of the locations of objects which appear to lie in a plane perpendicular to the line of sight.

The observations cited by HELMHOLTZ in the text will also have to be interpreted differently if a new assumption is to be made with respect to the horopter or if its shape is to be left undetermined for the present; for under these conditions, of course, no conclusion can be drawn from these observations as to the distance from the observer of points that are in the horopter. These observations will remain, however, opposed to the view which HERING has taken, because according to them it would generally not be possible to associate any fixed relation of depth-localization with definite pairs of points on the two retinas; rather, indeed, additional factors, including especially the distance of the point of fixation, would have to coöperate in a decisive manner to determine the impression of depth that is produced when corresponding retinal images fall on these places. If, as seems advisable for a clear analysis of the problem, the question of the relation of these observa-

[1] PFLÜGERS *Archiv.* CIX. p. 63. 1905.

tions to the horopter is disregarded entirely for the time being, then the most important and also the most interesting question in connection with depth-localization is whether, under all circumstances, and especially when the distance is different, definite pairs of points on the two retinas have the property of producing the impression of an object lying in that plane. This is exactly what HELMHOLTZ denied, since his observations led him to conclude that in this case it was a question of the absolute impression of distance, and that when the objects continued to be arranged in the same way their relative configuration as to depth could also be modified under the influence of the apparent distance. These relations have recently been examined by HILLEBRAND,[1] the result being that he reached the opposite conclusion. He states that fixed definite pairs of points give the impression of an object lying in the so-called "*Kernfläche*"; and that in particular this relation is independent of the degree of convergence of the eyes or of the distance of the observed objects. HILLEBRAND called this condition the "stability of the space-values."

Whether these observations really settle the question for all time seems doubtful to me, because, aside from other reasons, in cases of this kind the possibility of individual differences must certainly be taken into account.[2] It would hardly be possible to say, therefore, whether the explanation of the discrepancies which HELMHOLTZ obtained is to be found in the conditions announced by HILLEBRAND.

Some experiments of TSCHERMAK and KIRIBUCHI[3] should also be mentioned here. It is true that they themselves state that the purpose of their investigation was to determine the longitudinal horopter. However, in this determination they proceeded on HERING's assumption, which has just been discussed, namely, that points lying in the longitudinal horopter are localized as lying in a plane normal to the

[1] HILLEBRAND, *Zeitschrift für Psychologie*. V. p. 1. 1893.

[2] HILLEBRAND adjusted the vertical cords so that for definite conditions of convergence and of apparent distance they appeared to lie in a frontal plane. On changing these conditions, he found that they still appeared to lie in a frontal plane. It would undoubtedly have been more convincing to have repeated the adjustment under the new conditions and to have shown that the results were in agreement within the limits of error. The observations were restricted too to relatively small excentricities. Experiments made very recently in my laboratory by Dr. v. LIBERMANN show indeed that HILDEBRAND's rule does not apply to him. He adjusted objects lying over to one side or the other so that they appeared to be situated in the frontal plane passing through the point of fixation. In agreement with the earlier investigators, he found in this case that the necessary configurations differ somewhat from the mathematical horopter. However, these deviations (or the parallaxes necessary to locate the objects in the frontal plane) were not constant, but showed a definite and regular variation depending on the absolute distance of the point of fixation.

[3] TSCHERMAK and KIRIBUCHI, PFLÜGERS *Archiv*. LXXXI. 1900. p. 328.

line of sight. Accordingly, what they really determined was not the horopter, but rather the position that lines must have in order to appear to lie in such a plane. In so doing they found there was a difference, depending on whether the observations were made on vertical cords that were permanently in sight or on falling marbles. And as a matter of fact the falling marbles gave a curve that was more concave towards the observer than the curve given by the cords. The experiments show that the depth-localization (that is, the apparent location of the observed objects in a plane normal to the line of sight) depends on the kind of objects that are used. It would seem to me that this admits of only one conclusion, namely, that the very assumption which is under discussion here, that is, that the object whose images lie on pairs of corresponding points is localized on a fixed surface (*Kernfläche*), is proved to be *not generally applicable*.[1]

9. A series of determinations of the limiting (or threshold) values of the various factors that are concerned in the perception of depth have already been given either in the text or in the notes.[2] That is, the minimum perceptible differences of depth have been measured when one of these factors is involved all by itself, all the other factors having been carefully eliminated. Determinations of this sort have been given in the case of accommodation, convergence, and binocular parallax. The other important factors (aerial perspective, apparent size, etc.) which contribute to the impression of distance are so manifold in their nature and so hard to keep constant that perhaps it would be out of the question to attempt to make similar determinations for them. According to what we know about other domains of the senses, and as to the sense of sight also in regard to localization of direction

[1] The authors do not draw this conclusion. What they do rather is to take HERING's assumption as an absolutely established fact, and thus it is necessary for them to assume a double horopter, (a vertical and horizontal horopter). Still it is certainly not easy to see what such a double horopter really means, and it is even more difficult to see how it is compatible with HERING's ideas as to the nature and significance of the horopter. And we are bound to insist that the results of the experiments by no means necessitate this interpretation. It is possible, and indeed it is much simpler, to suppose, as was suggested in the text, that there are some conditions in which the points lying in the horopter are not localized in the frontal plane.

I think it is to be regretted that the authors speak of observations on depth-localization as horopter determinations, thereby describing their experimental results, not according to what they observed directly, but rather according to what they would mean on the basis of an assumption that is doubtful to say the least. This makes it more difficult to ascertain the true conditions and leads to misconceptions. In order to make a clear distinction between these questions, it should be kept steadily in mind (let us insist on this once more), that the observations of TSCHERMAK and KIRIBUCHI are determinations of *depth-localization*, and not horopter determinations in the usual sense of the word.

[2] ¶See references in the text to this Note 9, on pages 203 and 330. (J.P.C.S.)

and the visible configuration in the field of view, we may inquire further also as to the magnitude of the *just perceptible* impression of distance under certain specified conditions, and as to the accuracy with which such impressions can be compared, perhaps too as to the systematic errors that are made in this comparison. In a single word we may investigate the *eye estimate* of depth dimensions. As may readily be supposed, accurate quantitative determinations of such estimates by the eye are not possible except to a very limited extent. For, evidently, these determinations will involve not only the so-called empirical factors, such as aerial perspective, the form of contours, etc. but also factors like convergence and accommodation which in themselves cannot be measured with any great accuracy. This leaves the binocular perception of distance as the only field suited to quantitative investigation. Here the binocular parallaxes constitute something that is precisely defined and capable of quantitative determination as a basis of the perception of depth; and the same methods that are used in dealing with other questions relating to the physiology of the senses may be employed to investigate the way in which the impressions of distances are dependent on the parallax. A few very simple considerations are obvious at once. The absolute parallax of a single object is of very little consequence so far as giving us an idea of its distance is concerned, as experience with convergence has shown. The things that are really involved in the binocular perception of depth are the relative parallaxes, that is, the difference between the parallaxes obtained in observing a number of points or a more or less complicated object. On account of the dominating importance of the point of fixation generally, it may be surmised that it will turn out to be the difference of depth between an object and the point of fixation that will be determined in a relatively simple manner by the conditions of binocular vision. The quantity that has been defined as the *instantaneous parallax* of a point will evidently be a measure of this difference of depth between the point in question and the point of fixation. If we adopt HERING's ideas of the matter, we can be still more specific and may conjecture that here it is a question of the so-called *cross-disparities*, which, as we saw above, are closely related to the instantaneous parallaxes. The problem therefore is to determine the relative depth-impression (apparent distance in the line of sight from the point of fixation) due to a definite instantaneous parallax (cross-disparity) or to determine the functional relation that exists between instantaneous parallax (cross-disparity) and impression of depth. The first thing that must be determined here is whether a definite instantaneous parallax always produces the impression of a depth-distance of the same amount. And it is obvious immediately

that in all probability there is no such relationship as this. For we saw above that for two points separated by a definite interval of depth the relative parallax diminishes as the average distance of the two points from the observer is increased.[1] Thus a definite relative parallax between two objects may imply very unequal differences of depth, depending on the absolute distance from the observer. This is sufficient to show that if there were a perfectly fixed quantitative connection between instantaneous parallax and impression of depth, such that for a given value of the parallax the depth-interval measured from the point of fixation would be perfectly definite and always have the same value, such a relation would lead to the grossest kinds of illusions. We would vastly underestimate intervals of depth in the case of more remote bodies and overestimate them in the case of nearer objects. That this is not the case, seems to be proved by ordinary experience. Of course, the objection can be raised that under ordinary conditions binocular perception of depth does not function alone, but is aided by various other circumstances, precisely those so-called empirical factors. For this reason it seemed worth while to exclude all other aids, just as was done in the case of visibility determinations, and examine depth-differences that can barely be perceived under conditions which involved binocular parallax only. Experiments of this sort have been performed by ISSEL.[2] He tried first to adjust a rod, which could be moved in the median plane, so that it was exactly midway between two frontal planes whose positions were indicated by other rods. If the position of the movable rod had been governed by equal binocular parallaxes, a large error would have been made; the rod would have been placed too close to the nearer plane, that is, in *front* of the true middle plane. But this was by no means the case; the adjustments of the rod were, on the average, nearly correct, with a very small error of the opposite sign to that mentioned above. The other problem he undertook was to set a rod exactly as far behind (or in front of) a *distant* frontal plane as another rod was in front of (or behind) a *nearer* frontal plane. In this case, therefore, the depth-distances that were compared were not adjacent, as they were at first, but were separated by a considerable distance. Here also distances were judged equal for which the parallaxes were not equal by any means. Unquestionably, therefore, these experiments tend to confirm

[1] The relative parallax of a pair of points which were in the horizontal plane and not far from the median plane, and which were separated by a slight interval of depth, was found above to be equal to $2a\delta/E^2$; that is it was inversely proportional to the square of the distance of the observer.

[2] ISSEL, *Messende Versuche über binokulare Entfernungswahrnehmung.* Dissert. Freiburg 1907.

what we might have been naturally led to anticipate from a utilitarian standpoint: namely, that in general the question as to how much the depth-impression depends on binocular parallax cannot be settled simply by establishing a definite and perfectly valid functional relationship, because some other variable is involved also. The depth-impression produced in us by a definite instantaneous parallax (cross-disparity) does not depend simply on its amount, but it is co-determined always in a very decided way by the absolute distance of the point of fixation as we see it.

If the attempt were made to establish a definite law on this basis for the depth-impressions obtained under any conditions of binocular observation, the most logical procedure would be to assume that the relations were connected in some such way as not to afford any opportunity of serious and systematic illusions. This would be the case provided that, when the point of fixation was at some definite apparent distance E, the instantaneous parallax of another point would produce that particular depth-impression for which the point would have this parallax, provided the point of fixation were actually at the distance denoted by E.

Under these circumstances, when the idea that was formed about the distance of the point of fixation was approximately correct, there would always be at the same time an approximately correct binocular perception of the depth-relations.

This rule would imply on the one hand a definite functional relation between the apparent distance of the point of fixation and the depth-impression corresponding to any parallax; but it would mean also at the same time that, for a specified (apparent) distance of the point of fixation, a definite, though perhaps not very simple, mathematical connection must exist between the parallaxes of different points and the depth-impressions caused by them.[1] A binocular depth-perception governed quantitatively by this law may be called a relatively correct or more briefly a *proportionate* depth-perception. Not only now but quite often hereafter we shall have occasion to refer to this conception,

[1] Let E denote the distance of the point of fixation, and let p denote the instantaneous parallax of a point which is at a depth t behind or in front of the point of fixation; then

$$p = \frac{2at}{E(E+t)}$$

or

$$t = \frac{pE^2}{2a - pE}.$$

The relation between the observed depth distances and the corresponding parallaxes would therefore have to be also similar to this.

and as a name is needed for it, I shall be careful always to use the term which I have just proposed. The essential criterion of a proportionate depth-impression may be briefly stated once more: The characteristic thing about it is that, if F denotes the real distance of the point of fixation and E the real distance of another point, and if F' and E' denote their apparent distances, respectively, then the relative parallax between F' and E' must be the same as the relative parallax between F and E. Under the conditions specified above, when the absolute parallax of a point can be expressed by $2a/E$, the rule therefore is simply that

$$\frac{1}{F} - \frac{1}{E} = \frac{1}{F'} - \frac{1}{E'}.$$

If a large number of points is observed, any one of which may be the point of fixation without changing the apparent distances, the perception will be a proportionate perception, if for any pair of points the relative parallax of their *apparent* distances is equal to that of their *real* distances. Expressed in terms of a notation analogous to that above, this condition would be written as follows:

$$\frac{1}{E_n} - \frac{1}{E_m} = \frac{1}{E_n'} - \frac{1}{E_m'}.$$

The assumption of a proportionate binocular depth-perception is certainly, as has been stated, the most obvious assumption and will serve as the starting point for further tests. It is true, it can only be regarded as an approximation, as is self-evident, because the subjective values of the perceived depths cannot generally be determined with extreme accuracy. Attention must also be called to the fact that an accurate test is very difficult, because the apparent distance of the point of fixation can never be certainly and exactly told. Accordingly, the observational data by which the assumption may be tested are not only limited, but frequently difficult to interpret correctly.

The first group of observations which may be mentioned in this connection is that in which depth-distances that are perceived binocularly are compared with steps which are measured off parallel to the frontal plane (horizontal or vertical distances). Whether objects appear in their natural geometrical form or distorted (reduced or exaggerated relief), depends on the correct or incorrect estimate of this comparison. HEINE's observations[1] belong in this category.

He adjusted three vertical rods so that they appeared to form the edges of an equilateral triangular prism. He found that, when the

[1] HEINE, *Archiv f. Ophth.* LI. 1900. p. 563.

rods actually did make this figure (objectively), they gave the appearance of it only within a moderate range. Beyond this range such rods apparently had too small a depth, and at less than this range too great a depth, so that the arrangement of the rods must deviate from the true figure in opposite senses on either side of this range in order to appear to form an equilateral triangle. However, HEINE insists that accessory circumstances, such as the illumination, have a considerable influence on these relations, and hence he is inclined to assume that objects outside of this range are seen *in toto* at incorrect distances, and that this is the cause of the incorrect impression of depth or of shape.

Certain illusions of binocular depth-perception have also been observed by ELSCHNIG,[1] which apparently were not in complete accord with the rule stated above. However, I am of the opinion that peculiar conditions existed just here in these experiments, and that for that reason we are not justified in concluding that they tend to discredit the law. Any correct binocular depth-impression must under all circumstances be connected with the condition that the retinal images, which are fused in a unitary impression, and whose cross-disparity therefore determines the depth-impression, are really images of the same external point. The moment we impose conditions in which this is not the case, the door is thrown wide open for the most manifold illusions. If, for example, as in the case of the well known experiment with wall paper patterns, two different parts of the pattern are fused binocularly, a spot that happens to be on the paper will be seen with considerable cross-disparity, and so far as this results in a binocular depth-impression for this spot, it will appear to be at a certain distance from the surface of the paper; being either in *front* of it, in case the lines of fixation meet behind the plane of the paper, or *behind* it in the opposite case.

ELSCHNIG's observations are concerned with the appearance of *spheres*; and he noticed, indeed, first, in the case of stereoscopic fusion of photographic pictures, but also by direct observation of the spheres, that the impression he got was not that of a sphere, but of an egg-shaped form elongated in the sagittal direction toward the observer. No one can help noticing that in looking at round bodies the conditions are quite peculiar. Both eyes see the sphere outlined by a circle, and the natural assumption is that these two contours are fused binocularly. And yet they do not correspond to the same circles on the surface of the sphere, but to two different ones separated from each other by a certain interval. If, as was the case in ELSCHNIG's experiments, the pole toward the observer was marked, then the difference in the position of this mark in the two retinal images will be much greater in the case of these two contour circles than it would be in the case of one and the same circle of equal size, supposing it were visible all over to both eyes at the same time. And if each eye considers one point of the contour which it sees, the pole of the sphere will be seen under a much greater cross-disparity than when both eyes are fixated on the same point on the surface of the sphere. Thus it seems to be very clear why an excessive depth should be perceived in this case.

Another point that will certainly have to be discussed more fully hereafter is that the observation of stereoscopic photographs introduces many other conditions into the discussion. And even in the direct observation of real

[1] *Archiv f. Ophth.* LII, 1901, p. 294 and LIV, 1902, p. 111.

spheres, an illusion as to the absolute distance might cause an incorrect perception of form. But as the illusions above mentioned were obtained under the very conditions in which the probability was that that kind of disturbance was eliminated, perhaps we are right in conjecturing that the real explanation of it is to be found in the circumstances above mentioned.

Another observation of Elschnig's is in harmony with this explanation. He found that if the relief was reduced by shortening the base-line for the two stereoscopic photographs, he sometimes got the impression of a body flattened near the pole, but still always elongated out towards the contour line.

At any rate it can be stated that round objects, which present a contour depending simply on the conditions of visibility, are not suitable objects for testing the perception of depth, on account of the peculiarities discussed above. Observations of this kind do not justify us, therefore, in concluding that the rule given above, which states that the binocular perceptions give impressions of depth that are approximately in conformity with the real facts, is not perfectly general.

In the second place, the ability of the eye to estimate depths can be tested by a method which consists simply in comparing two different intervals of depth. Although such experiments would seem to be very easy to make, they involve likewise a series of conditions, which render it difficult to interpret the results exactly, especially when the latter indicate some illusions of the eyesight. It will be advantageous to discuss these conditions at the outset. The first thing to be noted is that in this case also a proportionate depth-perception may be an objectively incorrect one owing to illusions with respect to the absolute distance. Thus, for instance, points that lie at equal distances from the point of fixation, in front of it and beyond it, have a positive and a negative parallax which are by no means in a constant ratio to each other; the ratio varies with the distance of the point of fixation. Hence, whenever an error is made in trying to make two such distances equal, we must always take into account the possibility of an illusion as to the distance of the point of fixation itself. Then it must be noted that if such experiments are carried out in the usual way with the eyes free to move, any particular interval may be perceived in many different ways. At all events it is not at all obvious that the relative dimensions will always have to remain the same independently of this arbitrary factor. We cannot even be sure that a point A, on being fixated, will be seen at just the same distance it was supposed to have before when the point of fixation was elsewhere (at B, say), and A's distance was estimated by its cross-disparity and the distance of the point of fixation B. For this very reason, under certain conditions, a variable appearance of depth might be produced when the fixation was varied. Obviously, all these factors tend to make it very difficult to give a definite interpretation for any illusions of the eyesight that may occur. A more detailed discussion of these relations is impossible at present on account of lack of sufficient observational data.

In his observations AAL[1] endeavoured to obtain as simple and definite experimental conditions as possible. Keeping his eye exactly riveted on one point, he tried to adjust two contiguous intervals of depth, both in the median plane one behind the other, so that they would be equal in extent. The conclusion he reached was that in general "the subjective estimate fluctuates around a value which is equal to the objectively correct distance."

In ISSEL's experiments, already referred to, free movements of the eye were permitted; and while they have made us understand certain regular illusions of the eyesight, still these latter are relatively small in amount. Especially where it is a question of comparing two intervals of depth, one near and the other far, the supposition certainly cannot be disproved, that what is involved here are illusions as to the distance of the instantaneous point of fixation.

In HILLEBRAND's experiments[2] the eyes were also allowed to move. In them a large number of objects were arranged one behind the other according to a definite scheme, the basis of which was not depth-difference directly, but apparent absolute size. These experiments will be discussed in detail later, but it must be stated here that under certain assumptions, which will also be considered later, the results actually seem to indicate a relatively correct or proportionate depth-localization.

Considering everything, all that can be said at present in my opinion is that the theory (repeatedly mentioned above), by which the connection between the depth-impressions, on the one hand, and the apparent distance of the point of fixation and the instantaneous parallaxes (cross-disparities), on the other hand, would be one that corresponds very closely with the objective relations, is compatible with the facts known at present. No exceptions to this statement have been definitely established. It appears therefore to be the best starting point and basis in considering problems of this nature, although it has not been proved absolutely and may not even be capable of proof.

A relationship which is different from that which is objectively correct has been brought into the discussion by STERNBECK[3] merely as a conjecture. His theory requires that the minimum perceptible increment of apparent distance, that is, the increment corresponding to a just perceptible parallax, shall always be proportional to the apparent distance to which it is added. This assumption seems plausible to STERNBECK on account of its analogy to FECHNER's psycho-physical law. No observations have ever been made which

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[1] *Zeitschr. f. Psychologie.* 1. Abt. XLIX. p. 197.

[2] *Denkschriften der Wiener Akademie.* Math. naturw. Kl. LXXII. 1902.

[3] *Der Sehraum auf Grund der Erfahrung.* Leipzig 1907.

would tend to indicate that this sort of relation is more likely than the assumption which we have made above. Besides, it seems idle to me to try to discover some analogy here with FECHNER's law.

The truth is, in the development of that law, the idea was entirely opposite to STERNBECK's theory, because, according to FECHNER's law, the minimum perceptible differences are rated as being equal to one another everywhere. Now it has been shown beyond doubt that in very many cases this assumption is not at all true, and there can certainly be no question of its not being true in the case of intervals of depth; which is the point under discussion at present. On the other hand, a rule which implied that the minimum perceptible differences were rated in proportion to the quantity to which they were added, (which, as far as WEBER's law is concerned, would lead to an objectively correct valuation) would likewise surely not be of general applicability (consider simply estimates of minimum perceptible differences in brightness).

I must take occasion to say here that in my opinion, when STERNBECK (*loc. cit.*) says that in any case he is unable to accept HELMHOLTZ's view, that the minimum perceptible intervals of depth have the same values everywhere, he must have been labouring under some misunderstanding or mistake. HELMHOLTZ certainly did not entertain this opinion. He even stated explicitly that the analogous assumption in the case of frontal distances was untenable.

Undoubtedly, there is a close connection between estimates of *distance* by the eye and estimates of the *absolute size* of an observed body. If a retinal image of definite size is produced by some object, or if a certain angle is subtended at the eye, the object will generally give the impression of being larger when we see it from a greater distance than it does when we see it nearer by, no matter how we happen to obtain this impression of distance. There can be no doubt that some relation exists between impressions of distance and absolute size that corresponds in general to actual conditions. Hence errors in estimating size will frequently be traceable to certain conditions determining the impression of distance and to the illusions as to distance which are caused by these conditions.[1]

However, although there can be no doubt of the existence of this connection, it is well to say at the outset that we must not get the idea that it is too simple or too rigid. In particular, it would be wrong to suppose that it was obvious that this relation corresponds rigourously to the objective mathematical relations between distance, absolute size, and visual angle. For instance, if one of two objects which subtend the same visual angle appears to be twice as far away as the

[1] ¶Some more recent literature on apparent size is as follows:

W. BLUMENFELD, Untersuchungen über die scheinbare Grösse im Sehraume. *Zft. f. Psychol.*, 65 (1913), 241-404.—F. HEFFTNER, Objektgrösse und Gesichtfeld. *Arch. f. Ophthalm.*, 89 (1914), 186-196.—K. HOROVITZ, Grossenwahrnehmung und Sehraumrelief. PFLÜGERS *Arch.* 194 (1922), 629-646.—N. BERNSTEIN, Perception of size. *Zft. f. Psychol., Neurol. u. Psychiat.*, 1 (1922), 21-54.—G. MARZYNSKI, Sehgrösse und Gesichtfeld. *Psychol. Forsch*, 1 (1922), 319-332.—H. N. RANDLE, Sense-data and sensible appearances in size-distance perception. *Mind*, 31 (1922), 284-306.—(J.P.C.S.)

other, it is not necessarily true that it will also appear twice as large as the other, or that half of it will appear the same size as the other. In accordance with a statement which has been made before, and which will be expanded in more detail hereafter, we must insist rather that the physiological processes that are responsible for these immediate impressions do not by any means have to correspond accurately to relations objectively present, of which we may be intellectually aware.

That, as a matter of fact, they do not always correspond, is evident from the next class of phenomena which will be discussed here. When we gaze at any object with one eye screened, it is easy to notice that its apparent size varies with the state of accommodation. Every exertion of accommodation is accompanied by an apparent reduction in size, and every relaxation by an apparent magnification. The simplest explanation of this well-known and easily observed phenomenon would be to suppose that as the result of accommodation for near vision the object appears to be at the distance that ordinarily corresponds to this accommodation, that is, too near; and hence, as long as it subtended the same visual angle, its absolute size would appear to be less than it was. The trouble about this explanation is that as, DONDERS[1] long ago rightly pointed out as something remarkable, this is not what actually happens. The object does appear to become smaller when accommodation is exerted, but it by no means appears to come nearer at the same time; on the contrary, it appears to recede. Thus, with a constant visual angle, we see here apparent size and apparent distance vary in the opposite sense. Theoretically this is certainly a very noteworthy phenomenon, the explanation of which may be due to the fact that the impression of size is in some way directly affected by the physiological processes connected with the exertion of accommodation, but not by obtaining the impression of distance.[2] The question will be discussed later from this point of view.

Two disorders of vision known as *micropsia* and *macropsia*,[3] which have been much studied by ophthalmologists, and which occur under more or less abnormal conditions, are undoubtedly closely connected with the above phenomenon. Micropsia in particular is known to be concomitant with a partial paralysis of accommodation (paresis-

[1] *Archiv f. Ophth.* XVII (2) p. 27. 1871.

[2] Incidentally, of course, it would be hard to tell whether the controlling factor here is the process of accommodation itself or the effort of convergence that regularly accompanies it.

[3] ¶Pathological conditions in which objects appear to be unnaturally small (micropsia), or unnaturally large (macropsia).—(J. P. C. S.)

micropsia); whereas when the power of accommodation is completely gone, the phenomenon is said to be absent (Koster[1]). According to Schirmer,[2] micropsia is produced by great straining of the accommodation, especially in the case of presbyopes. It is natural to suppose that in such cases also the strain of accommodation, which is particularly great then, is the cause of micropsia.

Some other observations made under special conditions may be explained on the same basis without difficulty; as, for example, the following experiment due to Reddingius.[3]

"On placing a convex lens of 6 dioptries in front of each eye, it is possible for me to find a distance for which my vision through the centres of the glasses is both single and distinct, whether I use one eye or both eyes. Then when one of my eyes is closed, the existing macropsia appears to be very distinctly enhanced, although, of course, this magnification cannot be attributed to any increase of the size of the retinal image."

To understand this experiment, it must be remembered that binocular fixation involves convergence and along with it an exertion of accommodation. Owing to the high power of the positive lens, this accommodation, so far from being necessary for distinct vision in this case, is really a hindrance to it; and so there is no incentive for it in monocular vision. Undoubtedly, therefore, when one eye is covered, it immediately turns outwards and releases the accommodation, thereby, causing an apparent magnification in the well-known way. The exact counterpart of this experiment may be recalled in one of Burow's experiments.[4] He describes the phenomenon that occurs when the hand is removed from in front of one eye. This covered eye had not been adjusted for the same object as the other eye; and so the moment it was uncovered, a movement of convergence occurs, which is accompanied by an apparent reduction in the size of the object observed.

The object of Hillebrand's investigation,[5] which has been briefly mentioned already, was to study the relations involved in the impression of absolute size, when the conditions were such that as far as possible all the so-called empirical factors were eliminated and the impressions of both distance and size were obtained by binocular vision alone. His method was as follows.

Along two parallel horizontal lines extending far away from the observer a large number of vertical rods were placed, being arranged in pairs opposite one another, with equal spaces between them, like trees along an avenue. Thus each pair of rods was farther from the observer than the preceding one. The two rods in each pair were both in the same frontal plane, with the same gap between them in every instance, so that the "avenue" appeared to be united at its far end. Hillebrand undertook to modify the arrangement of the rods, so

[1] Koster, *Archiv f. Ophth.* XLII (3). p. 134. 1896.

[2] Schirmer, *Realenzyklopädie der ges. Heilkunde.* XII. p. 486.

[3] Reddingius, *Das sensomotorische Sehwerkzeug.* Leipzig 1898. p. 122.

[4] Burow, *Archiv f. Ophth.* XIII (2) 1867. p. 327.

[5] *Denkschriften der Wiener Akademie. Math.-naturw. Kl.* 72. 1902.

that this convergence of the two rows would disappear, and the distance between one pair of opposite rods would be apparently the same as that between any other pair. Obviously, under such circumstances, the rods could not continue to lie in straight parallel rows, but had to form curved lines becoming more and more widely separated as they extended farther and farther from the observer. The empirical curves thus obtained were called "avenue curves" by HILLEBRAND. The result, which he obtained over and over again with remarkable regularity, was that (for one observer and one set of observations) the difference between the angles subtended by successive pairs of rods was directly proportional to the difference between the binocular parallaxes.

If the value of the visual angle required to give an impression of equal absolute size (HILLEBRAND's angle of width) is denoted by W, and if the binocular parallaxes are denoted by p, then according to the above experiment $W - W_1 = a(p - p_1)$ or $dW = adp$ and $W = c + ap$, where c may have any value different from zero.[1]

As has been stated, the experiments do not enable us to draw a direct conclusion as to the apparent distances. However, it is not without interest to see what these apparent distances would have to be if the simple relation corresponding to the objective conditions existed between them and the impressions of size. Were this the case (as we shall suppose for the moment), the angle of width necessary to produce the impression of equal sizes would have to be inversely proportional to the apparent distances; that is, we ought to have $W = \beta/E'$, where E' denotes the apparent distance, and β denotes a constant which will be referred to again below. Accordingly,

$$E' = \frac{\beta}{W} = \frac{\beta}{c + ap}\,.$$

It will be worth while to consider this formula a little more in detail. Since

$$c + ap = \frac{\beta}{E'},$$

and hence

$$adp = -\beta\frac{dE'}{E'^2}\,,$$

[1] If $c = 0$, then $W = ap$; which would mean that the angle of width was directly proportional to the parallax or inversely proportional to the distances. This would require that the avenue curves should be parallel straight lines, and would mean the *absence of the very illusion* which constituted the starting point of the observations. If it is assumed that the above law holds for all distances, then as the distance increased, the angle of width necessary to produce the impression of the same size would approach a limiting minimum value, which would be given by the constant c.

that is,

$$dE' = -\frac{a}{\beta}E'^2 dp \ ,$$

it is obvious at once that the increase of the apparent distance corresponding to a definite small reduction of the parallax is not constant, but (absolutely in accordance with the actual facts) becomes greater and greater with increase of distance or decrease of parallax, varying indeed as the square of the apparent distance.

In order to determine the constant β, the apparent distance would have to be known for some one pair of rods or for some definite value of p. Of course, it would be impossible to know this with certainty, as the observations permit us to think of all the apparent distances as being increased or decreased in any ratio whatever. Still there is no reason for supposing that the illusions in this case are very glaring. On the contrary, it is likely that in the case of near objects, for which the values of p are considerable, and c is small as compared with αp, the apparent distance is practically the same as the real distance. Now as the real distance is $E = 2a/p$, where $2a$ denotes, as before, the interpupillary distance, we may put the coëfficient of β/α approximately equal to $2a$ and obtain therefore:

$$E' = \frac{2a}{c/\alpha + p} \ .$$

When this value is compared with that of the real distance, we see that for near objects, where p is large in comparison with c/α, the apparent distance is close to the real distance, and begins to lag behind it more and more as the distance of the object is increased. And if we assume that the equation is applicable for all distances, the value of the greatest finite distance at which objects will be seen without any parallax will be given by the expression $2a\alpha/c$.

Another thing to be noted too is that a perception of depth, such as is meant here, is of the kind which we described as being a relatively correct or proportionate perception [p. 383].

For it is obvious at once that the difference between the reciprocals of the *apparent* distance and the *real* distance has the same value always, namely, $c/2a\alpha$; and, consequently, if the apparent distances of any two pairs are denoted by E'_m and E'_n, and the real distances by E_m and E_n, then generally:

$$\frac{1}{E_m'} - \frac{1}{E_n'} = \frac{1}{E_m} - \frac{1}{E_n} \ .$$

This equation is the criterion which we found above for the so-called proportionate depth-perception. Hence, the entire illusion that occurs here in regard to perception of depth may be summed up in the statement, that in the apparent arrangement of the objects all absolute parallaxes are increased by the same amount above their values in the true arrangement.

There is a reasonable probability for inferring that the apparent distances are really approximately related in this manner. Still, it should be repeated that we are not justified in deducing it as an obvious consequence of HILLEBRAND's experiments. Whether an investigation made purposely for comparing the distances would give altogether the same result, no one is in a position to say at present.[1]

Connected with these conditions affecting binocular estimates of depths or differences of depth, there is another question, which, since it is not without a certain theoretical interest, and since too, as we shall see hereafter, it is of some practical importance, must be alluded to briefly. If our depth-impressions (speaking perfectly generally) are determined partly by the conditions of binocular vision and partly by empirical factors of very various sorts, we can try to find out the precise nature of this interaction and how far it goes; and we find that we encounter questions some of which at any rate are by no means easy to answer. One thing we know for certain, the empirical factors are involved whenever the binocular conditions are entirely excluded. This happens in the case of monocular vision and also in the observation of very distant objects whose parallax is not appreciably different from zero. In the observation of near objects the empirical factors are certainly involved to the extent of making it easier for us to get a correct comprehension of complicated figures (it may be perhaps by simply recalling the form of an outline), and so they constitute a part of the conditions on which binocular perception of depth depends. However, the question to be asked now is whether the binocular perceptions of depth can be co-determined *quantitatively*, and ultimately modified, by empirical factors, and if so, to what extent this can go.

In certain cases this possibility will certainly have to be admitted. For instance, in gazing steadily at a body a moderate distance away,

[1] The relations in regard to apparent distance have been derived here from HILLEBRAND's observations on the basis of the assumption concerning the connection between apparent distance and impression of absolute size; which, on the supposition that there is such a connection, and that it is a fixed and general connection, is certainly the only possible assumption. Whether the conclusions which I have deduced are in harmony with HILLEBRAND's own views, I am unable to say, because his own account of them, which was not entirely clear to me throughout, has left me in doubt on this point.

the details of a distant background will be seen beyond the point of fixation mainly on account of the conditions of binocular vision. This does not exclude the action of other agencies, such as aerial perspective, from assisting in giving a more accurate impression of depth. In certain cases, therefore, it cannot be denied that the two sets of conditions, binocular and empirical, assist each other in the manner here implied.

Further light on these relations may be expected by producing conditions such that the empirical factors are decidedly opposed to the depths seen by binocular vision. For instance, this is what happens when photographs are fused stereoscopically which have not been taken properly, the base-line being too short or too long. Experiments made under these conditions show that often the relations of binocular vision predominate to such an extent as to make the object look wrong and deformed in spite of many an empirical aid. Often, however, even such distortions will not be noticed and the objects will be correctly perceived. On the whole, it is fair to assume that there is a co-operation between the empirical factors and binocular vision such as has been indicated, or that at least under certain circumstances the quantitative relations of binocular vision may be modified by those of the empirical factors. But to what extent this occurs, can hardly be estimated at present with any certainty; and, besides, it is probable that individual differences are of much importance here.[1]

10. The principle mentioned on page 356 was first published and used by ROLLMANN. Recently it has found a wide application in those devices which have been advertised by the rather unfortunate names of "anaglyphs," "stereographs," "plastographs," etc. The process employed is opposite in some respects to that described by HELMHOLTZ in the text. The drawings here are made on a white background instead of on a black surface. The principle by which the two eyes are enabled to see different views by using glasses of different colours is just as simple in this case as in the other. If a drawing is made in red lines on white paper, it will be almost invisible as viewed through a red glass, because the background also appears red. The disappearance is more complete, the more nearly alike the two colours are.[2] By trying a few different glasses it is easy to select one such that,

[1] ¶The following references may be inserted here: E. LAU, Neue Untersuchungen über das Tiefen- und Ebenesehen. *Zft. f. Sinnesphysiol.*, 53 (1921), 1-35.—A. FRUBÖSE and P. A. JAENSCH, Der Einfluss verschiedener Faktoren auf die Tiefensehschärfe. *Zft. f. Biol.*, 78 (1923), 119-132. (J. P. C. S.)

[2] It is easy, but not of particular interest, to formulate the precise conditions necessary to produce a complete disappearance.

though the red drawing is still visible when observed by itself, it is not pronounced enough to have any appreciable effect on the total impression when both drawings are seen at the same time. On the other hand, the red drawing will be plainly visible through a green glass. The background now appears green and the drawing almost black, since the light reflected by the red ink will be mostly absorbed by the green glass. Similarly, a green drawing on a white background may be plainly perceived through a red glass, whereas it will be nearly imperceptible through a green glass. Accordingly, if we wish to contrive that the same drawing shall present a different picture to each eye, then supposing a red glass is in front of the right eye and a green glass is in front of the left eye, the part of the picture intended for the right eye must be executed in green, and the part intended for the left eye must be executed in red, while the part intended for both eyes must be in black. Accordingly, in these pictures near objects are represented by dark surfaces with a green border on the left and a red one on the right. For distant objects it is just the reverse.[1]

Apart from its simplicity, the chief advantage of this process over all those where separate pictures are used is that the actual fusion of the drawings greatly facilitates the binocular fixation of corresponding points; and for this reason persons who have never had any training or individuals who for some cause or other have trouble with binocular vision usually succeed in obtaining stereoscopic fusion better and more easily by this method than by the others. Another advantage is that the method is suitable for purposes of objective demonstration, because pictures such as those described can be projected on a screen and viewed simultaneously by a large number of spectators each of whom is furnished with a pair of spectacles containing a glass [or piece of celluloid] of the right colour for each eye. Pictures suitable for projection were first made by the ZEISS firm at HERING's suggestion.[2] For some years past very satisfactory pictures of this kind have been supplied at a moderate price by the firm of SKLADANOWSKI.[3]

11. As a kind of supplement, something should be added here as to the way in which binocular perception of depth is dependent on various special conditions. In the first place it may be expressly stated that the binocular perception of depth is not at all connected with

[1] Incidentally, the opposite method of procedure, as given by HELMHOLTZ in the main text (coloured diagrams on a black background) is not the process which was used by ROLLMANN, who invented the whole affair. He used a white background, as described above. HELMHOLTZ's description is based on a mistake as to this particular point.

[2] PFLÜGERS *Archiv.* LXXXVII. 1901. p. 229.

[3] SKLADANOWSKI, *Plastische Weltbilder.* Berlin 1903.

a fusion of the two images in each eye into a unitary impression. This impression can be obtained very well even when the separate images are clearly perceived. The truth of this statement, which was made by HELMHOLTZ,[1] can be regarded as established by data he presents. Similar observations have since been made over and over again. In the report of the experiments which were made by F. AUERBACH and myself[2] on the time required for the binocular perception of depth differences, it was expressly stated (p. 344) that the electric spark flashing out either in front of the point of fixation or beyond it was seen distinctly double, but could be localized as to distance with perfect certainty and accuracy. This well-known fact has since been verified again by TSCHERMAK and HÖFER,[3] who proved also that localization of depth on the basis of double images was a method of no little refinement. In their experiments a movable needle had to be placed at the same distance from the point of fixation as a reference needle. When the point of fixation was two metres from the observer, and the reference needle from 40 to 80 cm away, the movable one could be placed at the same distance with errors amounting to only a few centimetres.

It may be considered as self-evident that, as is the case everywhere, the occurrence of the depth-impression is connected with particular conditions on which the coöperation of two perfectly definite retinal areas depends, so far as this effect is concerned. These conditions (which may easily include something more than just a certain similarity between the two images) will always be fulfilled in the observation of ordinary solid objects or of objects at different distances. On the other hand, if pictures differing in any desired manner are presented to the eyes by an instrument on the order of the stereoscope, then, as in stereoscopic vision generally, the production of the impression of depth from these separate pictures will depend on various peculiar circumstances that cannot be exactly defined. Experiments in this field have been made by HEINE.[4] There can be no doubt as to the fact (which he found also) that the fusion of the two pictures is more difficult, the farther they are apart. In TSCHERMAK's experiment

[1] The matter referred to here is the same fact mentioned by HELMHOLTZ further on from a somewhat different point of view. There he happens to be considering the distance at which the half-images are seen in case of distinct diplopia, and it is expressly stated that as a rule they appear at the correct distance of the object in question. Evidently, this is substantially the same as the statement made here, although the latter, having reference to the conditions of binocular perception of depth, is formulated a little differently.

[2] *Archiv für Physiologie*. 1877. p. 297.

[3] TSCHERMAK and HOFER, Über binokulare Tiefenwahrnehmung auf Grund von Doppelbildern. PFLÜGERS *Archiv*. XCVIII. 1903. p. 299.

[4] HEINE, PFLÜGERS *Archiv*. CIV. 1904. p. 316.

mentioned above the distance between the two images was always small; even in the most extreme case it was less than two degrees, as may be computed from his data.

W. A. NAGEL[1] tried to find whether the power of binocular perception of depth is dependent on the degrees of brightness that enable us to distinguish colours or whether it exists at twilight intensities. This question is of particular importance in connection with the duplicity theory, according to which only a certain part of the organ of vision operates at intensities that are below the colour threshold. The investigation showed that even under the conditions of twilight vision binocular perception of depth was possible, the accuracy of it corresponding to the visual acuity under these conditions, which indeed is very much below that of daylight vision.

GUILLOZ[2] succeeded in obtaining stereoscopic effects by fusion of after-images.

EWALD[3] has described a beautiful experiment in which a stereoscopic effect is produced through the instrumentality of memory images. By means of a rotary mechanism the pictures, which were to be stereoscopically fused, were exposed alternately to each eye. In order to prevent confusion from after-images, instead of darkening the field of view of the eye which was not observing a picture, there was a bright spot in it sufficient to overpower or erase the after-image. It was possible even in this way to get a correct stereoscopic perception.

Lastly, it should be added that EWALD succeeded also in producing pseudoscopic effects of far larger range than any obtained before. HELMHOLTZ states that a reversal of relief never succeeds except in the case of objects which after being transformed in this way have an appearance that at least does not entirely transcend all our experiences, in other words, there must be some plausibility about the transformation. By systematic training, as EWALD found in his own case, we can learn to fuse other stereoscopic pictures too and get a reversal of the relief; and sometimes the figures will be very weird and uncanny. Gradually more and more proficiency and confidence can be gained in this direction.[4]

[1] NAGEL, Stereoskopie und Tiefewahrnehmung im Dämmerungssehen. *Zeitschr. für Psychol.* XXVII.

[2] GUILLOZ, Sur la stéréoscopie obtenue par les visions consécutives d'images monoculaires. *C. R. de la société de bilol.* 1904. p. 1053.

[3] EWALD, PFLÜGERS *Archiv.* CXV. 1906. p. 514.

[4] ¶The following reference may be inserted at the end of this Note: P. ZIMMERMAN, Über die Abhängigkeit des Tiefeneindrucks von der Deuchtlichkeit der Konturen. *Zft. f. Psychol.*, 78 (1917), 273-316. (J. P. C. S.)

12. *Supplementary Note.*[1]—Certain phenomena which may be observed by watching an object moving laterally (transversely) in the field of view are of much interest in connection with the question of binocular perception of depth. Under such circumstances, if the light which comes to one eye differs in intensity or in colour from that which comes to the other eye, the temporal relations for starting the process of stimulation and evoking the sensation will not be the same in both eyes. The result will be that the images in the two eyes will be shifted to the right and left with respect to each other, thereby producing binocular parallaxes and impressions of depth. The simplest effect of this kind is produced by causing a rod to oscillate to and fro in a frontal plane, in which case, provided the light coming from it to one eye is more intense than that coming to the other eye, the rod will appear to be moving, not in the frontal plane, but in an elliptical path. Allusion has been made elsewhere (Vol. II, p. 422) to these phenomena especially with reference to their application to heterochromatic photometry. The important thing to be noted at present is that, as is a consequence of these conditions, the impressions of depth are determined in a perfectly precise way by the relation that exists *at exactly the same moment* between the sensations in the two eyes.

Another very beautiful phenomenon, which is likewise concerned with the binocular perception of depth in the case of objects in motion, has been described quite recently by A. v. Szily.[2] A strip of white paper is fastened to the upper end of a pendulum, so that it stands vertical and is bisected by the axis of the pendulum, when the latter is in the position of equilibrium. When the pendulum is swinging to and fro, the piece of paper executes periodic rotations, first one way and then the other. The movements are observed in front of a dark background. Now place a right-angle reflecting prism in front of one eye, which has the effect of reversing right and left for this eye; then with proper amplitudes of vibration, we get the impression that the strip of paper is being turned in the median plane around a frontal axis, its upper end being apparently inclined alternately, first towards, and then away from the observer. The explanation of this phenomenon follows at once from the familiar conditions of binocular perception of depth. Thus suppose the prism happens to be in front of the left eye. Then if the upper end of the paper strip moves to the left, this movement will be seen correctly by the right eye; but instead of being

[1] ¶Written by Professor v. Kries to be inserted here in the English Translation; received by the Editor, January 1924. (J. P. C. S.)

[2] A. v. Szily, Über eine auf der veränderten binokularen Projektion beruhende Sinnestäuschung der Bewegungsrichtung. Pflügers *Archiv. f. d. ges. Physiol.*, 201, pp. 247–249.

a movement to the left, it appears to the other eye to be a movement to the right. And so the result is, there is a cross-disparity of the same sort as would be produced by an object which was closer to the eye. Hence, when the images in the two eyes are fused, the spectator must get the impression that the upper end of the strip is coming toward him. The effect is just the reverse so far as the lower end of the strip is concerned, because it is really moving in the same phase of vibration from left to right, and hence, owing to the perversion produced by the prism in front of the left eye, this end of the strip has a cross-disparity of the opposite sign to that of the upper end. In order to see the effect well, the amplitudes of vibration ought not to be too large; otherwise, we shall get double images of each end of the strip, which, as we know, may not prevent us from having impressions of depth, but still make it harder to have them.

The conditions of the experiment may be varied still more if, instead of observing just a strip of paper vibrating about its centre, we use simply some small object which oscillates as a whole in the field of view, moving periodically, first to the right and then to the left. If an object of this sort is viewed as before with a reversion prism in front of one eye, under proper conditions, as might be anticipated, the same kind of impression will be produced; that is, the object will appear to execute a periodic motion in the median plane, toward the observer at one time and away from him at another. Still it will be noticed that the conditions here are essentially different from what they were in the first form of the experiment. In the latter case the observer fastened his eyes constantly on the centre of the oscillating strip of paper. But in the other case, it is true, the observer may also keep his eyes practically fixed in one position, and if he does so, the conditions will be the same as before. However, he may also follow the apparently opposite movements of the component images in the right and left eyes, by executing periodic ocular movements, first decreasing and then increasing his convergence, in such manner that no double images occur. The trained observer will have little difficulty in making the observations either way, just as he prefers. Provided the occurrence of double images is prevented by moving the eyes as described above, the illusion will be obtained in a very striking manner. The object apparently oscillates in the median plane, toward the observer and then away from him. And under such circumstances the innervation of convergence might be not only a very important factor but presumably a decisive factor in forming impressions as to distance. Of course, we must not suppose that the existence of cross-disparities will not contribute also to these impressions, because, as was pointed out by Helmholtz in his discussion of Wundt's experiments (page

314), the eyes are not able to keep pace exactly with the movements of either the object or its reflex image.

On examining how the impressions made on the two eyes cooperate together, it is interesting to observe also the connection that exists between the phenomena of binocular rivalry on the one hand and those of binocular perception of depth on the other hand. Observations that are of much value from this point of view have been reported by v. SZILY.[1] In these experiments single geometrical figures were described consisting of black and white portions. By a special optical device these areas are viewed in such a way that the black and white places are mutually interchanged in the two eyes, the result being perfectly definite impressions of depth.—K.

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§31. Binocular Double Vision

The phenomena of binocular vision investigated in the preceding pages were simply those indications by which the sense of sight enables us to locate the observed object at some definite place in space. The subjective phenomena manifested in these circumstances have now to be considered.

In the case of monocular vision we have seen already how it was possible not only to get some idea of the distribution of objects in space by noticing the way in which we see them, but also to form an idea of the way in which they are distributed over the surface of the field of view. In binocular vision the objects appear on the visual globe of each eye, but since, as we have seen, the images in the two fields are generally not exactly alike, they cannot be made to coincide perfectly in the common field of view, so that certain outstanding inequalities in the two visual globes will be perceptible. In this chapter we propose to consider those phenomena that are due to the geometrical differences between the images on the two visual globes, reserving for the next chapter on the "Rivalry between the Visual Globes of the Two Eyes" the discussion of all those phenomena that depend on differences of illumination or colouring in the two fields.

It might be well to state that this analytical method of considering the field of view is not the natural mode of perception as it is at first acquired; on the contrary, it is apt to be the result rather of conscious reflection on the peculiarity of our visual impressions. Then we cease to think of the world around us as it really *is*, and begin to consider

[1] A. v. SZILY, Stereoskopische Versuch mit Schattenrissen. PFLÜGERS *Archiv*. 105. p. 964. 1921.

how it *looks* to us from the place where we happen to be for the time being. The thing that interests us then is chiefly the way things look, either because we wish to reproduce them as the artist does or because as physiologists we endeavour to find a theoretical explanation of them.

Beginning, therefore, by examining the binocular field of view as such, we notice first that the configuration of objects in the two component monocular fields is not the same. For instance, when we look through a window at the trees outside, we can trace the foliage a little farther to the right with the left eye than we can do with the right eye. The left eye enables us to see parts of the foliage on the right edge of the window which are not visible to the other eye, because they are hidden from it by the window-frame. And so the window-frame is contiguous to different masses of foliage in the two fields of view.

Similarly, the cross-bar of the window conceals from one eye a part of the scene that is different from that which it conceals from the other eye. Thus, as the gaze is allowed to wander over the foliage, the window-bar will be encountered twice at two different places, each time cutting out some portion of the scene at least. In other words, the window-bar is seen at two places in the field of view at once; that is, it appears *double*.

On the other hand, suppose we look first directly at the window-bar or the panes of glass, and then let the eyes travel over one of the panes until they meet a vertical bar and cross over it to the next pane. Possibly there may be the trunk of a tree which happens to lie alongside and behind the vertical bar on the right in the field of view of the right eye, whereas in the field of view of the left eye it is alongside the bar on the left. Thus, as the series of points on the window are observed in succession, the more remote object will occur twice and will appear double.

In §28 we found that the sequence of points in the field of view may not only be determined by actually passing them in review, but can be judged also by the order in which the images are ranged side by side on the retina. And so we do not even have to let the gaze really wander over the field of view in order to see the double images. We can look steadily at one point and yet be aware of the different configuration of the objects in the two component fields. Thus the same object may appear on opposite sides of the point of fixation, or its distance and direction from that point may be noticeably different in the two fields; in either case we can perceive that the given object is apparently in two different places in the field of view.

The two eyes b_0 and b_1 (Fig. 64) are both supposed to be focused on the point a; and hence this point will be seen single and at its true place in space. The point c is nearer the eyes than a, and so it must appear to the eye b_0 as being to the right of the visual axis b_0a. The same point c as seen by the other eye b_1 appears, however, to be to the left of the point a. And so in the common field of view it is apparently first to the right, and then to the left, of the point a; that is, it appears double, or is seen, as we say in this case, in *heteronymous double images*, which means simply that the image that is apparently over on the right belongs to the left eye, and *vice versa*.

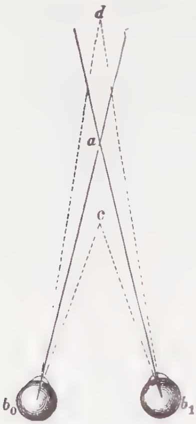

Fig. 64.

It is exactly opposite in the case of the point d which is farther off than the point of fixation at a. As seen by the right eye, this point appears to be to the right of a, and as seen by the left eye, it appears to be to the left of a. Consequently, we say that the point d is seen in *homonymous double images*.

A somewhat different case is exhibited in Fig. 65, where, as before, the two eyes are at b_0 and b_1, and their common point of fixation at a. But now the nearer point c lies outside the angle b_0ab_1; and in this case it will appear to both eyes to be to the left of the point of fixation, since the direction-lines b_0c and b_1c are on the left of b_0a and b_1a, respectively. But the angle cb_0a is much smaller than the angle cb_1a, and hence the angle of separation between c and a is much less in the field of view of the eye b_0 than it is in that of the other eye b_1. If this difference is large enough to be noticeable, the image will again be seen in two different places in the common field of view; that is, it will appear double. But in this case the double images are not so plain as when they were on opposite sides of the point of fixation, as in Fig. 64. Especially when they are farther away from the point of fixation over on one side of the field of view, their distance apart and the contrast between them and their surroundings must be fairly great in order for them to be noticed.

Fig. 65.

They will be somewhat easier to see when there is some sharply outlined object f situated on one side of a about equally far from both eyes and in between the prolongations of the sides of the angle b_0cb_1, so that the double images of the point c in the common field of view

will be on opposite sides of f. The apparent sequence of points in the field of view of the eye b_0 is then acf; while in that of the eye b_1 it is afc. In this case the separation of the two images will be easier to see than when they are viewed against a background of uniform colour and illumination.

Lastly, even when the images of the same point are equally far from the point of fixation in the visual fields of the two eyes, but in such different directions from it as to be noticeable, two images will be seen instead of one. For instance, this is the case when the point c happens to be above or below the point a, and at the same time a little nearer the eyes than the latter.

Thus, in general, objects will be seen double whenever their apparent positions with reference to the point of fixation in the visual fields of the two eyes are so different that this difference can be appreciated by the eyesight. On the other hand, those objects which appear to be situated on the visual globe in the same position with respect to the point of fixation will be seen single.

When an object is seen single by both eyes, the image will be called a *total image (Ganzbild)*. In the case of an object which is not seen single, the two images taken together will be termed a *double image*. The latter consists of two *half-images*.

We must proceed now to investigate more fully what points on the visual globes of the two eyes are apparently in the same position with respect to the point of fixation, so that they coincide with each other in the common field of view. We may refer to them as pairs of *coincident points (Deckpunkte)* or *corresponding points*. In a certain theoretical sense they are sometimes called *identical points* also. Since a certain point on the retina corresponds to each point on each visual globe, we may also speak of *coincident, corresponding* or *identical points on the two retinas*. In the case of a pair of non-corresponding points, we can adopt FECHNER's term and speak of them as *disparate* (or disconnected) points.

1. *In normal eyes the points of fixation on the two visual globes are a pair of corresponding points (Deckpunkte).* The point of fixation in the visual field of each eye corresponds to the place on the retina which is distinguished anatomically from the rest of the retina and which is known as the *fovea centralis* or place of most distinct vision. It is the point in the field of view on which the eyes are focused. Thus the above statement is equivalent to saying that the point we happen to be gazing at in the space in front of our eyes will invariably be seen single, or that the particular point in the object whose images fall on the retina of each eye in the *fovea centralis* will be seen single. The rule

here formulated is verified by all observations of normal eyes. Presently, we shall have occasion to speak of certain cases of squinting in which there are exceptions to this rule.

As soon as we begin to inquire into the reason for this behaviour of the eyes in binocular vision, we become involved in a subject that has been the source of much debate, namely, as to how it is that we can have single vision with two eyes. If the sensations are regarded merely as tokens or symbols, whose interpretations have to be learned, the answer to this question presents no special difficulty. Nearly all external objects affect different nerve-fibres of our body at once and cause compound sensations, which we gradually learn to associate together as being the token of our senses for a particular object, without being conscious of the compound nature of the token itself. On the contrary, in most cases of this kind the compound character of the sensation is usually never realized until it is subjected to scientific analysis. The sensation of a musical note of a certain definite *timbre* is composed of a majority of sensations of much simpler tones. When a pencil is held in the hand and felt by two fingers, two groups of separate nerve-fibres are involved. We get the same smell through both nostrils. The sensation produced on touching a wet body, which appears to be simple, is really due to the sensations of smoothness and coldness both at the same time. Many similar instances might be given. In fact, merely because a complex effect is produced on a complicated organism like the human body, we have no right to infer that the object itself is complicated.

As a general thing, therefore, it is altogether a matter of experience, whether a certain group of sensations, which frequently recur together, gets to be associated in our minds as the token of the senses for one object or for several objects.

Ordinarily, the object on which the attention is riveted for the time being is focused by both eyes at the same time; that is, the image of it will be formed in the *fovea centralis* of each eye, where vision is most accurate. The consequence is that there will always be images of one and the same external object in the fovea of each eye at the same time; and, incidentally, the unity of the object may be verified by touching it, whenever that seems to be necessary. Thus we soon learn to realize that the visual sensations in the foveas of the two eyes always mean the same thing so far as their relation to external space is concerned; and so the explanation of single binocular vision is that, when the eyes are used in the natural, normal way, the object at which we are gazing is imaged in the *fovea centralis* of each eye at the same time; and we know, or can know, by touching the object that there is really only one thing there.

But according to the opposite view, which insists that certain bodily sensations have the faculty of arousing certain ideas of space, prior to all experience, it is necessary to suppose that not only the centres of the two retinas but also every other pair of corresponding places produce identical apperceptions of space by virtue of some innate mechanism. This was the reason in the first instance for speaking of corresponding places (*Deckstellen*) on the retinas as *identical* places. A critical comparison of the two views must be postponed until after the following chapter.

Frequently in cases of what is known as *concomitant strabismus*, there are (as was stated above) exceptions to the rule that the foveas of the two eyes are corresponding places. This is true especially of those individuals who use one eye just as well as the other for seeing. In this form of strabismus the axes of the two eyes cannot be made parallel, that is, the eyes are either convergent or divergent, the angle between the visual axes being practically constant, no matter in what direction the eyes are gazing. When the visual acuity of one eye happens to be considerably better than that of the other eye, the patient contracts the habit of looking at a thing with his good eye alone, and he is not apt to use the other eye at all unless the good eye is screened. When the two eyes have about the same visual acuity, the strabismus is *alternating*, that is, in looking at anything, the patient uses first one and then the other. Incidentally, he judges correctly as to the directions of the observed objects by using both eyes. Now in the majority of these latter cases it is found that the two points of fixation are no longer corresponding places, but that the *fovea centralis* on the retina of one eye corresponds to a different place on the retina of the other eye, which is more to the outside or to the inside, depending on the direction of the squint. And yet the patient has single vision in spite of the faulty adjustment of his eyes. By holding an ophthalmic prism base-up or base-down in front of one of his eyes, it can be demonstrated that he really does see with both eyes, and does not simply suppress one image, as is generally supposed. On looking through the prism, one of the images will be seen above the other, just as would be the case with a person whose vision was normal. The effect of the prism is to shift one of the images upward, thereby separating the total binocular image into its two half-images, one above the other; so that it is easy to tell positively whether the two images are seen or not, and whether one of them is more to the right or the left than the other. Or the prism may be adjusted in front of one eye base-in or base-out, so as to shift the images sideways with respect to each other; as will be the case even when the prism is so chosen and so adjusted that the image of the object is in the *fovea centralis* of each

of the two eyes. Sometimes by extra exertion patients of this sort may be able to make the visual axes of the two eyes parallel, but even then distant objects, which under these circumstances should be seen single, will be seen double.

The same thing happens even when a successful operation has been performed and the eyes are restored to their normal position. At first the patient is much annoyed by the double images, but presently he learns not to mind them, until finally, after the lapse of a year or more, the normal identity-relation is established. Yet this is not always the case, especially if the visual acuity of one eye is considerably less than that of the other. Under such circumstances the double images that make their appearance after the operation usually remain in the same relative position with respect to each other, but in the orientation the vaguer one is suppressed. There are some cases also in which this habit of suppressing one image has become so confirmed that it is impossible to perceive it even with the aid of prisms and coloured glasses.

Just as it is easier for a patient, whose visual acuity is less in one eye than it is in the other, to get rid of the double images after the operation by suppressing one of them than by trying to reconstruct a new identity-relation, so also it is harder for a patient with strabismus, who has one bad eye, to get an idea of the incongruity of the retinas of his two eyes. In cases of this latter kind, even when the strabismus condition has existed for years, the foveas of the two retinas continue always to be corresponding points. It is the same way in all those cases where the angle of convergence or divergence of the lines of fixation varies as the direction of vision varies or changes periodically at different times; because under such circumstances the image falling on the fovea of one eye will fall at very different places on the retina of the other eye, and, consequently, the patient is unable to form any fixed habit of association or correspondence between the two images.[1]

Again, it has been found that when a piece of red glass was inserted in front of one eye of a strabismic patient, whose vision was less good

[1] The proof of the fact that many strabismic persons use both eyes for seeing and yet see single, was given by PICKFORD in ROSER and WUNDERLICHS *Archiv f. Physiol. Heilkunde*, 1842, p. 590. The first cases of strabismic incongruity were described by ALBRECHT V. GRAEFE in the *Archiv. f. Ophthalmologie*, I, 1, 234; see also NAGEL, *Das Sehen mit zwei Augen* (Leipzig, 1861), pp. 130-135. The results of a larger number of observations were given by ALFRED GRAEFE in the *Archiv. f. Ophthalmologie*, XI, 2, pp. 1-46. See also F. C. DONDERS in the *Archiv f. die hollandischen Beiträge zur Natur- und Heilkunde*, Bd. III, pp. 357, 358; and in *Anomalies of accommodation and refraction*, pp. 164-166. These observations are of fundamental importance for the theory of binocular vision, and it is much to be desired that they could be repeated as often and as accurately as possible.

in one eye than in the other, double images would be seen sometimes and then suddenly disappear before the eyes had been moved at all. Or after the operation had been performed, the coloured half-image will be seen sometimes on the right of the colourless image and then again on the left of it; or perhaps the patient may not be able to tell on which side it is. In an eye of this sort, where the image is so little heeded because it is so vague, the orientation is apt anyhow to be more or less uncertain, and there is a conflict, as it were, between the identity-relation that existed prior to the strabismus and the new relation that cannot be very positively and definitely constructed. As ALFRED GRAEFE very aptly remarks, the conflicting statements that we get about this very matter are characteristic of the process itself.[1]

2. *The retinal horizons of the two eyes correspond to one another.* In the case of emmetropic eyes, the retinal horizons were defined (p. 43) as those meridians of the two eyes which coincide with the visual plane, when the eyes are parallel and both in the primary position; and it was stated then that they correspond with one another. In the case of near-sighted eyes, this is not usually so; and the suggestion was made to regard those meridians as the retinal horizons which were in the visual plane when the eyes were so adjusted that rows of corresponding points on the two retinas were in this plane. For near-sighted eyes this will usually mean a convergence-position directed a little downward. Then the statement made above would simply be a consequence of the definition of what was meant by "retinal horizon." However, another thing that should be noted here is that when the point of fixation lies in the median plane, the retinal horizons of the two eyes apparently coincide with the visual plane, as well as the eye can judge.

Accurate determinations of the positions of the retinal horizons have been published by VOLKMANN.[2] The measurements were made on his eyes which were a little near-sighted. The observer stood with his eyes opposite the centres of two circular discs which were fastened on a vertical wall. When the eyes were directed to infinity, the optical axis of each eye passed through the centre of the opposite disc. The discs could be turned around horizontal axes through their centres; and on each of them a fine line was drawn representing a diameter or

[1] Of late years very careful study has been given to the vision of strabismic patients and especially to the modifications of it as the result of operation. The author proposes to treat the great array of facts that belong here from certain definite theoretical points of view; and, consequently, I prefer to speak of this whole matter in the Appendix.—K.

[2] *Physiologische Untersuchungen im Gebiete der Optik.* Leipzig 1864. Heft 2. pp. 206-208.

a radius, by which the angle through which the disc was turned could be read on a scale around the edge of the disc which was marked in degrees. Each set of experiments included thirty observations. The results were as follows (the right-hand and left-hand discs being referred to as "disc *R*" and "disc *L*," respectively):

Experiments No. 1.—A diameter of disc *L* being placed so that it was horizontal, the problem consisted in setting the disc *R* with its diameter parallel to that of the other disc. In order to see the two lines separate, the head had to be tilted a little to one side. The average angle between the diameters was found to be 0.443°, with a probable error of 0.08°.

Experiments No. 2.—The diameter of disc *R* was made horizontal, and the problem was to set the diameter of disc *L* parallel to it. In this case the average angle between the two diameters was found to be 0.553°, with a probable error of 0.11°.

Experiments No. 3.—The diameter of disc *L* being horizontal, the problem was to set that of disc *R*, so that when they were fused together they would form as fine a line as possible. In this case the average angle between the two lines proved to be 0.397°, with a probable error of 0.13°.

Experiments No. 4.—The problem was the same as in the preceding case, except that disc *R* was fixed and disc *L* had to be adjusted. The average angle in this case was 0.467°, with a probable error of 0.14°.

Experiments No. 5.—In these observations radii were used instead of diameters. The radius of disc *L* was made horizontal, and the problem consisted in adjusting disc *R* so that its radius was apparently in the same straight line with that of the other disc. The average angle was found to be 0.46°, with a probable error of 0.125°.

Experiments No. 6.—The problem was the same as in No. 5, except that the fixed disc was disc *R* and the adjustable disc was disc *L*. The average angle obtained was 0.463°, with a probable error of 0.096°.

As we see, the results of all these experiments are in very close agreement; the arithmetical mean of all six sets of observations being 0.464°. The angle is such that the outer side of the retinal horizon of each eye is slightly lower than the inner side.

Experiments No. 7.—In another form of the experiments, a single disc was used which was viewed only by one eye, and the problem was to set the diameter marked on the disc so that it would be exactly horizontal. When the left eye was used, it was found (as the average of thirty trials) that the left-hand end of the diameter was set too low by an angle of 0.203°.

Experiments No. 8.—The problem was the same as in No. 7, except that here the right eye was used; and in this case the right-hand end of the diameter was set too low by an angle of 0.233°.

The sum of the angles obtained in No. 7 and No. 8 is (0.203 +0.233 =)0.436°; which is in fairly close agreement with the angle between the retinal horizons as found above.

Experiments Nos. 1-4 were made by VOLKMANN with several other observers, and the following values were obtained for the angle between the two retinal horizons:

Professor H. WELCKER.....................0.72°
A medical student named KÄHERL...........0.26°
Dr. SCHWEIGGER-SEIDEL....................0.43°

I myself have tried VOLKMANN's experiments Nos. 5 and 6 on my own eyes, without being able to detect any appreciable difference between the two retinal horizons, provided I had been looking previously at distant objects, or provided the axes of my eyes had been kept parallel by continuing the experiment for a long time. But after I had just been reading or writing and my eyes were convergent, I did find that there was a slight angle between the two retinal horizons, in the same sense as VOLKMANN found, which was variable in amount and which gradually became less and less as the experiments proceeded, until it vanished entirely.

Dr. DASTICH (whose left eye was emmetropic and whose right eye was myopic) obtained an angle of 0.31°.

As to the way in which this identity-relation between the two horizontal meridians presumably originated, we ought to note that when the eyes are focused on a definite point of the object, no matter how the line proceeds in which the surface of the object is cut by the visual plane, there will invariably be a row of images of the same set of points of the object in the two meridians of the visual globes and retinas that coincide with the visual plane. But for all other meridians the relation will be very variable and will depend on the position and form of the object. For example, if a vertical line passes through the point of fixation, its two images will be on the corresponding points of the two retinas in the vertical meridians of the visual globes. If the upper part of the observed line is inclined toward the observer, its two images will be in meridians of the visual globes which slope toward each other upwards. But if the upper part of the line is inclined away from the observer, the two meridians where its images appear to be will diverge upwards. Hence, except in the case of the meridians that coincide with the visual plane, the particular meridian in one eye where the images will lie that are depicted on a certain meridian of the other

eye will depend on the form and position of the object. The only corresponding meridians of the two eyes that are independent of the form and position of the object are the meridians that lie in the visual plane.

Of course, by turning the eyes in different directions, different meridians of the retina can be made to fall in the visual plane. However, perhaps we can assume that in the ordinary conditions of life, except in the case of persons whose occupations require them to hold the body and head over on one side, during most of the time the eyes are in or near the primary position; and that, therefore, those retinal meridians which, when the eyes are in the primary position, coincide with the visual plane (but which are really the retinal horizons) are the meridians of all others where corresponding images are most apt to be; and that, consequently, the habit was formed of projecting the images on these meridians the same way in space.

On the other hand, it is possible that excessive concentration on near objects, involving downward convergence of the eyes, may account for the angle between the retinal horizons such as VOLKMANN found in his own case and in the cases of some other observers; for when the eyes are turned in this way, the retinal horizons are actually brought into the visual plane.

3. *The meridians which are apparently vertical with respect to the retinal horizons are a pair of corresponding meridians.* It was stated above (p. 173) that the meridians of the visual globes which, as far as the eye can tell, appear to be perpendicular to the retinal horizon are really a little farther apart above than they are below. In other words, if the retinal horizons are in the visual plane, the apparently vertical meridians diverge a little upwards or converge a little downwards. These same apparently vertical meridians, which therefore appear to have the same positions on the two visual globes with respect to the point of fixation and the retinal horizon, prove to be a pair of corresponding meridians in the binocular field of view.

The angle between the two corresponding apparently vertical lines may be found in the same way as the angle between the retinal horizons if we leave out those methods in which the two lines were made to coincide; because in the latter case it is too easy to fuse two similar lines of the same colour into a resultant stereoscopic image, even when the directions of the lines are quite different. However, this difficulty can be obviated by making the colour of the lines entirely different. For instance, a white thread on a black background can be combined with a black one on a white background. The most reliable and concordant results which I finally succeeded in getting were obtained by the following method.

A sheet of black paper was mounted on a vertical drawing board, and on it were fastened side by side a red strip of paper (with straight parallel edges 3 mm apart) and a piece of blue thread. They were both almost vertical, but slightly divergent upwards, their distance apart being such that on the level with the observer's eyes it was just equal to his interpupillary distance. The strip of paper was fastened at the top and bottom, but the thread was fastened only at the top, a little weight being attached to its lower end. This lower end was pulled to one side as far as necessary, where it could be held in the proper position by sticking a pin in the board. With the axes of his eyes parallel, the observer looks toward this arrangement, so that the blue thread appears to lie exactly midway along the strip of red paper, adjusting the thread until it seems to bisect the paper from top to bottom, and then sticking the pin there so as to hold it. The angle of divergence can then be obtained easily by measuring the real distances between the thread and the strip of paper at the top and bottom.

The most direct way of verifying the truth of the proposition stated above is to measure the divergence of the two corresponding lines first when they are horizontal and then when they are vertical, by the method which has been described, and to measure at the same time the angles between a horizontal line and lines that are apparently perpendicular to it. Experiments of this sort have been carried out in my laboratory by Dr. Dastich, the results being as follows:

Angle between apparently vertical corresponding lines....	2°40′
Angle between the retinal horizons....................	0°18′
Difference..	2°22′

The error in the estimate of the right angle was 1°12′ for his right eye, and 1°21′ for his left eye; the sum of the two being, therefore, 2°33′. The difference between the first pair of angles (2°22′) is the measure of the angle that ought to be made with each other by the two apparently vertical meridians when the retinal horizons of the eyes are in the visual plane. The agreement between this value and the sum of the other pair of angles (2°33′) is as close as could be expected with the degree of accuracy that is possible in the case of experiments of this nature. Accordingly, we may say that the apparently vertical corresponding lines are practically the same as those lines which seem to the eye to be perpendicular to the retinal horizons.

This conclusion, by the way, is confirmed indirectly by Volkmann's researches. In addition to the two sets of experiments described above, in which Volkmann tried to set the diameter of one of his discs horizontal by using one eye (Nos. 7 and 8 above), he also carried out

some other experiments in which the problem was to set the diameter vertical, that is, to set this line absolutely in the vertical direction and not by trying to adjust it at right angles to a visible horizontal line. However, as already stated, the retinal horizons of VOLKMANN's eyes were apparently not absolutely horizontal under the conditions in his experiments; and hence the apparently vertical directions as determined by him must have been also perpendicular to the retinal horizons. The results which he obtained were as follows:

Experiments No. 9.—Viewing the disc with his left eye, and trying to set the diameter vertical, VOLKMANN found a deviation from the true vertical of 1.307°, as the average result of thirty observations.

Experiments No. 10.—The problem in this case was the same as in No. 9, except that the disc was viewed here with the right eye. The mean deviation from the true vertical with the right eye was found to be 0.82°.

VOLKMANN also measured the angle between the two apparently vertical corresponding lines by the same methods which he had used for horizontal lines, and the numerical results which were obtained in this way are given in the following summary:

Method: Same as that in	Average angle between apparently vertical lines	Probable errors
Experiments No. 1.	2.23°	0.16°
Experiments No. 2.	2.06	0.07
Experiments No. 5.	2.16	0.22
Experiments No. 6.	2.14	0.21
	Total average: 2.15°	

Now the sum of the two deviations as found in Experiments Nos. 9 and 10 for each eye separately is $1.307 + 0.82 = 2.127°$; which turns out to be so nearly the same as the value just given for the angle between the two apparently vertical corresponding lines that it indicates that the lines in the two visual globes which seem to the eye to be vertical are also a pair of corresponding lines. This is again in accordance with our proposition.

At VOLKMANN's suggestion, the experiments were repeated by Mr. SCHWEIGGER-SEIDEL. The values he got for the deviation of an apparently vertical line from the true vertical were 0.666° for his left eye and 0.657° for his right eye. The sum of these two angles is 1.32°. This latter value agreed very closely with the value which he obtained for the angle between the two apparently vertical corresponding lines (1.44°).

In a final set of experiments, VOLKMANN adjusted the diameter of one of the discs so that it was horizontal, and then tried to set

the diameter of the other disc perpendicular to it in the total binocular image. These results also proved to be in close agreement with the previous ones and tended to verify the proposition, that the apparently vertical meridians are a pair of corresponding lines. This is a special case of the more general proposition, which was formulated above, namely, that lines which appear to be equally placed in the two monocular fields are pairs of corresponding lines. In other words, as soon as we have established the fact that the retinal horizons are a pair of corresponding lines, it follows that the apparently vertical lines which seem to have the same positions with respect to the point of fixation are necessarily also a pair of corresponding lines.

It seems that in emmetropic eyes the angle between apparently vertical lines has nearly always about the same value, namely, 2.5°. According to my experience, this angle is much less for near-sighted eyes. E. Hering, who is myopic, found that it was practically zero in his case.

Fig. 66.

In our investigations of the theory of the monocular field of view, the processes which we studied there in connection with the development of the eyesight did not enable us to assign any definite value to this angle; or rather the value was left indeterminate. Presently, when we come to consider the theory of the horopter, certain factors will appear that seem to govern the size of this angle.

4. *Points equally distant from the retinal horizons, which are on the apparently vertical corresponding lines, are a pair of corresponding points.* Careful experiments on this subject were carried out by Volkmann also. Two rectangular crosses composed of two horizontal lines a, a' and two vertical lines s, s' (Fig. 66), were adjusted so that the centre of each was exactly opposite one eye, the distance between the two being, therefore, the same as the observer's interpupillary distance. Extending outwards from the vertical lines, two other

horizontal lines (b, b') were drawn below a and a'. The line b was stationary, but the other line b' could be raised or lowered. Adjusting the visual axes so that they are parallel, the observer gazes at the centres of the two crosses until they appear to be fused; and then he raises or lowers the horizontal line b' until it seems to be the exact continuation of the fixed line b on the other visual globe. In VOLKMANN's experiments the distance of b below a was 5.50 mm. The average of thirty trials gave 5.51 mm for the distance of b' below a', when the adjustable line was opposite the right eye; and 5.47 mm, when this line was opposite the left eye. The distance of the two crosses from the eyes was 300 mm, and so the difference between the two values to be compared here is below the limit of the minimum perceptible interval.

The conditions of natural vision happen to be especially conducive to acquiring a fixed practice in making comparisons of vertical distances on the visual globes of the two eyes. For whenever the point of fixation is in the median plane of the body, that is, when the eyes are directed straight ahead, any point in the object lying above or below the point of fixation will necessarily be always at the same angular distance from this point on each visual globe, although it may appear to the two eyes to be in somewhat non-corresponding (disparate) meridians. This is true even when such a point is much farther away, or much nearer, than the point of fixation. Thus every time we look straight ahead, we have the opportunity of gaining experience as to what are the corresponding vertical dimensions on the two visual globes. Consequently, as we shall see presently, it is especially easy to tell when one of the double images is vertically above the other.

5. *Points in the retinal horizons at equal distances from the point of fixation are pairs of corresponding points.* A series of experiments was carried out by VOLKMANN to determine this fact; the method being similar to that just described, except that instead of a pair of horizontal lines, one of which was stationary and the other adjustable, he had in this case a pair of vertical lines. Each of these vertical lines was to the right of the vertical line of the cross, but one of them was above, and the other below, the horizontal line. The one above was the stationary line, and it was situated 5.20 mm from the vertical line of the cross. The one below was adjustable. When the latter line was opposite the right eye, its distance from the vertical line of the cross was found to be 5.24 mm, as the average of thirty settings. With the left eye, the average of the same number of settings gave 5.21 mm. Here again, therefore, the differences to be compared are less than the size of the

minimum perceptible interval. Evidently, these measurements too were made by VOLKMANN with very great accuracy.

My personal experience is that this adjustment is very much harder to make than it is when the lines are horizontal; due to the fact that in my case there is an apparently stereoscopic fusion of the vertical lines of the cross that have to be fixated. This is the case even when the two lines of fixation are a little more convergent or divergent than is necessary for exact fusion. The effect of it is that the vertical side-lines oscillate to and fro, and so I can make one or the other of them appear to come nearer the fixed vertical line, just as I choose. I get better results with this experiment when one of the fixed vertical lines does not extend below, and the other does not extend above, the horizontal lines of the two crosses.

As a general thing, consistent results cannot be obtained by making comparisons between horizontal distances on the two visual globes, unless the objects are practically at an infinite distance, on the earth's horizon, for instance. Of course, the interval between two points on the horizon will necessarily be the same in the images on the visual globes of the two eyes; and by comparing these images, we can discover what horizontal distances in these fields (or on the retinas of the two eyes) are equal in length. But with all nearer objects there is a difference in the perspective projection, and hence it will not be likely that the angle subtended by a horizontal line drawn between two adjacent object-points will be the same on both visual globes. Accordingly, we find that double images situated in a horizontal line side by side are much easier to fuse, and harder to be separated apart, than images that lie vertically one above the other. Nevertheless, under favourable conditions and with practice (as VOLKMANN's experiments prove), the ability can be acquired of comparing the two visual globes, so as to make fairly accurate and correct estimates of the equality or inequality of two vertical dimensions. Moreover, by virtue of the symmetry in the position of the two eyes, the distribution of errors in the two eyes cannot be unsymmetrical. If a and a_1 are equal lengths on the exterior hemispheres of the two visual globes, and b and b_1 equal lengths on the other two hemispheres, then from the symmetrical positions of the two eyes there will be no reason for considering a as being longer or shorter than a_1, or for considering b as being longer or shorter than b_1. Besides, as the eyesight enables us to tell correctly that $a=b$ and that $a_1=b_1$, we can also tell correctly that the corresponding lines are $a=b_1$ and $b=a_1$.[1]

[1] Concerning certain modifications of the rules here given, see Note 1 at the end of this chapter.—K.

After having established the directions on the two visual globes or on the two retinas which correspond to each other as being apparently horizontal or as being apparently vertical, and having determined what lengths horizontally or vertically appear to be equal, we have then the necessary data to enable us to compare the apparent position of any point in one monocular field with that of any other point in the other field. It is only with respect to the central portions of the visual globes, as we have seen, that we have a right to speak of an accurate comparison between the positions of the double images, because in the peripheral portions there is too much uncertainty about recognizing the corresponding places and also about estimating distances by the eye. Accordingly, for our present purposes, the central portion of each visual globe may be regarded as the portion of a plane surface.

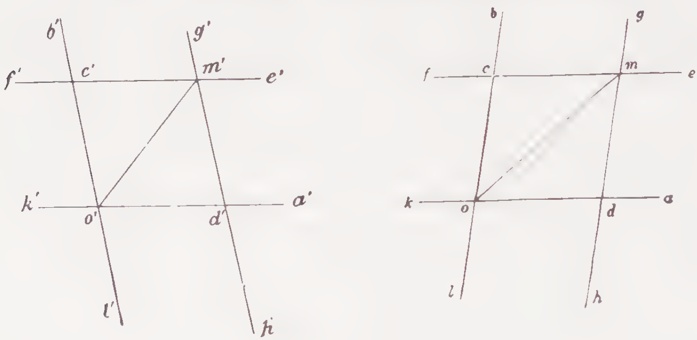

Fig. 67.

Let o (Fig. 67) designate the position of the point of fixation of the right eye which is supposed to be in the plane of the paper; and let o' designate the position of the point of fixation of the other eye. The apparently horizontal and vertical lines for the right eye are represented by ak and bl, respectively; and those for the left eye by $a'k'$ and $b'l'$, respectively. Moreover, suppose that co, $c'o'$ are equal intercepts on the two apparently vertical lines; then these lengths will appear to be equal also, and c and c' will be a pair of corresponding points. Similarly, suppose that do, $d'o'$ are equal intervals on the apparently horizontal lines. Through c and c' draw ef and $e'f'$ parallel to ak and $a'k'$, respectively. Then every point on ef must be at the same distance from ak as the point c is, and not only really so but apparently so too, because the eye can judge correctly the distance between parallel lines. The same thing is true with respect to the lines $a'k'$ and $e'f'$ in

the left-hand portion of Fig. 67; and since the apparent distances of c and c' from ak and $a'k'$, respectively, are supposed to be equal, the lines ef and $e'f'$ must appear to be a pair of horizontal lines on the two visual globes at equal distances from the retinal horizons; and hence, provided the above proposition is true, namely, that all points that are apparently in the same relative position on the two visual globes are pairs of corresponding points, ef and $e'f'$ must be a pair of corresponding lines.

Similarly, gh and $g'h'$ can be shown to be a pair of corresponding lines; and hence, finally, the points m and m', where ef and $e'f'$ intersect gh and $g'h'$, respectively, must be a pair of corresponding points.

These conclusions may be summarized by saying that, on the assumption of the fundamental proposition, which has been stated several times above, *any pair of points on the two visual globes, which are at equal distances in the same direction from the apparently horizontal and apparently vertical pairs of corresponding lines, will be a pair of corresponding points.*

The truth of this proposition can be tested experimentally by means of the stereoscopic diagrams D on Plate III. In order to prevent the corresponding lines from being too easily fused, the figure on the right is drawn with white lines on a black background, and the one on the left with black lines on a white background. They are supposed to be viewed with parallel lines of fixation and to coincide with each other in the common field of view under these circumstances. Any one who is unable to do this with his naked eyes must look at them through a stereoscope. In my own case each diagram, as seen by the eye for which it is intended, appears to consist of two sets of lines exactly at right angles to each other; and I hope that it will appear the same way to most of my readers whose vision happens to be normal. Otherwise, similar figures will have to be constructed to suit the particular idiosyncrasies of the observer; but care must be taken to draw the horizontal and vertical lines in one figure so that they make the proper angle with the corresponding lines in the other figure and can be made to coincide when the axes of the eyes are parallel. The interval between the centres of the two figures should be the same as the observer's interpupillary distance; and the distances between the horizontal lines should be the same in both figures, and also the distances between the vertical lines.

When I gaze directly at the centre of each half of the figure with that eye for which this half was intended, all the lines in one half will fall on the corresponding lines in the other half in the common field of view. It is easy to tell whether this is the case or not, because the

black lines on the left do not fuse easily with the white lines on the right.[1]

The experiment made with diagrams such as those just mentioned enables us to explain also how we can find pairs of corresponding points in the two eyes. The axes of the eyes must be parallel to the median plane and directed to the centres of the two halves of the figure, the plane of the figure itself being perpendicular to these axes. Then suppose that planes are passed through the horizontal lines of the drawing and the nodal points of the eyes. The planes that go through the central horizontal lines, where the points of fixation are, will coincide with the retinal horizons. The other planes will intersect each other and the retinal horizon in a horizontal line perpendicular to the visual axis; which may be called the *equatorial axis* of the retinal horizon. The angle between one of these planes and the retinal horizon is the *angular altitude* (*Hohenwinkel*) of that plane. All points in any such plane have the same apparent altitude above the visual plane when we think of them as being projected on an infinitely distant field of view; and hence it may be called a *plane of equal angular altitude.*

Similarly, imagine the sets of planes that pass through the vertical lines in each half of the figure and the nodal point of the corresponding eye. The middle plane of each set goes through the point of fixation and will be the apparently vertical meridian. All the other planes of this family will meet it in a line which is perpendicular to the visual axis; and this line may be called the *equatorial axis of the apparently vertical meridian.* The angle between one of these planes and the plane of the apparently vertical meridian will be called the *azimuth* (*Breitenwinkel*) of this plane; being reckoned positive toward the right, and negative toward the left, for both eyes. These planes constitute a system of *equal-azimuth planes.*

With the aid of these definitions, it is easy to locate the positions of a pair of corresponding points on the two visual globes. Let planes be passed through the given point in the field of view and the equatorial axes of the retinal horizon and the apparently vertical meridian; which will determine the angular altitude and the azimuth. *Any pair of points that have the same angular altitude and the same azimuth in the two fields of view will be a pair of corresponding points.*

This definition of a pair of corresponding points is found to be justified by direct experiment. Imagine that the two figures which

[1] If an observer s apt to be confused by the large number of lines in these diagrams, (as Mr. E. Hering finds to be the case), similar observations can easily be made with simpler systems of lines. In my articles on the horopter I had supposed that this went without saying, but, as the omission has led to some criticism, I think it well to make the explicit statement here.

represent the configuration of the field of view are extended into infinite planes. Then the distributions of the pairs of corresponding points will be shown as far out as to 90° on either side of the visual axis. This is perfectly sufficient too for this purpose, for although the field of view of each eye separately extends outwards a little more than 90°, the binocular field is much smaller, because the bridge of the nose hides this outermost part of the field from the other eye. Incidentally, too, an exact experimental determination of a pair of corresponding points cannot be made except for those parts of the two visual globes that are fairly close to the point of fixation. For as we go farther away from this point, it begins to be extremely difficult to tell by indirect vision what objects on the two visual globes are coincident, and what objects are not; and unless the differences in the double images are quite pronounced, they cannot be perceived anyhow.

Another thing that should be added is that the pairs of corresponding points are not equally far from the point of fixation along all pairs of corresponding meridians of the two visual globes; although this is the case with the apparently horizontal and apparently vertical corresponding lines. The diagonal om (Fig. 67) connecting the point m with the point of fixation o is longer than the diagonal $o'm'$ connecting the corresponding point m' with the point of fixation o', and yet om, $o'm'$ are two corresponding intervals on corresponding meridians. The difference between them is not much.

If we put

$$md = co = m'd' = c'o' = a\ ,$$

$$mc = od = m'c' = o'd' = b\ ,$$

the lengths of the corresponding diagonals will be:

$$mo = \sqrt{a^2 + b^2 + aab \sin\epsilon}\ .$$

$$m'o' = \sqrt{a^2 + b^2 - 2ab \sin\epsilon}\ ,$$

where ϵ denotes the complement of the angle cod or $c'o'k'$. Relatively speaking, the difference between them is greatest when $a = b$; in which case

$$mo = 2\,a \cos\left(45° - \frac{\epsilon}{2}\right) \text{ and } m'o' = 2\,a \cos\left(45° + \frac{\epsilon}{2}\right).$$

For my eyes the value of the angle ϵ is 1°13′, and hence the ratio between these two lengths is as 1 is to 1.0215 or as 47 is to 48. I used the system of lines shown in Fig. 68 in order to observe this difference. The points a and a' were focused by the left eye and the right eye, respectively; in which case the lines ac and $a'c'$ appeared to form one

line in the binocular image. The same thing was true with respect to ab and $a'b'$. The line fg was drawn on a separate piece of paper so that it could be rotated around the distant point g. The problem consisted in adjusting gf so that it would appear to be the continuation of the line ed; the eyes being focused all the time on a and a'. The result which I got was that, when the interval ad was 20 mm, I had to make the interval $a'f$ about 19.5 mm. Of course, great care has to be taken to be sure that ac and $a'c'$ appear to form a continuous line all the time. The difference involved here is close to the limit of minimum perceptibility.

I find also that these differences of which we have been speaking can be manifested to the eye by using two systems of concentric circles as shown on Plate IV, diagram O. On the left-hand side the

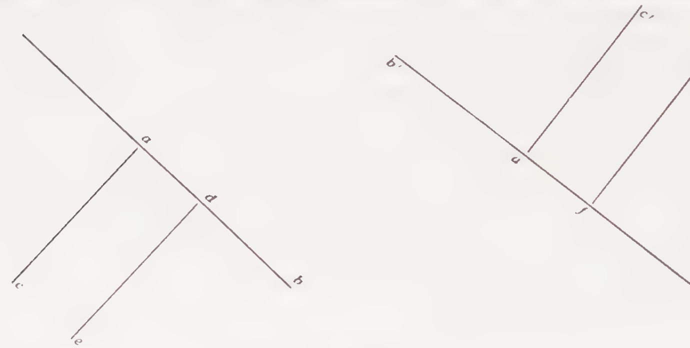

Fig. 68.

circles are drawn by black lines on a white ground; whereas on the right-hand side, it is just the other way. The observer's eyes, supposed to be parallel, must be focused on the centres of the two figures, so as to fuse them. The white and black lines in the vertical and horizontal meridians will be actually fused; but in the other meridians they will not be fused and will lie side by side. The black lines will be on the outside in the first and third quadrants, and the white lines in the second and fourth quadrants. A radius drawn in the first quadrant of the right-hand field would have to be made longer than the corresponding radius in the left-hand field, in order for the former to appear to be as long as the latter.

From the above considerations a formula may be derived for the size of the angle between corresponding lines which are in different directions. The calculation will be given later, but the result is that, for parallel lines of fixation, the angle between two corresponding meridians is

$$\Delta = \gamma + 2\epsilon \sin^2\beta \,,$$

where γ denotes the angle between the two retinal horizons when the eyes are in the given position, 2ϵ denotes the angle between the two apparently vertical meridians, and β denotes the mean value of the angles between the given pair of corresponding lines and the retinal horizons. This formula may be tested experimentally by means of a set of measurements which were made by VOLKMANN[1] to determine the angles between corresponding meridians. The values of the constants γ and ϵ in the above formula, as given in the subjoined table, were obtained from all the observations by the method of least squares.

Angle between Corresponding Meridians
(for VOLKMANN's eyes)

Inclination to the vertical $(90°-\beta)$	Angle Δ			Difference between observed and calculated values
	Mean of observations	Probable error	Calculated value	
0°	2.15°	0.106°	2.166°	−0.016
15	1.99	0.064	2.062	−0.072
30	1.78	0.195	1.781	−0.001
45	1.51	0.075	1.397	+0.113
60	1.15[2]	0.114	1.013	+0.137
75	0.81	0.084	0.732	+0.078
90	0.46[3]	0.062	0.628	−0.168

$\gamma = 0.628°$ $2\epsilon = 1.5375°$

The probable errors of the mean values in the second column of this table as given in the third column were computed from the values given by VOLKMANN for each set of experiments. We see that in general the difference between observation and calculation does not exceed the probable error in each case; and so the agreement may be regarded as quite satisfactory.

Having determined the positions of the corresponding points on the visual globes of the two eyes, we may proceed now to find the positions of those points in external space, whose images are depicted on corresponding places of the two retinas, and which are, therefore, seen single. The locus of all these points is called the *horopter*. In general, it is a curve of double curvature, which may be regarded as being the line of intersection of two surfaces of the second degree (an hyperboloid and the surface of a cone or cylinder). The intersection

[1] See Experiments 100-112 in the second part of VOLKMANN's *Physiologische Untersuchungen im Gebiete der Optik*, pp. 202-213.

[2] There is an error in the calculation as given by VOLKMANN (*loc. cit.*, p. 213).

[3] Average of the two sets of experiments Nos. 106 and 107.

of two surfaces of the second degree is generally a curve of the fourth degree, that is, a curve which will be cut by a plane in four points. In this particular case, however, the two intersecting surfaces have a straight line in common, which is not a part of the horopter; and the rest of the line of intersection will be a *curve of the third degree*, that is, a curve which will be cut by a plane only in three points. A remarkable property of this curve is that a pencil of lines drawn through any point on it to all the other points on the curve will generate a cone of the second degree. In the special case when the vertex of the cone is the infinitely distant point of one of the branches of the curve (of which

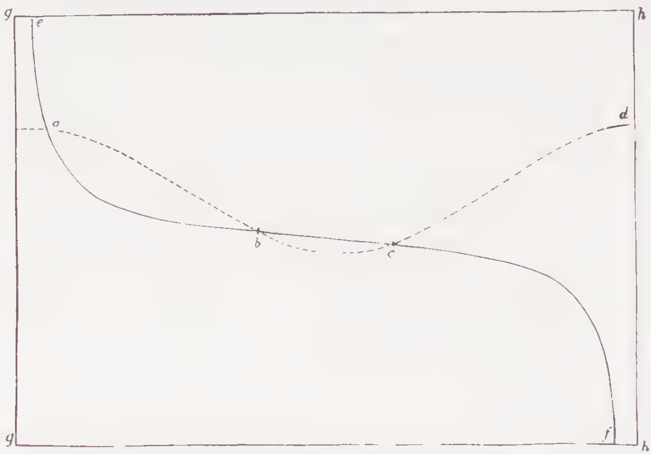

Fig. 69.

there are at least two), the cone becomes a cylinder whose base will be a curve of the second degree. An idea of the form of such a curve of the third degree can be obtained by supposing that it is drawn on the surface of a cylinder and that the cylinder is then unrolled on a plane. The form of the curve would be then like that shown by the continuous line *eabcf* in Fig. 69. When the paper is rolled into a circular cylinder so as to bring the edges *gg* and *hh* together, this curve will have the form of a curve of the third degree. The dotted line *abcd* represents the line of intersection of the cylinder with a plane (with the visual plane, for instance), which crosses the curve of the third degree in the three points *a*, *b* and *c*. At two places *e* and *f* the curve extends to infinity, being asymptotic to the straight line *gg* (or *hh*, which is the same line as *gg*).

Supposing that this curve of the third degree represents the horopter, it will necessarily have to pass through the points of inter-

section of the lines of sight of the two eyes. Let the places of the two eyes be designated by b and c, and the point of fixation by a. Then the arc bc, which is the part of the curve comprised between the two eyes, will fall inside the head, and cannot be considered as part of the horopter, that is, not in the sense in which we ordinarily mean or according to the definition given above; because, supposing that points on this segment could send forth light and that such light could actually enter the two eyes, their images would have to be on the two external or non-corresponding halves of the retinas. Anyhow, the definition of the horopter ceases to have any practical meaning for those points in space which happen to be so close to the eyes that their images are simply large blurred splotches on the two retinas. Thus the horopter as such is composed of two entirely separate branches, eb and fc, that is, of those portions of the curve of the third degree that are comprised between each eye and infinity. From a strictly mathematical point of view, it is more convenient to consider the entire curve and accordingly to speak of it as the *horopter-curve*; reserving the term *horopter* or *point-horopter* to mean those portions of the curve that are seen single. Therefore, corresponding lines of sight will intersect each other in the horopter-curve, but when these lines have to be produced backwards for this purpose, the point of intersection does not lie on the horopter itself.

Under certain conditions, by the way, the horopter-curve may become practically coincident with its asymptote gg and with the plane curve of the second degree which is obtained from the curve ad when the sheet of paper in Fig. 69 is curved to form the cylinder. Accordingly, the horopter then will consist of a straight line and a plane curve of the second degree intersecting each other in one point, where the two separate branches of the horopter-curve will be united in this special case. This will occur whenever the point of fixation is at a finite distance, and the two retinal horizons, extending in opposite directions sideways, are equally inclined to the visual plane. On the assumption that the movements of the eyes are executed in conformity with Listing's law, the above condition will be fulfilled provided the point of fixation is either in the median plane of the head or in the primary position of the visual plane. In the former case the point of fixation will lie on the rectilinear portion of the horopter curve; and in the latter case it will be a point on the conic section, which happens to be a circle under these circumstances, and is known as J. Müller's *horopter-circle*. And, lastly, should the point of fixation lie in both of the planes above specified, it will be at the point where the rectilinear portion of the horopter intersects the horopter-circle. When we take up presently the mathematical theory of the horopter, more

precise methods of constructing the positions of the horopter-lines will be given.

In one single instance the horopter is a surface, which in fact is a plane; and that is when the point of fixation is in the median plane and at an infinite distance, and the retinal horizons, as is usually the case or practically so at any rate if the eyes are normal, are both in the visual plane. Then this horopter-plane will be parallel to the visual plane, its distance from the latter depending on the amount of divergence of the apparently vertical meridians of the visual globes of the two eyes; that is, it will contain the line of intersection of these two meridian planes and will usually be practically the same as the horizontal plane on which the observer is standing, provided his eyes are normal and directed straight toward the horizon. For near-sighted eyes this horopter-plane is apt to be farther from the visual plane than the floor-plane.

My own interpupillary distance is 68 mm, and the height of my eyes above the floor is 1660 mm. The dihedral angle between the two planes passing through the centres of my eyes and the median line on the floor is 2° 20′ 48″. The angle between the apparently vertical meridians of my eyes is 2° 22′. Dr. KNAPP's vision is normal; his interpupillary distance is 62.5 mm, and the height of his eyes above the floor is 1627 mm; so that in his case the calculated angle is found to be 2° 14′ 20″; whereas the mean observed value was 2° 8′. Professor VOLKMANN is a little near-sighted; the numerical data for his eyes are practically the same as for mine, but the discrepancy between the calculated angle and the observed angle made by the apparently vertical meridians in his case (which was 2° 9′) is a little greater than it was for me. The interpupillary distance of Dr. DASTICH's eyes is 62.8 mm, and the height of his eyes above the floor is 1640 mm; so that the calculated angle in his case amounts to 2° 11′; whereas the angle of convergence of his vertical meridians proved to be greater than this value, being between 2° 33′ and 2° 40′.

I am disposed to think that it is not likely that this relation is responsible for the oblique positions of the apparently vertical meridians. It was shown above that the judgment of the eye in monocular vision was not a reliable means of establishing the positions of these meridians, because angles whose sides do not have the same directions cannot be made to fall on the same places on the retina. Now when both eyes are being used and are pointed toward very distant objects (which is the only way of obtaining consistent results in comparing the two visual globes by their appearance to the eyes), most of the field of view above the horizon is the sky itself, where during the daytime there are no sharply outlined objects; whereas the ground below

the horizon is apt to contain a great variety of definite conspicuous points, which we are absolutely obliged to heed too, when we are walking about and are aware of them by indirect vision. Thus a person with normal vision may get in the habit of connecting those images with the same place in space that happen to fall on the two retinas at the places where the images of a certain point on the ground are usually formed when he is walking along naturally. Persons who happen to be near-sighted do not see the ground distinctly, and consequently their eyes are not trained in this way, and they have to construct the identity-relations for their vision mainly with the aid of objects that are close to them.

Another matter that must be mentioned here is that, when a person holds his body and head erect and looks at a point on the floor-plane which is also in the median plane of the head, the entire floor-plane is not the horopter in this case, but yet the entire rectilinear part of the horopter does lie in this plane.

Incidentally, there seem to be also some individuals for whom the apparently vertical meridians, instead of being quite straight, bend a little around the point of fixation, so that the angle between the upper halves of these meridians is a little less than that between the lower halves. This was the way a student of mine, who was well trained in optical observations, described the phenomenon for the case of his own eyes. Under these circumstances, apparently, the floor-plane does not have any effect except on the lower portions of the visual globes (corresponding to the upper halves of the two retinas), because practically speaking it is not so important for straight lines to look straight in the other parts of the two fields, where an independent identity-relation has been formed by observing surfaces of objects that were steeper.

In the preceding discussion the horopter has been defined as the locus of points which are seen single. In order that lines may appear single, all that is necessary is that the lines on the two retinas where the images are shall be corresponding lines, but the images do not have to correspond point by point. If a second image of a line is shifted along the line itself, it may still correspond to the first image throughout its whole length. This case is apt to occur especially with straight lines, because they can be shifted along themselves and still continue congruent. The surface on which straight line, having a certain definite direction, must lie in order to produce two images that correspond in this fashion, is called a *line-horopter*. For lines on the two visual globes that are apparently perpendicular to the retinal horizons the line-horopter is said to be *vertical*; and for lines that are apparently parallel to the retinal horizons, it is said to be *horizontal*. The line-horopter in the case of lines, whose images on the two visual globes are parallel,

is in general an hyperboloid of revolution, which in certain special cases may become a cylinder or a cone. For those systems of straight lines which all intersect each other in a point on the horopter-curve, the line-horopter will be a cone of the second degree which connects the common point of intersection with all the other points on the horopter-curve.

As a rule, any straight line passing through two points on the horopter-curve will be seen single; and through any point in space as seen with both eyes it is possible to draw at least one straight line that will be seen single. This line may be found as follows. Draw the line of sight from the given point to each eye, and call one of these lines a and the other b'. There is a certain line of sight (b) for the first eye that will correspond to b' for the second eye; and a certain line of sight (a') for the second eye that will correspond to a for the first eye. The line of intersection of the two a, b and a', b' will be the required line through the given point that will be seen single.

I shall proceed now to give the constructions for finding the positions of the *vertical* and *horizontal horopters* in each of the comparatively simple cases referred to above; which will enable us also to determine the horopter-curve, on the assumption that the observer's eyes move in accordance with LISTING's law, and that the retinal horizons in the primary position are practically coincident with the visual plane.

A. *When the point of fixation is in the meridian plane.*—In this case the vertical horopter is a cone, the horizontal horopter consists of two intersecting planes, and the horopter-curve consists of a straight line and a plane curve, which is a conic section.

In Fig. 70 the plane of the diagram represents the meridian plane of the observer's head. He is supposed to be standing erect and holding his head so that the primary positions of the lines of fixation are horizontal and parallel to Ao. From the point o, which is midway between the centres of the two eyes, draw the vertical line oa perpendicular to oA. The lowest point (a) in this vertical line is the point of intersection of the apparently vertical equatorial axes of the two eyes for the primary positions of the lines of fixation. The horizontal plane through a, which contains the straight line DE, will be the horopter when the eyes are gazing far off in the horizontal direction oA. As was stated above, this plane is practically the same as the floor-plane, when the eyes are normal.

Suppose now that the point of fixation is moved to another point B in the meridian plane of the observer's head. Then Bo will be the

line of intersection of the visual plane and the median plane; and, MÜLLER's horopter-circle drawn in the visual plane will pass through B and the centres of the two eyes. Let Bp be the median diameter of this circle, and draw pb perpendicular to Bp. The vertex of the vertical horopter will be on this perpendicular. In order to locate it consider a third point of fixation, designated by C, such that the line Co' bisects the angle $Ao'B$ whose vertex is at the centre o' of one of the eyes; that is, the point o' lies near o on a line perpendicular to the plane of the paper. The visual plane for this point of fixation C will be one of the two planes of the horizontal horopter for the point of fixation B. The other one of these planes will be the median plane. Now draw MÜLLER's horopter circle in the visual plane for the point C, that is, construct the circle which passes through C and the centres of the two eyes, whose diameter, say, is Cq. Then all straight lines will

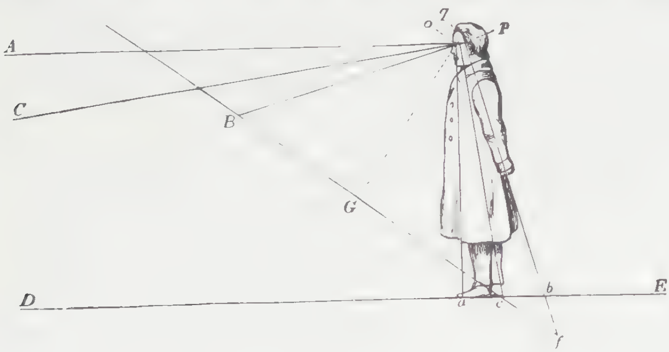

Fig. 70.

be seen single which lie (1) in the plane Coo', or which lie (2) in the median plane and pass through the point q. However, in this latter case the image of the farther end of the line in one eye will correspond to the image of the nearer end in the other eye.

If a straight line is drawn through q perpendicular to Cq and meeting the horizontal line DE in the point c, then Bc will be the rectilinear part of the horopter-line, and the point f, where Bc and pb intersect, will be the vertex of the vertical horopter-cone. The latter, by the way, cuts the observer's visual plane in the horopter-circle whose diameter is Bp; which enables us, therefore, to construct it.

Thus, while one portion of the point-horopter is the straight line Bf, the other portion is the ellipse in which the plane Coo' cuts the horopter-cone.

The section of the cone represented by Bp is a circle in a plane perpendicular to the element of the cone which coincides with the straight line pf. The section, which is perpendicular to the diametrically opposite element Bf, and whose projection in the median plane is the straight line Go, will necessarily be a circle also. The intermediate sections, passed through the centres of the two eyes, and lying between Bo and Go, must be ellipses with their longer axes transversal. Sections, such as Co, that are outside the angle BoG, will be ellipses whose longer axes are in the median plane; or else, in case they happen to cut the line Bf in points lying beyond the point f, they will be parabolas or hyperbolas.

B. *When the point of fixation is in the primary position of the plane of fixation.*—In this case the vertical horopter is an hyperboloid, and the section of it made by the visual plane is one of MÜLLER's horopter circles, which goes through the point of fixation and the centres of the two eyes. The horizontal horopter consists of two planes, one of which is the visual plane, and the other a plane perpendicular to it. The two branches of the horopter-curve are the MÜLLER circle and a straight line.

The two points marked a and b in Fig. 71 represent the centres of the two eyes. The point of fixation is designated by c. The circle abc is MÜLLER's horopter circle and forms a part of the horopter-curve. Now if the straight line fg is the median line of the visual plane, the rectilinear portion of the horopter-line will meet the circle in the point f, that is, to one side of the point of fixation. Draw the diameter cd and the chord fd, and through the latter pass a plane perpendicular to the plane of the circle. This plane will be the second plane of the horizontal horopter. Any straight line lying in this plane and passing through the point d will be seen single; and the same is true with respect to any straight line in the visual plane.

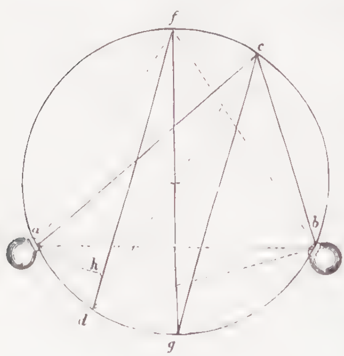

Fig. 71.

In order to construct the rectilinear part of the horopter-line completely, all that is necessary is to lay off on fd a length $fh = fa$, and draw through h a line perpendicular to the visual plane. This line will meet the floor-plane, that is, the unlimited horopter plane for the primary positions of the lines of fixation, in the same point

as the rectilinear part of the horopter-line; and so the latter can be drawn.

In case the deviations of the apparently vertical meridians happen to be zero, the rectilinear part of the horopter-line will be perpendicular to the plane of the circle.

The direction of the line-horopter may be determined experimentally by mounting a straight piece of bright wire or a piece of stretched white thread in front of a dark background in such a direction that, when viewed through two glasses of different colours, it will be seen single; or, which is a better way still, so that, when the convergence of the eyes is varied a little one way or the other, it will be seen in parallel double images. For instance, place a vertical wire a short distance in front of the eyes in the median plane of the head, and let the eyes be directed horizontally toward the middle of the wire; and then it will be found that the upper part of the wire seems to be slightly over to the left in the right eye, and over to the right in the left eye. If the point of fixation is a little beyond the middle of the wire, it will be seen in double crossed images which diverge upwards from each other; whereas when the point of fixation is a little nearer than the middle of the wire, the double images will not be crossed and will diverge downwards from each other. In order that the wire as seen through the two pieces of coloured glass may appear exactly in one single image, or in order that its two images may appear to be exactly parallel, the upper end of the wire should be a little farther from the observer than the middle of the wire. This phenomenon, noticed first by BAUM, was afterwards employed by MEISSNER, as above stated, to investigate the rolling movements of the eyes. Thus whenever the angle between apparently vertical corresponding lines is altered by rolling the eyes, the inclination of the wire to the visual plane has to be altered also, if it is to continue to be seen single. And the farther away the point of fixation is, and the more the plane of fixation is elevated, the more the wire will have to be inclined to the visual plane. On the other hand, in looking downward at a near point of fixation, the wire may be perpendicular to the plane of fixation or the upper end may even be a little toward the observer.

Having thus determined the relations of apparent equality and inequality on the visual globes of the two eyes, we must proceed now to investigate the *accuracy* of such comparisons. As was explained in the previous chapter, this accuracy is very high, when it is simply a question of perceiving differences of depth in an object as ordinarily observed by the eyes. On the contrary, when it comes to detecting double images or making comparisons between the positions of the

images on the visual globes of the two eyes, our judgments are not only comparatively unreliable, but they are subject to a great many visual illusions. And yet it might be supposed that this latter process was simpler than the former, especially, too, as the estimation of stereoscopic relief necessarily involves various factors of experience. But just because this judgment is of such enormous practical value, it is much more highly trained than the faculty of perceiving double images and their mutual positions, which are of importance so far as the appearances of objects are concerned, but have little to do with the objects themselves. Similarly, our judgments of the actual dimensions of two objects at different distances are apt to be much more reliable than our estimates of their apparent sizes, although the visual angles which they subtend involve direct comparisons between equal or unequal lengths on the retina, whereas it takes a long course of learning by experience to gauge the effect of distance on the size of the retinal image of a given object.

In the first place, with reference to the estimation of depth by binocular vision, with the exception of certain illusions already mentioned, which are due to incorrect judgment of the convergence of the visual axes, this is most accurate in the case of those objects that happen to be on the horopter, where they are seen exactly single. It is not quite so accurate for objects, which, although they are not on the horopter, are so close to it that the double images are not yet recognized as such; and it becomes more and more unreliable as the double images get to be more distinctly separated, being worse in proportion as these images are farther apart.

Elsewhere[1] I have already called attention to the fact (which has been likewise confirmed by E. Hering[2]), that the double images by no means appear to be at the same distance as the object of fixation (as used to be assumed), and are projected as if they were on some imaginary horopter-surface that had to pass through the point of fixation. But the double images do appear to be very nearly at the correct distance of the object which is responsible for them. This may be easily verified by a simple experiment. Keep the eyes steady and look at a point on a wall a few feet away, at the same time holding the upper edge of a piece of cardboard on a level with the eyes several inches away, so as to hide all objects in front of you below your visual plane. Now get somebody to stand on one side at any distance he pleases, and insert a knitting needle up from below, until you begin to see the upper end of it, in double images, of course, since you are

[1] *Archiv für Ophthalmologie.* X. 1, p. 27.

[2] *Beiträge zur Physiologie.* Heft 5. p. 335.

supposed to be gazing steadfastly at the point on the wall. Without once changing your point of fixation, and without having seen the needle single, you will immediately have an idea of its distance from you. You can test it by trying to grasp the needle without seeing your hand, and if you do not succeed the first time, you will at least come very near doing so. In order to prevent any possibility of your judgment being based on the apparent diameter of the needle (which is not apt to be the case) you may have an assortment of needles of various sizes, any one of which can be selected at random.

In experiments with moving stereoscopic objects, whose apparent distance from the spectator varies (as in HALSKE's apparatus described on page 360), it often happens that we get distinctly separate double images, especially if the motion is too swift for the eyes to keep pace with it, although this velocity does not interfere at all with the illusions in regard to the apparent depth-movements.[1]

Binocular perception of depth persists until the double images get to be very far apart, as they do especially when the object is a long way off and the eyes are focused on something near at hand, so that scarcely any connection between the two images can be any longer perceived. Then, as in the case of monocular vision, the apparent size of the remote object may be compared with that of the object on which the eyes are converged. However, if the observer happens to be aware of the real linear dimensions of the latter, he may unconsciously use this object as a standard for measuring the image of the more distant one. For instance, suppose a person is standing at a window and looking toward a house across the street from him; then if he holds his finger in front of his face and focuses his eyes on it, he will see two images of the building far apart, and the house itself will appear to be bigger or smaller, according as he moves his finger farther away or nearer to him. In this case the finger is the constant standard of comparison, because its linear dimensions and distance are continuously distinct in the perception, whereas this is not the case with the distant house.

Thus when the double images happen to be far apart, there is increasing uncertainty about binocular perception of depth; and, conversely, when the objects are seen absolutely single, or nearly so, the closer they are to the horopter, the easier it is to tell about the relief (except, of course, for the illusions which were mentioned above).

This can be verified, so far as the rectilinear part of the horopter is concerned, by taking a straight, slender knitting needle and bending

[1] Concerning this matter, see Note 2 at the end of the chapter.—K.

it slightly in the middle at an angle of about 175°. Then hold it in front of your eyes with the plane of this angle in the median plane of the head, so that the needle would appear to be absolutely straight to an eye which was located on the bridge of the nose, and so that even to each of the actual eyes by itself the bending would be quite unnoticeable on account of the strong foreshortening in perspective. Yet by looking at the needle under these conditions with both eyes at the same time, the bend in it can be detected, in case it happens to lie nearly in the same direction as the rectilinear portion of the horopter, so that, when the eyes are converged on a point a little nearer or farther off than the needle, the latter will be seen by double images that are practically parallel. However, if the needle has some other direction in the median plane, so that it makes quite an angle with the rectilinear portion of the horopter line, the bend in it will not be perceived.

For MÜLLER's horopter-circle I made the experiment in the following fashion. Two small strips of wood were laid on a table side by side, and the observer was required to take a position such that his eyes could just see over the edge of the table. In one of the pieces of wood two long, slender knitting needles were mounted vertically side by side, about a centimetre apart; and another similar needle was stuck in the other strip of wood. The three needles stood in a row at equal distances from each other and approximately at equal distances from the observer, that is, each about half a metre away. Owing to the interposition of a screen, the observer was not able to see anything more than the tops of the needles. The experiment consisted in finding out how far the lateral needle could be moved forward or backward, before the observer could detect that they were not all three in one plane. I discovered that it was not necessary to shift one of the needles through a distance more than half of its diameter, or about a quarter of a millimetre, before I could discern that the three needles were not in a straight line, but formed an arc. The angular difference in the position of the middle needle as compared with the positions of the two outer ones amounted here to only 21 seconds of arc.[1] However, in order to get this high degree of accuracy, it was necessary that the direction of the row of needles should correspond to that of the horopter-circle at that place. Thus when the needles were directly in front of my eyes, the middle one being in the median plane of my head, and the two outer ones being equidistant from me, I could tell with more accuracy whether they were all in one plane. But when the needle on the right was a little nearer to me and the one

[1] ¶It is not exactly clear how this was computed from the data. (J.P.C.S.)

on the left a little farther away, it was not nearly so easy to decide whether all three needles were in a straight row or in a curved line. On the other hand, when the middle needle was a little to the right of the median plane, where the right-hand portion of the horopter-circle begins to come toward the observer, it was necessary to move the right-hand needle a little closer than the left-hand one, in order for me to have the greatest certainty about my judgment of the relief of the row of needles. And if in this position the row of needles happened to be perpendicular to the direction in which the eyes were looking at the time, I found that then it was much harder to tell whether the needles formed a straight line or a curved arc. Thus the best position always was when the row of needles was adjusted so as to be tangent to the horopter-circle.[1]

In this experiment, it should be noted, the needles must not be too far apart, else the observer may be subject to the illusion which makes a horizontal concave arc appear to be straight. For most observers, with the needles spaced as above described the depth of the arc would have to be less than one-tenth of a millimetre in order for it to look like a straight line; which is much smaller than the perceptible differences of depth.[2] And even when the needles are far enough apart to make the illusion apparent, it will be found that the range of displacements between the impressions of convexity and concavity is very much less when the row of needles conforms to the direction of the horopter-circle than when it makes an angle with it.

When the eyes are directed straight ahead toward a point on the horizon, the horopter will be a horizontal plane below the visual plane. For persons with normal eyes, generally it will be actually or practically coincident with the level plane on which the observer is standing. When the point of fixation is in the median line of this floor-plane, although this entire plane will not be the horopter, the rectilinear part of the horopter-line will then be wholly in this plane. I notice corresponding phenomena on this floor-plane which lead me to infer that here too judgment of relief in the floor-plane is particularly accurate, because it is an horopter-surface. This can be tested by standing in a level meadow and first observing the relief of the ground in the ordinary way. There may be little irregularities here and there, but still the surface appears to be distinctly horizontal for a long way

[1] Mr. E. Hering's criticism of this experiment implies that he completely failed to understand its meaning.

[2] In my earlier work on this subject, it was stated that a curve coinciding with the arc of the horopter-circle would appear straight. This statement was due to making the measurements with needles that were too close together. As a matter of fact, the curve is much flatter than the arc of the horopter-circles.

off. Then bend the head over and look at it from underneath the arm; or stand on a stump or a little elevation in the ground, and stoop down and look between the legs, without changing much the vertical distance of the head above the level ground. The farther portions of the meadow will then cease to appear level and will look more like a wall painted on the sky. I have frequently made observations of this kind as I was walking along the road between Heidelberg and Mannheim. There in front of me, beyond a succession of fields, was the Neckar, interrupting an otherwise level plain that extended a mile or so away on the other side of the river toward the Olberg at Schriesheim. When my head was erect, I could see perfectly well the widespread level beyond the river; but when my head was tilted on one side or inverted, the impression I got was that the ground ascended abruptly from the river to the Olberg there high above me. There used to be a gate which was separated by a yard from the house beyond it; and ordinarily that was the way it would appear. But when I tilted my head to one side and looked at it, it seemed to be close against the house. I mention this as just one instance of many such appearances. Moreover, the little unevennesses in the road were much more distinctly perceptible when my head was held erect.

All these effects can be produced just as well by inverting the image instead of turning the head round. The best way of doing this is with a right-angle prism, as shown in Fig. 6. If the hypothenuse-face is horizontal, objects seen through the prism will appear to be upside-down. I took two prisms of this sort and fastened them on a little piece of board with an interval between them corresponding to my interpupillary distance. When I viewed a landscape through this contrivance, I discovered that the stereoscopic relief of the ground vanished in this case just as it did when I stooped down and looked at it between my legs. Sometimes, however, the relief of low-lying clouds in the sky can be seen better through the prisms than without them, because the effect of the prisms is to make the clouds appear where the ground ordinarily is.

And, lastly, if one will stoop down and look between his legs, using the prisms at the same time, the ordinary appearance of the ground with its distinct relief will be restored, just as it looks under natural conditions. In this case the reflected image of the ground will again be in the horopter of the inverted eyes. This last experiment proves that it is not the extraordinary position of the head by itself nor the unusual orientation of the image that is responsible for the want of accuracy in the perception of depth, but it is the inverted position of the image with respect to the eyes.

Moreover, these results are in accordance with the explanation given by Mr. E. HERING.[1] In the case of his eyes there is a very slight deviation of the apparently vertical meridians, and he states that the more remote parts of the level ground look exactly the same way to him with both eyes as with one eye.

It is evident how essential it is for us to have the right perception of relief when we are walking. Usually, we proceed without looking directly at the ground at all, and yet we are sufficiently aware of the little unevennesses in its form. Recently, I have often had occasion to notice how very annoying even an exceedingly slight apparent displacement of the image of the ground can prove to be. I was off on a tramp in the mountains, and, being a little near-sighted, I was wearing a pair of eye-glasses with very weak concave lenses (focal length 3 feet), in order to enable me to see the distant scenery better. I had been careful to have the glasses made so that the centres of the two lenses were at the same distance apart as the interpupillary distance of my eyes, and consequently when I looked centrally through the glasses at distant objects, there was no visible distortion of depth, such as occurs when the lenses are too close together. Still the axes of the lenses were not exactly parallel, due to the bridge-connection between them, and I observed some slight displacements of objects when they were viewed through the lower portions of the glasses. Thus when I looked directly at the ground, there always seemed to be a faint elevation just in front of my feet, on account of some false stereoscopic action; and though this effect was so slight as to be hardly noticeable ordinarily, I found it was hazardous to wear the glasses when I was walking fast down a rough mountain path where it was necessary to be very sure-footed, and where I did not have time to pause and consider each stone on which I was about to step, and to estimate the distance. It is true, I could see the stones a little more distinctly with the glasses than I could without them; but yet I found it safer to remove them. This struck me as a remarkable proof of the sureness and promptness of the operation of the trained association between the indications of the senses and the movements of the body.

The apparent modification of the colours of the landscape, which is noticed when the head is held in some unusual position, seems to have some connection also with the change of relief that takes place under the same conditions. As long as the dimensions of depth are clearly distinguished, the modifications of the colours of objects by the intervening atmosphere will simply be the natural and usual attributes

[1] *Beiträge zur Physiologie.* Heft 5, p. 355. It is scarcely necessary for me to say that the floor plane does not appear to me like a vertical plane, which is the inference he makes from his theory.

of distance, and will therefore make no particular impression on us. But the moment the effect of the relief is disturbed by the inversion either of the head or of the image itself, and the landscape has the appearance of a flat picture, our attention will be immediately attracted to the colours.[1] Even with one eye shut, some slight effect of this kind may be detected in looking at a landscape, first, with the head erect, and then with the head tilted under one arm. Apparently, the explanation in this case is that the upper part of the retina has become fatigued for the green colour of the grass on the ground, and the lower part for the blue colour of the sky above; and so when the retina is inverted, the colours seem to come out more vividly. Still this peculiar emergence of the characteristic atmospheric hues of distant objects I find is not very distinct except when they are viewed with both eyes at the same time. In Mr. HERING's discussion of this subject, as usual, he makes no distinction between monocular and binocular vision.

The reason for this exceptional accuracy of the relief for points that are on the horopter is given by FECHNER's psycho-physical law, as E. HERING supposes also. The apparent distances of objects on the horopter from the point of fixation are equal; and the slightest discrepancies in this relation of equality may be readily and exactly detected. When the given object is not absolutely on the horopter, there will be a discrepancy of this kind. But when we have to decide about the form of an object that is not on the horopter, this involves the question of the relations between the distances of the double images of its various points, and not simply the question as to there being some difference between the two parts of the double image. According to our theory, corresponding points on the two retinas are those whose mutual positions have been most frequently compared and found by experience to be associated with the same point in space. From the anatomical point of view, the correspondence between points on the two retinas is due to their having some natural connection in their localization. Either assumption will account for the fact that the comparison between images on the two retinas that are corresponding or nearly corresponding is better and more reliable than it is between so-called disparate images.

This is the reason why we are in the habit, without knowing it, of adjusting objects as nearly as possible on the horopter, when we want to examine them conveniently and accurately. Thus, when on holding a book in the most convenient position for reading, double images are formed of the vertical lines, which diverge slightly from each other,

[1] ¶This observation may explain the abnormally vivid appearance of the colours in the inverted image focused on the ground-glass plate of a camera. (L.D.W.)

these images will appear to be parallel, that is, the vertical horopter will be in the plane of the paper. Of course, if the person's eyes happen to be accommodated for infinity, the horizontal lines in the paper will not be on the horopter; and this may be the reason for the marked preponderance of vertical lines over horizontal lines in the characters of European alphabets.[1]

The other method of comparing the visual globes of the two eyes consists in observing the apparent distribution of the objects in the common binocular field and trying to perceive the *double images*. It was expressly stated above that, generally speaking, it is only in the central parts of the visual globes that we are able to get a good perception of the double images. There are very gross inaccuracies in them in the peripheral parts of the two fields. But the main difficulty about perceiving the different positions of the two half-images of one and the same object is the idea we have of the unity of this object which they represent. We have tried to show that it is likely that our metrical estimates of the visual globes depend on the training of the eye; and if this is so, the perception of the double images will depend on the eyesight also, and, like all judgments by the eye, it may be exceedingly erroneous, owing to all kinds of psychic influences, especially those that tend to give us the impression, whether right or wrong, that the two images pertain to the same object. Consequently, it is extremely hard to notice the difference between the two images of real material objects when they are not very large or conspicuous, and so in this case most persons fail to perceive the phenomenon of double images entirely, although such images must have been almost continually before their eyes. There is difficulty also in separating the double images of lines of the same colour and luminosity which happen to be so situated that they might easily be supposed to be images of one and the same line in the object. But the hardest exercise of all is to perceive the double images by movements of the eyes. When we look at an object, we focus our eyes successively on the various parts of the surface, thus continually forming pairs of corresponding images in the foveas of the two retinas. At the same time these parts of the image, being perceived most distinctly, will rivet our attention most. The moment the attention is distracted to some point of the object off to one side, almost involuntarily our eyes will turn to look directly at it; and it takes special care and will-power to prevent them from doing so.[2]

[1] ¶The following reference may be inserted here: E. Kaila, Versuch einer empiristischen Erklärung der Tiefenlokalisation von Doppelbildern. *Zft. f. Psychol.*, 82 (1919), 129-197. (J. P. C. S.)

[2] Concerning methods of making the double images apparent, see Note 3 at the end of this chapter.—K.

And so when we want to see the double images as well as possible, we must take pains, in the first place, to avoid moving the eyes and to look steadily at some clear definite point of fixation. Secondly, it helps us to separate the two images when their colour and luminosity are different, because then we are not so apt to think of them as being images of the same thing. Lastly, by partially concealing one of the double images, or by adding some peculiar mark to call the observer's attention to it, all sorts of other inequalities between them can be produced, to aid us in making quite nice discriminations in separating the two images.

These difficulties may be avoided, and apparently equal dimensions on the two visual globes can be compared as accurately as possible, by using the methods described above for finding the positions of corresponding points and lines. And yet none of these methods will enable us to make nearly as accurate a comparison between corresponding dimensions in the two fields of view as can be made in each field by itself.

Volkmann's experiments, as described above, enable us to obtain some precise numerical data on this subject. In the experiments described on page 413 (see Fig. 66) the vertical distances between two pairs of horizontal lines were compared with each other, one pair being located in the right-hand visual globe to the right of the central vertical line, and the other pair being located in the left-hand visual globe to the left of this line. In the common binocular field the two pairs of lines appear to unite on the central line. The interval between one pair of lines was constant and equal to 5.5 mm, and the object of the experiment was to try to adjust the interval between the other pair of lines so that it would be equal to that between the pair of fixed lines. Volkmann obtained average results, from each set of thirty observations, which were indeed quite concordant, the differences between them and the correct value being between 0.01 and 0.03 mm. But when we examine the individual observations, we find that in the first set of experiments (where the pair of adjustable lines was on the right) there was one instance in which the interval of 5.5 mm was put equal to 6.0 mm, and another case in which it was put equal to 5.0 mm; and in the other set of experiments the values 5.0 and 5.85 mm occur. In the experiments with vertical lines, the interval of 5.2 mm was made equal to 5.55 mm and 4.75 mm in two observations of one set, and then again to 5.55 and 4.85 mm in two observations of another set.

Now such large errors would certainly be impossible when the two pairs of lines were observed side by side and touching each other in the same visual globe. The main reason for the difficulty in binocular comparison, it seems to me, is because it is so hard to keep the fixation

absolutely steady, the result being that there are incessant little variations in the overlapping of the two fields. In order to test this, two parallel lines were drawn on a sheet of paper extending clear to the edge of it, the interval between them being 5.5 mm. On another sheet of paper two lines were drawn which were not quite parallel being 4.5 mm apart at one edge of the sheet and 6.5 mm at the other edge. The two pieces of paper were then fitted together, one partly on top of the other, so that the pair of converging lines appeared to form the continuation of the pair of parallel lines. Then continually moving the upper sheet slightly to and fro, so as to imitate the fluctuations of the visual globes, I tried to decide with *one* eye whether the converging lines were the same distance apart as the parallel lines at the place where the former emerged from under the latter. Thus the two pairs of lines were both in the same field of view, and the fluctuation of the axes of the eyes as it takes place in binocular vision was imitated by the movements of one pair of lines. On the other hand, the pair of converging lines could be partly covered by a sheet of white paper, and then, with both eyes open, the portion of these lines which was still visible could be brought into contact with the pair of parallel lines, as in VOLKMANN's experiments; so that the two pairs of lines were juxtaposed in the common field of view, and one appeared to be the continuation of the other. There was a little advantage in this method over that used by VOLKMANN, in which one line of each pair was drawn all across the field and coincided with the corresponding line; whereas in my experiments, as also in the one described on page 420 (see Fig. 68), the two lines did not overlap and coincide with each other at all, but merely appeared to be the prolongations of each other. Differences in the distances between the two pairs of lines amounting to as much as half a millimetre could always be easily detected, and even a difference of half that magnitude could scarcely fail to be seen. The result was I found I could make the comparison of the corresponding intervals in the binocular field about as accurately as I could compare the same intervals on the visual globe of one eye, provided in the latter instance I tried to imitate the fluctuations of the two visual globes with respect to one another by continually moving one of the drawings to and fro.

The individual errors in VOLKMANN's comparisons of the direction of a line on the visual globe of one eye with that of another line on the visual globe of the other eye are also remarkably large. The differences from the average frequently amounted here to as much as half a degree, sometimes to as much as a degree. In monocular vision, however, it is altogether out of the question to make the mistake of supposing that a line is straight when the two portions of it meet at an angle of

179°; and in fact when the angle was 179.5°, it could hardly fail to be detected. In monocular vision it would be still less possible to make the mistake of supposing that two adjacent straight lines were parallel which were inclined to each other at an angle of one degree or even half a degree. But such differences as these may be overlooked in comparing the visual globes of the two eyes, and the only reason for it, so far as I can see, must be due to the fluctuations in the magnitude of the torsional rotations of the two eyes, which, as I have stated above, can be perceived by means of after-images. There is nothing surprising about our obtaining a pretty accurate result as the average of a large number of observations, even if the individual observations should indicate values that are wide of the mark on both sides.

Hence, the chief reason why our estimates of depth-dimensions of actual objects are so much more reliable may be because our training in converging the two eyes and making them traverse together the outline of some object of familiar material shape happens to be extremely highly developed; whereas we are not much in the habit of trying to keep the fixation steady with unequal images on the retinas of the two eyes.

In this connection I must speak of a matter which I have often noticed. When I am trying to see a stereogram which is hard to fuse, it takes some effort to make the corresponding lines and points coincide, and with every movement of my eyes, they are apt to separate again. But the moment I have succeeded in obtaining a vivid perceptual image of the material shape that is intended (which frequently flashes out as if by some lucky accident), thereafter my two eyes will glide over the figure with perfect sureness, without the slightest risk of the two images being separated again. Along with the perceptual image of the form of the body, we obtain also the law that governs the mode of motion of the lines of fixation when we observe the object that is thus presented to us; and I think the question may properly be asked whether after all there is really anything more in this visual apperception of material form than this law which governs the movements of our eyes. However, this much can be said at least, that if we believe that the ability of measuring the visual globes has been acquired from experience by the movements of our eyes, then this question will have to be answered in the negative.[1]

We proceed now to study those conditions which tend to limit the accuracy of our comparisons of the visual globes of the two eyes; as, for instance, when images coincide which are depicted by non-

[1] ¶Reference may be made here to A. Basler, Über die Verschmelzung von Formen. Pflügers *Arch.*, 167 (1917), 184-197. (J.P.C.S.)

corresponding points on the two retinas; or, again, when images which are depicted on corresponding points, appear to be at different places in the field of view.

The main reason for the fusion of images on so-called disparate points of the retina is the similarity between them to the two perspective images of one and the same object. The more perfect this kind of similarity is, the harder it will be for us to get rid of the idea of the single object in space, and, independently of that apperception, to compare the arrangement of the points and lines as seen on the visual globe and their mutual distances apart.

Consider, for example, the two pairs of vertical lines *E* on plate II. If the right-hand line of each pair of lines is focused by the eye opposite each pair, the resultant binocular image will consist of a couple of lines, of which the one on the right will appear to be a little lower than the one on the left. Under these circumstances the two images of the line on the left cannot be on corresponding places on the two retinas, because the two lines of the right-hand pair are 3.5 mm apart, and those of the other pair are only 2.7 mm apart, that is, the interval between them is 0.8 mm less than that between the right-hand pair. Nevertheless, I find it almost impossible to discern that either of the two lines, which appear to stand obliquely one back of the other, appears double. It is only by looking with perfect steadiness at one of the lines that I can see some indications of its being double. Perhaps there are some individuals who can see the double images easily even in this case; just as there may be others who cannot see them at all, for there are very great individual differences in this respect.

The difference between the intervals of the two pairs of vertical lines in the diagram *H* on Plate II is larger (the intervals themselves being 3.7 and 7 mm, and the difference being 3.3 mm). When they are fused stereoscopically, I can manage to see here also a single pair of lines, one far behind the other, but the double images of one of them, perhaps also of both of them, never do disappear completely in this case, because the distance between them now is comparatively too large.

In the diagram *J* on the same plate the difference of the intervals between the two pairs of vertical lines is likewise quite considerable (6.7 and 9.2 mm, with a difference of 2.5 mm), but yet it is less than it was in diagram *H*. Stereoscopic fusion in this case is facilitated too by the connecting links between each pair of lines both above and below which produce the perspective view of a rectangular slab. The difference between the two intervals in this diagram is sufficiently great for me to execute the required stereoscopic fusion easily and perfectly, and yet by making a slight effort I can perceive too the

double images that are present. In this latter case if I gaze steadily at one of the vertical lines, I can see the other one double; and it is easier to see the double image of the shorter line on the right-hand side of the total stereoscopic image than it is to see that of the longer line on the left-hand side. When I gaze steadily at the right-hand line in the total image, and then very gradually increase the convergence of my eyes, by gently and cautiously making the requisite muscular effort (as I have learned to do by long practice), I am able to resolve the right-hand line in the resultant image into two very slightly separated images (the interval being about 1 or 1.5 mm); and then the vertical line on the left will appear double too, as I can succeed in seeing momentarily. But without having a fixation-mark, it is very difficult to hold the eyes steady for some time in an adjustment of this sort, and the incessant fluctuation of the lines of fixation is manifested by the continual variation of the interval between the double images of the right-hand line. It is easier for me to keep my eyes steady in looking at diagram *H* when the pair of lines on the left appears to be entirely in between the pair on the right, and all four lines are seen single. If the observer has sufficient control over the movements of his eyes, he can make the two images coincide in any desired position, and generally he can perceive double images too in each position, provided they do not fall entirely too close together.

I have even learned to tell what to do in order to see the double images or not to see them. Thus when I do not want to see them, I try to judge by my eye how much farther from me the right-hand line of one of the stereograms *E*, *H* or *J* is than the left-hand line; in other words, I pay attention to the depth-dimensions. If I want to see the double images, I try to imagine the form which the combined image would have if it were drawn on the paper; for instance, what the horizontal distance between the vertical lines would be if it were measured in the plane of the paper, etc. This whole matter seems to me to be like the different interpretations of the shape of the surface of an object, such as a cube, for instance, which is placed in front of the eyes in some oblique position. Thus I might wonder whether the faces of the cube were really perpendicular to each other, or whether the edges were all equal, as might be ascertained with some degree of accuracy even by an oblique view. Or I might wish to draw the cube on a sheet of paper, and see how its faces would look as parallelograms on the visual globe. In this case I would not notice how much larger the angles were that look obtuse than those that look acute, or how much longer one of the diagonals on a side was than the other, etc. If the sides of the cube are much distorted by perspective, I may be able to realize clearly that the angles are all equal and all right angles,

and yet not quite be able to get rid of the idea that the three right angles meeting at one point in the picture are unequal angles which together make four right angles. But if the view is just a little oblique, maybe even with the closest attention and after much practice I shall still be unable to detect any apparent difference in the size of the angles on the visual globe. For instance, this is the case when my eye happens to be on the prolongation of one of the edges of the cube, that is, when there is practically only one side of the cube in front of me, and it is not inclined much to the line of fixation. Anyhow, we can tell correctly the form of a real object much better than its appearance in the field of view; and this is one of the main reasons why it is so hard to make a sketch of a body.

It is exactly the same way with the apperceptions of depth in the field of view and with the double images. Suppose I am thinking about the depth-relations; then I know by experience that differences in the distances of corresponding points in the two retinal images are the visual tokens of a certain definite dimension in space in the object itself, and any such difference will not be urged on my attention unless it is very marked. Thus when the perspective distortions are quite large, it is impossible to ignore entirely the apparently rhomboidal form of the faces of the cube, even though at the same time we perceive correctly that they are really squares.

But then again suppose my attention is directed to the appearance in the field of view. Now I shall notice differences between the two images which I had overlooked before. But the perception of depth may intervene and betray me into overlooking minor differences in the two aspects of the body; just as the perception of the real form of the cube cannot make me blind to slight perspective distortions of its surface. Here, as before, it is a question of recognizing the difference of certain dimensions in the field of view which we know by experience to be the visual expression of equal magnitudes in external space; only, in one case the two magnitudes that have to be compared with each other are on the different visual globes of the two eyes, whereas in the other case they are both in the same field of view.

Incidentally, the best way of getting the stereoscopic depth-impression with the stereograms H and J is to let the eyes traverse the interval of depth from one end to the other. But the effect can also be obtained without moving the eyes at all, only it will not be so impressive. From time to time double images will be seen in this case, my experience being that the fixation of my eyes then is such as to make the centres of the two figures coincide, while the two vertical lines in the combined image will appear double. This is the adjustment for which the interval between the total double images is least.

Moreover, it is easier to see the double images when there are some incongruities between the two pictures that are to be fused, no matter how slight they may be, provided they tend to destroy the notion that the pictures are views of the same solid object. Volkmann has observed that all we have to do is to cover half of one of the lines in stereogram E with a white card, or draw two horizontal lines at different levels in the intervals between the two pairs of vertical lines, thereby making two slightly dissimilar figures each of which resembles the letter H. Another method is to draw one of the patterns so that it is a pair of white lines on a black ground, and to draw the other pattern in just the opposite way (as shown in stereogram P, Plate IV); which makes it difficult or impossible to fuse them stereoscopically. The diagram G, Plate II, is an exact copy of diagram E, except that on opposite sides of the right-hand line in each pair of lines two dots have been added at equal distances from the left-hand line.[1] If the two dots are fused by gazing steadily at them, instantly the two adjacent lines will appear separated; for one of them being to the right, and the other to the left, of the point of fixation, the difference is far more impressive than it would be if both lines were on the same side of this point and merely unequally far from it. But if the eyes are focused on the left-hand line in the compound image, instead of being focused on the dot, the dot will appear single too, but now the right-hand line, apparently exactly behind it, may be seen double without much trouble. It is impossible not to notice here that the right-hand member of the pair of lines on the left seems first to be nearer than the dot, then farther; and then we perceive that the dot is at the same distance from the left-hand line in both cases, but that the right-hand line is not. Thus, by a sort of contrast-action, the dot, which ought to appear in the plane of the paper, stands out in front of it, as if it were a little nearer the left-hand line on the right-hand side, and a little farther from it on the left-hand side.

Points which are at somewhat different vertical distances above or below the retinal horizons may be made to fuse stereoscopically also. For instance, the two pairs of horizontal lines in stereogram F, Plate II, can be made to coincide, although the interval between the right-hand pair is 3 mm, and that between the other pair is 3.7 mm. A case analogous to this is to be found in looking at actual objects, wherever two horizontal lines are located on one side of the median plane. The lines being nearer one eye than the other, their separation appears more to the former eye than to the latter. However, in looking at real objects, differences in vertical distances are apt to be small as compared

[1] ¶These dots do not show up in stereogram G. (L.D.W.)

with those that occur between horizontal distances. Apparently, this is the reason why pictures cannot be fused unless their vertical dimensions are very nearly the same. As we continue to gaze at the stereogram F, the fusion of the two pairs of lines soon ceases; and this is found to be the case even when the intervals are much less different.

Another point to be noted is that so-called disparate images can be fused not only when they are on the excentric parts of the retinas, but even when they are in the foveal regions or in the foveas themselves. When the two crosses in stereogram L, Plate III, are fused by gazing steadily at the centre of the binocular image, the two vertical lines to the right of the crosses will necessarily be fused also into what seems to be a continuous line. I find that the same thing is true too when I gaze very carefully and exactly at the centre of the cross itself, although it is not always so by any means, unless I am exceedingly careful about the fixation. Sometimes the upper vertical line, or maybe sometimes the lower one, will appear to be farther from the cross, and the interval between the two vertical half-lines may perhaps amount to as much as a millimetre or more, without my being able to see any double image of the cross itself. If I converge my eyes and look first at the card itself, and then turn my eyes apart until the two crosses come together, I find that the upper half of the vertical line belonging to the right-hand picture generally continues farther away from the cross than the lower half. This indicates that the convergence of the eyes has not been completely relaxed. But I can deliberately reduce the convergence of my eyes beyond this (although they will still be convergent, because my interpupillary distance is 66 mm, while the interval between the two pictures is 63.5 mm), and then the upper half of the vertical line will be nearer the cross than the lower half. It is easy to compare the vertical half-lines here, and their fluctuations show that they are due to quiverings in the positions of the eyes, such as are not revealed by the double images of the apparently fixed vertical arm of the cross. This is a point that might well be noted in experimenting with double images. We have no right to assume that the two retinal images of a point are precisely corresponding points, unless the fixation is more accurate than it is ordinarily. And so with respect to the stereograms E and F, Plate II, I find that I am always apt to focus these pictures so that the narrower pair of lines falls entirely within the wider pair. All that is necessary in order to prove that this is so, is to cover the half of one pair of lines with a white card.

Originally, I had intended to use a stereogram similar to L, Plate III, for determining the lengths of corresponding segments on the horizontal line; but it proved to be altogether unsuited for the purpose, because, even when the displacements of the vertical lateral vertical

lines were quite large, the vertical line of the cross continued to appear single. As a matter of fact, the experiment succeeded much better when the upper half of the vertical line of the cross was erased also in one figure, and the lower half in the other figure.

It is even possible to fuse a vertical line in one picture with two vertical lines approximately corresponding to it in the other picture. In stereogram *T*, Plate V, there are two lines on the left and three on the right. If the right-hand lines in the two groups are made to coincide exactly, the image of the left-hand line of the group on the left will fall in between those of the two left-hand lines of the group on the right, and be fused with them. And so we get here the impression of a total image of three lines, the one farthest to the left being nearer the observer, and the one closely adjacent to it being farther from him, than the line on the right. The three lines appear to be the edges of a right triangular prism, and, in fact, they do form precisely this figure as it would appear if the prolongation of one of the sides of the prism passed through the observer's left eye. In order to see where the image of the single left-hand line is, its centre has been marked by a black dot. When I fixate the right-hand line of the total image, this dot falls alternately on one or the other of the pair of corresponding lines, or else it falls in between them. This shows that the convergence fluctuates.

Thus too, as is intended to be shown by the stereogram *R*, Plate V, one circle can be fused with another that is slightly larger or smaller than it. This agrees with the real case in which an observer views a circle (or sphere) which is situated over on one side of his median plane, being nearer one eye than the other. In this case there is little trouble in fusing the vertical portions of the images of the two circles and in keeping them fused for quite a while. But the horizontal arcs have a tendency to separate, unless the two circles are practically of the same size. The point of fixation in this stereogram should be at the centre of the total image. In performing this particular experiment I happened to catch myself turning my head, without knowing it, over toward the larger circle; the effect of this being to make the two circles appear to be nearly the same size. Of course, the fusion then was very much more perfect. But when we try to fuse one circle with two other circles, one of which is a little bigger than it, and the other a little smaller (see stereogram *S*, Plate V), we find, indeed, that there is no particular trouble about fusing the parts that are practically vertical; and usually the isolated circle will fuse with one side of the larger circle and with the opposite side of the smaller one. But above and below the circles do not fuse, and connecting arcs of the single circle may be seen passing from the larger circle to the smaller one.

The result is that two circles will be seen in the total image which appear to be united by a connecting link above and below, but this connection is not very distinct but confused. The right-hand side of the interior circle appears to be behind the outer circle, and the left-hand side in front of it, due to a stereoscopic action similar to that which takes place in connection with the vertical lines in stereogram T. Here also the extent of the fusion depends on our being able to see some resemblance to real objects in the combination of the two pictures. Where such resemblance cannot be traced, the two pictures tend to separate.

VOLKMANN[1] has made some measurements on the limiting values of the differences that are just perceptible in stereoscopic vision. He examined with a stereoscope two pairs of black lines (a, b and c, d) on a white background. The line designated by d consisted of a strand of hair stretched in a frame. The latter was adjustable, so that this line d could be shifted toward the other lines or away from them. In the initial position the frame was adjusted so that when the lines a and c were fused in the stereoscope, the lines b and d would be fused also. Then the line d was shifted either toward the adjacent line c, or away from it, until it parted company with the line b of the other pair with which it had been originally fused. The effect of the lenses in the stereoscope was equivalent to viewing the lines at a distance of 150 mm.

From my own experiments as described above, I believe it can be assumed here that, when the observer was required to focus his eyes steadily on a single line in the resultant image, he really had to focus them so that, supposing he could have distinguished the double images, both lines would have been seen in double images at nearly equal distances apart; and therefore that the real distance of the fused double images may have been only about half as great, or a little more than half as great, as the differences between the two intervals that were compared with each other.

A summary of VOLKMANN's results is given in the following table. Each of them is the average of fifteen measurements. The values of the interval cd are for the extreme distance it was possible to separate these lines and still fuse them with ab. The measurements are recorded in millimetres.

These results indicate considerable individual variations not only between different observers, but even in the case of the same observer after he has had more practice in making the measurements. Thus it would seem that VOLKMANN himself gained greater facility in recognizing double images after he had experimented with them for a

[1] *Archiv f. Ophthalmologie*, II, 2, pp. 32-59.

Observer	Interval ab	Interval cd	Difference $ab-cd$	Position of lines
Volkmann	5.3	3.46 7.57	1.84 −2.27	vertical
Volkmann	5.3	4.52 6.62	0.78 −1.32	vertical (2 months later)
Volkmann	1.5	0.91 3.25	0.59 −1.75	vertical
Volkmann	8.0	5.91 10.99	2.09 −2.99	vertical
Volkmann	5.3	4.88 6.05	0.42 −0.75	horizontal
Volkmann	1.5	1.15 1.97	0.45 −0.47	horizontal
Volkmann	8.3	7.26 9.01	1.04 −0.71	horizontal
Solger	5.3	2.13 10.00	3.17 −4.70	vertical
Solger	5.3	4.66 5.91	0.64 −0.61	horizontal
Krause	5.3	3.21 8.48	2.09 −3.18	vertical
Krause	5.3	4.92 5.86	0.38 −0.56	horizontal

couple of months. No doubt, also, the reason why he could detect the double images for smaller intervals between the lines than the other two observers was because from the start he had had much more experience in making experiments in physiological optics. Still it is probably true also that proficiency in making measurements by the

eye will vary considerably according to the particular nature of the estimates that have to be made. Moreover, the results go to show that, as has already been stated, it is much easier to perceive vertical differences between horizontal lines in the two fields than it is to perceive horizontal differences. The personal equation does not seem to be as big a factor in regard to the latter differences. When we consider that perhaps only one-half of the recorded deviation ought to be taken, and that from this the width of the lines themselves, amounting to about a tenth of a millimetre, has to be subtracted, and, lastly, that at a distance of 150 mm the smallest visible interval is only about a twentieth of a millimetre, in some of the experiments on horizontal lines there really is not very much room left for fusion to operate. Some of VOLKMANN's other experiments show that generally as the angle between the pairs of lines and the vertical gets to be greater and greater, the fusion difference between their distances becomes less and less, and is a minimum when the lines are horizontal.

VOLKMANN also undertook to determine the greatest differences that could exist between the directions of the lines when they could still be fused. The two lines were drawn through the centres of two circular discs which could be rotated with reference to one another. The two diameters at first were parallel to each other and both inclined to the vertical at a certain angle, as recorded in the first column of the subjoined table. Then the disc opposite the right eye was turned one way or the other until stereoscopic fusion ceased, the angles obtained in this way being given in the three last columns of the table. Each angle as found by VOLKMANN was the average of twenty determinations of its value. SOLGER's results are the averages of thirty determinations. The length of the lines is denoted by D.

Inclination to the vertical	Angular interval between lines		
	VOLKMANN $D=60$ mm	VOLKMANN $D=20$ mm	SOLGER $D=60$ mm
0°	5.5°	7.4°	17.5°
10	5.1	6.9	15.5
20	4.4	6.1	14.0
30	3.8	5.8	11.5
40	3.7	5.3	10.2
50	3.4	4.4	8.9
60	2.7	4.1	6.2
70	2.4	3.3	4.5
80	1.9	2.8	3.9
90	1.5	2.1	2.9

Evidently, when the lines are nearly vertical they can be fused with a much greater angle between them than is the case when the lines are nearly horizontal. But here likewise there is considerable difference between individual observers and also as to details in the observations. Shorter lines can be fused more readily than longer ones.

WHEATSTONE, who invented the stereoscope, argued from his experiments that, just as disparate images could be fused into a single image by stereoscopic projection, so likewise corresponding points of two retinal images might be shifted to two different places in space, and thus be seen double. This conclusion has been the subject of much controversy. However, when it is properly understood, with the necessary limitations, it cannot well be contradicted. For the moment we admit that under some circumstances and in a certain sense, disparate images may be seen single, it necessarily follows that under the same circumstances and in the same sense even corresponding images are bound to be seen double. In Fig. 72 suppose that the areas A and B are both green, and C and D both red. They may be considered as being portions of any stereogram, which on being fused will represent a single tilted surface. Then the line ab will be fused

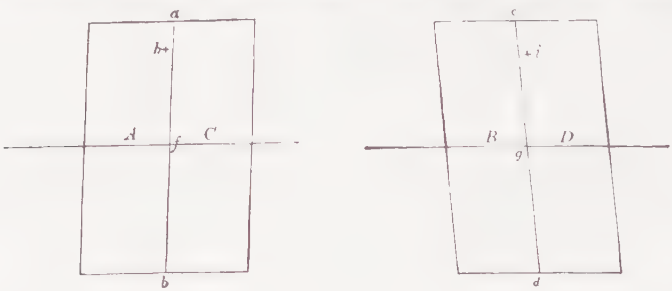

Fig. 72.

with the line cd, although these two lines do not correspond in direction exactly. Suppose the points of fixation are at f and g; then h and i will be a pair of corresponding points vertically above f and g, respectively, which may be on opposite sides of ab and cd, because by hypothesis the latter are not a pair of corresponding lines. In the diagram these points h and i are marked with little crosses simply to show their positions; but on the stereogram itself they are not supposed to be distinguished in any way from the background of the pictures. In the combined image the apparently tilted area will look green all over on the left of the line of separation of the two colours, and red all over on the other side; and hence the point h in the green portion will

necessarily be also on the left of this line, while the corresponding point i will be on the right of it. Obviously, the common act of vision cannot alter the arrangement of the points on the visual globes of the two eyes. And thus the two points h and i will be localized as being at *two different spots on the tilted area that seems to be present*, and yet not at two different points on the visual globes, because here we are not supposed to be thinking of the visual globe anyhow. However, of course, this lasts only as long as we are still under the impression of the solid perceptual image and therefore are prevented from making an accurate comparison of the positions of ab and cd relative to the retinal horizons. As soon as our attention is diverted from the appearance of a material object and from the form of the images on the visual globe, after a little practice, we may be able to see that the lines ab and cd are divided by a red and green band, in which the green of the point h coincides with the red of the point i.

I might pause here to say that, according to the theory of those who believe that the correspondence between the two retinal images is something intuitive, in a case of this kind the parts of the other image on the border of the coloured areas would be obliterated by the so-called rivalry between the two visual globes. Immediately adjacent to any such boundary the green and red that come together there would suppress the corresponding red and green ground. But even if this were so, it would still be possible to choose the positions of the points h and i so that the conflict at these places would be evenly balanced, and then all our objections would be valid once more.

Incidentally, the points h and i in the two drawings must not be indicated in the same way; otherwise, they might make the impression of an object lying beyond the fused picture of the lines ab and cd; in which case, therefore, the *contiguity* of the points and lines would not be one of the factors in the space-apperception.

If it is desired to indicate the corresponding points which are intended to be seen as separate points, they must be denoted in some different way from each other. In one of WHEATSTONE's experiments, which has been the subject of much discussion, there was a heavy black line in one picture and a very fine line corresponding to it in the other picture. But this fine line was intersected by another heavy line. The result was that when the two pictures were combined stereoscopically, apparently the two thick lines were fused in a single line inclined to the plane of the paper, while the narrow line appeared to be adjacent to it in the plane of the paper. Now in this one of WHEATSTONE's stereograms there is so much difference in the directions of the two lines that are to be fused that undoubtedly most people will see them as separate lines without any difficulty, although they may

do so for different reasons. Wheatstone himself was evidently one of those observers who can contrive to ignore double images when they are very far apart. Every observer has to adjust the angle between the lines to be fused to suit himself. In my own case I found I could be more certain about obtaining the result by drawing a pair of lines in each figure intersecting each other at a small angle, one of the lines being heavy and the other light, and the heavy line in one figure corresponding to the light line in the other. Stereogram *M*, Plate III, is drawn in this fashion to suit my own eyes. In the case of an observer whose apparently vertical meridians diverged differently from mine, the relations of the lines in the stereogram would have to be altered somewhat, of course. Now when I examine this stereogram, the heavy line on one side fuses with the heavy line on the other side, and the light line on one side with the light line on the other side; nor am I able to make the heavy line on the left coincide with the light line on the right. The only way I am able to see that these two lines are absolutely parallel to each other is by changing the divergence of my eyes and shifting the images apart. It is wrong to imagine that one of the images vanishes entirely as we are gazing at the stereogram, and hence is left out of account; for then we ought not to get any stereoscopic effect there. But, as a matter of fact, the pair of intersecting lines stands out clearly, apparently with the upper end towards the spectator, as may be seen by comparing it with the fine vertical lines close by. This would not be the case if the light line on the right were entirely invisible.

A similar effect is obtained with the stereogram *N*, Plate III, where the two outer edges of the upper parts of the triangular black strips are a pair of corresponding lines, and, similarly, the inner edges of the lower portions, which are the prolongations of the upper outer edges. In the fused image we see simply a single black area, but the corresponding edges appear on opposite sides of it. In this stereogram also the mutual inclination of the two black triangles to each other must be altered to suit the divergence of the vertical meridians of the observer.

In the stereograms *M* and *N*, most observers will find it impossible to detect the fact that the lines which are apparently fused in the common field of view do not really coincide, but that the light line in the right-hand picture in *M* coincides with the heavy line in the left-hand picture; and that in *N* it is the opposite edges of the black area that coincide. But I do not mean to say that anybody who has had some fairly good practice in observing double images might not succeed in seeing these stereograms correctly. When I gaze very intently at the centre, I notice myself that the lines in question do not appear exactly

single, and yet I am not able to separate the double images distinctly. By drawing the figures with ink on glass, as was done by W. v. Bezold, it will be easier to separate them; because then by suddenly changing the illumination, one figure can be made to appear bright on a dark background, and the other dark on a bright background. Then there is no tendency to fuse the two pictures, and their disparate positions will be readily perceived. All I will say here is this—which appears to me to be the real significance of Wheatstone's experiment—namely, that, as long as we are absorbed in the apperception of the material form, although we hold the point of fixation perfectly steady, the impressions we get of corresponding points simply help to fill in various portions of the total image. When the conditions are most conducive to our making faulty comparisons between the different images in the two eyes, the images of disparate points will be fused, and those of corresponding points will be separated. It is true, as we have said, one of these things cannot happen without the other, and the second is a logical consequence of the first. But that is no reason why, when the mode of observation has been modified for the very purpose of enabling us to compare the images on the two visual globes with as little disturbance as possible, and with the deliberate intention, therefore, of keeping disparate points separate, the images of corresponding points should not then be fused again.

It may be added that when the stereograms M and N are illuminated by electric sparks the stereoscopic fusion is absolutely perfect. There will not then be any trace of the double images which ought to be seen in the common field of view when only the images of corresponding points were made to coincide. Consequently, this effect has nothing to do with the movements of the eyes.

Some other conditions are involved in the fusion of two different retinal images, which have yet to be mentioned.

In the first place, as long as stereoscopic perception of depth is present, it is not true (as some have supposed who believe there is an identity-relation between the two retinas from birth) that one of the pair of double images disappears by being completely suppressed, so that there is no sensation of this image at all. If such were the case, no binocular perception of depth would be possible, because that depends entirely on the difference between the two images and on the perception thereof. Indeed, it is just this extraordinary precision of the depth-sense that is the proof of our ability to discern so accurately the difference between the two images, not as a difference in the way the visual globes are filled out, but simply as being the visual expression of the fact that the various points of the object are at different

distances. It is true that individual parts of the images, where there is no perception of depth, do disappear either temporarily or altogether; and these cases will have to be considered more fully in the following chapter.

In the second place, the effect of movements of the eyes on the fusion of the double images is another question that has to be considered. In this connection, E. Brücke has suggested the theory that the only way we get a perception of depth is by continually letting our eyes traverse the various contours of the observed object, and focusing each point of these outlines simultaneously in the corresponding foveas of the two eyes one after the other. Now as our attention is usually concentrated on those images which are formed at the places on the retinas where vision is most distinct, it might be a reasonable question whether the double images of the other parts of the object were not disregarded, simply because those parts of the in age that are seen most accurately and that rivet the attention most are ordinarily corresponding parts. The argument against Brücke's theory grants that the factors which he stresses are indeed of much importance in enabling us to obtain perfect apperceptions of depth, and that the description he gives of the way these apperceptions originate is in accordance with the conditions of ordinary natural vision. The truth is that it is only by moving our eyes that we can succeed in fusing images that are very different. Thus we endeavour to see singly all the various parts of the scene one after the other, letting our attention wander naturally, as it will always do, to those places on which our eyes happen to be focused at the time. When the object is surveyed in this fashion, the apperception of depth is decidedly more exact and more vivid than when the eyes are fastened on one spot. Perhaps the reason for it is that we do not perceive differences of depth accurately except at those places which happen to be very near the horopter at the moment. Thus, by changing the convergence of the eyes, and bringing each point of the object on the horopter one after the other, or at least very nearly on it, we do gradually get an exact apperception of all the differences of depth. On the contrary, when the gaze is riveted for a long time on one point, double images will be more apt to show up, and, especially in regard to those points whose double images are very disparate, differences of depth will be vague. The truth is that double images, which cannot be separated from each other by gazing long and intently at one place, must be so nearly at the threshold of the resolving power of the eyes that it is safe to assume that the reason why they cannot be disunited is on account of little unavoidable movements of the eyes. But while all this may be granted, Brücke's theory is a little too rigid if he

intended to imply that *all* perceptions of depth were derived simply by movements of the eyes, or that it was possible to abolish *all* double images merely by seeing each separate point singly one after the other. For Dove showed that even by the instantaneous illumination of an electric spark it was possible both to obtain stereoscopic effects and to fuse double images. The apparatus described on page 197 (Fig. 32) may be employed for this purpose. Only, one must be careful to see that the two lines of fixation are directed to corresponding parts of the stereogram at the moment the flash occurs. My method of making this experiment is to mount the stereogram on the inside wall of a dark box, and make two pinholes in it at corresponding points. The wall of the box is perforated also where these pinholes are. The room must not be so perfectly dark that the observer cannot see these holes by the dim light shining through them. Then he focuses his eyes on them until they coincide with each other in the binocular field, and when everything is ready, the spark is discharged. Stereograms, such as E, M and N, Plates II and III, in which the differences between the two patterns are not too large, will be found to give clear vivid stereoscopic relief, with an absence of perceptible double images. But a stereogram like H, in which the differences are more considerable, will show up in single lines, without producing any apperception of depth. Pairs of horizontal lines, one above the other (as shown in F), separate with surprising ease. In case the drawings are simple and consist only of a few lines, they will be seen in their entirety at a single flash. On the other hand, in the case of complicated stereoscopic photographs with numerous details, the spectator does not get a clear impression of the whole scene at once, and it may take several sparks to reveal it all. It is a curious fact, by the way, that the observer may be gazing steadily at the two pinholes and holding them in exact coincidence, and yet at the same time he can concentrate his attention on any part of the dark field he likes, so that when the spark comes, he will get an impression about objects in that particular region only. In this experiment the attention is entirely independent of the position and accommodation of the eyes or, indeed, of any known variations in or on the organ of vision. Thus it is possible, simply by a conscious and voluntary effort, to focus the attention on some definite spot in an absolutely dark and featureless field. In the development of a theory of the attention, this is one of the most striking experiments that can be made.

These experiments with instantaneous illumination not only enable us to realize what an important factor the attention is in the case of double images, but they are also instructive in other ways. For example, consider a stereogram such as J, Plate II; it can be fused

into a single stereoscopic image without much effort, or it can be seen double almost just as easily. Stereograms of this kind may easily be seen either way under the illumination of the electric spark. We are apt to have first the impression of single stereoscopic vision. But when the flashes are made to succeed each other at intervals of about ten seconds (which gives time for the after-images to die out completely), presently the observer will begin to see the double images, although he may continue to gaze steadily at one spot so that each succeeding luminous action is precisely equal to the first one. Although it is comparatively hard to see the double images with such stereograms as *M*, Plate III, still I am able to do so finally when the illumination is instantaneous, provided I try very hard beforehand to imagine how they ought to look. In a case of this kind the influence of the attention can be studied purely by itself, because any effect due to movements of the eyes is absolutely prevented. These same experiments can be performed with VOLKMANN's tachistoscope mentioned on page 197.

Another matter that should be noticed here is that various reliable observers, such as WHEATSTONE,[1] ROGERS,[2] and WUNDT,[3] have succeeded also in fusing after-images, which were not altogether exactly at corresponding places on the two retinas, and have obtained a stereoscopic impression of depth in this way. ROGERS, in fact, found that he could develop the after-image first in one eye, then in the other, and then finally combine them stereoscopically. This process enables us to avoid the effect which any previous apperception of the real images would be very apt to have on the interpretation of the after-images. I may add that I myself have obtained distinct apperception of depth from positive after-images which I had developed by gazing for a moment at some brilliantly lighted object.

Both these experiments and those with electric sparks show that ocular movements are not necessary for perception of depth; because the after-images move with every movement of the eye, and it is simply impossible to make disparate images correspond to each other by any such movement. These experiments with after-images are by no means easy to perform. The after-images must be developed very distinctly, and even then there is a constant tendency to project them on the real background of the field and to consider them as being merely spots on that surface.

PANUM's rule for the fusion of double images is that contours resembling each other, which are depicted on approximately corresponding retinal points, must be fused together. Here the circum-

[1] *Phil. Transact.* 1838. T. II, pp. 392–393.

[2] SILLIMAN's *Journal,* (2) XXX, November 1860.

[3] *Beiträge zur Theorie der Sinneswahrnehmung.* pp. 286–287.

ference which contains those points on the other retina which may be fused with a given point on the first retina is called the *corresponding circle of sensation* of that point. In accordance with the previous results, PANUM makes the horizontal diameters of these "circles" longer than their vertical diameters. On the other hand, in my treatment of the subject, I have maintained that the reason why it was possible to fuse the double images was because the eye was not reliable enough or accurate enough to estimate the corresponding dimensions of the two images without possibility of errors, and that any error of this sort is fostered by the apperception of the material object which is present before us or which we fancy is present before us. In opposition to PANUM's way of stating the law, VOLKMANN has adduced such instances as that of stereogram *G*, Plate II, in which some little incongruities between the two pictures, for example, the mere insertion of a dot on one of them, may be sufficient to disturb fusion. In rebuttal PANUM argues that such cases invariably involve some dissimilarity in the contours, and that, according to the way the law is stated, this dissimilarity would necessarily prevent fusion. Some other experiments of VOLKMANN's indicate that when the differences in the intervals between pairs of lines are the same, the lines with the smaller intervals cannot be fused as readily as those with the larger ones. In reply to this, PANUM alleges that, when the eyes are focused on lines which are close to each other, their images are very near the centres of the two retinas, and that there the corresponding "circles" of sensation are smaller, and consequently the double images cannot be fused. But the experiment of VOLKMANN's referred to above may be performed in a different way as follows. In the stereogram *U*, Plate V, five lines are drawn on each side. On the left-hand side the distance between lines 1 and 3 and lines 4 and 5 is 4 mm in each case; whereas on the right-hand side the interval between the lines in each of these same pairs is 5 mm. On each side of the stereogram the line 2 is inserted in between lines 1 and 3, and in both cases line 2 is 3 mm from line 1. Thus it is only 1 mm from line 3 on the left-hand side, but 2 mm from line 3 on the other side. Now if we gaze steadily at line 4 in the total image, we shall see line 5 single and lying a little behind line 4. If, however, the eyes are fastened on line 1, the two lines 3 will appear to be separated, but, of course, the line 2 will be seen single and at the same distance away as the line 1. The only way we can see the line 3 single is by moving the eyes, and then the entire group of lines will look like a vertical prism with four dihedral edges, the line 2 apparently being drawn on the front face of the prism parallel to the edges. But when the eyes are gazing steadily at line 1 in the total image, the pair of lines 3 will be depicted on the retinas in exactly the same places as

the pair of lines 5 were when the eyes were focused on lines 4. Evidently the obstacle to fusion here is line 2; and yet this line does not lie in between the double images, but on the left of both of them, and, according to the way the law is given by PANUM, it ought not to hinder fusion. However, if the union of the double images is regarded as an illusion of the eyesight, it is obvious from FECHNER's law that the discrimination of distance between 1 and 2 mm, which are the distances involved in the case of lines 2 and 3, is more reliable than that between 4 and 5 mm, as involved in the case of lines 4 and 5.

Experiments with figures of circles give similar results. Suppose we have a stereogram with two circles on it a little different in size, so that they can be fused binocularly. Draw a concentric circle around each of them with the same radius in both cases, that is, with a radius not much bigger than that of the larger of the two inner circles. Then it will be comparatively easy to separate the images of the latter.

Lastly, there is one other question which comes up here, and which likewise is of some theoretical importance; that is, whether we can distinguish the impressions of one eye from those of the other. In this connection, it is well to remember that when groups of lines are seen stereoscopically the intervals of depth are always seen correctly, even by instantaneous electric illumination, but the relief is never reversed. Even when I tried to imagine as well as I could the reversal of the relief, in order to produce an intentional illusion (as I could do very quickly in reversing the relief of medals in the case of monocular vision), I found it simply impossible to alter the stereoscopic relief.[1] And yet such reversal of the relief would be bound to take place if the impression of the two retinal images could be confused with that impression which would be obtained by interchanging the retinal images with each other. Hence, it follows, in the first place, that the instantaneous impression made by two retinal images must be distinctly and definitely different from that which the same retinal images would make if each were transferred to the corresponding points of the other eye.

The fact that ordinarily one is not clearly aware which eye it is with which he sees this image or the other, is a somewhat different thing. We cannot be certain about it, or at least not perfectly certain, and our judgment will depend on secondary considerations; for we cannot decipher anything from our sensations except those interpretations we have learned to make by oft-repeated observations. Thus we may have learned to tell perfectly that two double images of a certain sort,

[1] See the same observations as made by AUBERT and MARBACH (page 315 of AUBERT's *Physiologie der Netzhaut.* Breslau, 1865), where a great many different figures are given. Practically the same results have also been obtained by DONDERS recently.

which are closely adjacent, and which have certain local signs, signify an object that is farther from us than the point of fixation, and not one that is nearer to us; and still we may not be sufficiently well trained in interpreting the local signs of the images to tell which of the two half-images belongs to one eye, and which to the other eye. To be sure about this, one must close one eye or cover it; although this is not what one does in ordinary vision, where, as has been stated, the double images are usually not heeded at all. Therefore, as a rule, without making a special experiment for that very purpose, we are ignorant as to which image belongs to one eye; and which to the other eye. Nor do the movements of the eye aid us much here, because when the eyes are converged (as they will be in this case) we do not have any clear idea of the direction in which each eye by itself is shifted.

On the other hand, the extreme portions of the common field of view over on the right will be seen constantly by the right eye only, being concealed from the other eye by the nose. And, similarly, objects far over to the left will be visible to the left eye only. Consequently, when a region of the field is completely hidden from one eye, we naturally infer that the objects perceived there must be seen with the other eye. A striking experiment described by ROGERS should be mentioned here. Make a tube about two inches in diameter with a piece of black paper, and, holding it up to the right eye, point it toward the far corner of the room over on the left. At the same time hold a sheet of paper several inches from the other eye, so as to screen this eye from the part of the room seen through the tube. Then you will have a very decided illusion as though you were looking at the corner of the room with your left eye through a hole in the paper; whereas there is no hole in it, and it is the other eye, and not the left eye, that is looking through the tube.[1]

I have already stated that in looking at a photographic stereogram where there is a little dark spot or imperfection on one of the pictures, the impression I generally get is that there is a haziness in the eye with which I see this spot, which I try instinctively to brush away by moving the lids of that eye. Perhaps, this is an indication that in a case of this kind I have an inkling that the trouble is due to some vagueness about the image in that particular eye.[2]

The question as to the directions in which the double images appear to be, may be answered by what has been previously stated in regard

[1] ¶This beautiful experiment may be made by holding the open hand in front of the left eye a little distance away; and then the screened eye will seem to be gazing through a hole in the hand of the same size and form as the opening in the tube, through which the other eye is really looking. (L.D.W.)

[2] Concerning the discrimination of visual impressions in the two eyes, see Note 4 at the end of this chapter.—K.

to the direction of an image in monocular vision. The image is seen by each eye just as it would be formed on the retina of an imaginary cyclopean eye, as conceived by E. HERING, supposing that this eye were directed toward the point of fixation. Thus, in binocular vision the two retinal images may be supposed to be transferred to the retina of this imaginary eye, where they mutually overlap each other and are then projected correspondingly in space. As far as our imperfect stereoscopic perception of depth will admit, together with such aid as can be obtained by monocular judgment of distance, the distances of the images from the observer will be correctly estimated. The reason too why the double images are always separated when they are projected in space is apparent from the experiment which was proposed by E. HERING and performed by J. TOWNE.[1] If the images were projected along the right directions of the lines of sight, they might possibly be shifted to the place where those lines intersect, and thus would appear single. But inasmuch as the directions of vision are incorrectly referred to a centre lying in the median plane of the face, what really happens is that two different directions of vision never can meet again in the space in front of the observer, and, therefore, image-points projected along these directions must remain separate always. The presumable explanation of this faulty projection has been given in a previous chapter.

Laws of Corresponding Points and Lines.—Consider a pair of planes, one perpendicular to one line of fixation, and the other perpendicular to the other line of fixation; these planes being equidistant from the point of fixation. The coördinates of any point in one plane being denoted by x, y, those of any point in the other plane may be denoted by ξ, v. And suppose that the coördinates of the points where the lines of fixation cross these planes are $x=y=0$ and $\xi=v=0$, respectively. The equations of the lines in which the retinal horizons of the two eyes intersect these planes may be written as follows:

$$ax+by=0 \quad \text{and} \quad \alpha\xi+\beta v=0 \qquad (1)$$

and, similarly, the equations of the lines in which the apparently vertical meridians of the two eyes intersect this same pair of planes may be written as follows:

$$cx+dy=0 \quad \text{and} \quad \gamma\xi+\delta v=0 \qquad (1a)$$

[1] These important observations on the apparent directions of vision were made by Mr. J. TOWNE independently of Mr. E. HERING. In a letter to me he states that he had shown the experiments as early as 1859; although his first publications, as far as I have been able to ascertain, were made in 1862.

If the coefficients in these two pairs of equations are so chosen that

$$\left.\begin{aligned} a^2+b^2=\alpha^2+\beta^2=1 \\ c^2+d^2=\gamma^2+\delta^2=1 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1\text{b})$$

(as can always be done without altering the equations by multiplying one equation of each pair by a certain factor), then, according to a familiar principle of analytic geometry, the expression

$$ax+by$$

will represent the distance of the point (x,y) from the line $ax+by=0$. The corresponding expressions in the other equations have similar meanings. Moreover, by giving proper signs to the factors by which the equations have to be multiplied, the expressions

$$ax+by \quad \text{and} \quad \alpha\xi+\beta v$$

can be made to be positive on corresponding sides of the two retinal horizons; and, similarly, the expressions

$$cx+dy \quad \text{and} \quad \gamma\xi+\delta v$$

can be made to be positive on corresponding sides of the two apparently vertical meridians.

It is an experimental fact that corresponding points in a pair of planes are at equal distances from the retinal horizons and likewise from the apparently vertical meridians. Assuming that the conditions above specified are true with respect to the coefficients of equations (1) and (1a), we may write, therefore, the conditions of correspondence as follows:

$$\left.\begin{aligned} ax+dy=\alpha\xi+\beta v \\ cx+dy=\gamma\xi+\delta v \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \qquad (1\text{c})$$

A straight line in one field is said to be in correspondence with a straight line in the other field, when there is a point-to-point correspondence between them.

Thus the straight line

$$l(ax+by)+m(cx+dy)+n=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1\text{d})$$

will be in correspondence with the straight line

$$l(\alpha\xi+\beta v)+m(\gamma\xi+\delta v)+n=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1\text{e})$$

in the other field, where l, m, n denote here any three arbitrary factors. For giving (x,y) any constant values, suppose the line is drawn in the other field whose equation is:

$$\alpha\xi+\beta v=ax+by \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1\text{f})$$

Then at the place where this line meets the line whose equation is (1e) we must have also:

$$\gamma\xi+\delta v=cx+dy\ ,$$

as is obtained in this case by subtracting equation (1e) from equation (1d). Hence, the point where the lines (1e) and (1f) intersect will be a point corresponding to the point (x, y).

The equation of any straight line

$$fx+gy+h=0\ . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1g)$$

can readily be put in the form of equation (1d) by putting

$$f=la+mc\ , \qquad g=lb+md\ , \qquad h=n\ ;$$

or

$$l=\frac{df-gc}{ad-bc}\ , \qquad m=\frac{bf-ag}{bc-ad}\ , \qquad n=h\ ;$$

and hence the three coefficients l, m, n in equation (1d) will be determined. Thus, by forming equation (1e) from equation (1d), we shall obtain the equation corresponding to the line given by equation (1g).

Equation (1d) may be put in the so-called normal form by dividing it by

$$k=\sqrt{(la+mc)^2+(lb+md)^2}\ ,$$

and then the magnitude n/k will denote the distance of the surface represented by equation (1d) from the origin of the system of co-ordinates. Similarly, n/κ where

$$\kappa=\sqrt{(l\alpha+m\gamma)^2+(l\beta+m\delta)^2}$$

will denote the distance from the origin of the plane represented by equation (1e). Accordingly, the two distances will not be equal unless

$$k^2=\kappa^2\ .$$

With reference to equations (1b), this condition is equivalent to

$$ac+bd=\alpha\gamma+\beta\delta\ ;$$

which implies that the two pairs of planes whose equations are given by (1) and (1a) make equal angles with each other in each eye. And if this is not the case, then the condition $k^2=\kappa^2$ cannot be satisfied unless

either $m=0$ or $l=0$; that is, unless the planes (1d) and (1e) coincide either with the pair of planes given by equations (1) or with the pair of planes given by equations (1a). The above property, therefore, distinguishes these two pairs of corresponding planes from all other such pairs of planes containing the lines of fixation of the two eyes, and so they may be called the *planes of the principal meridians* of the two eyes.

Calculation of Corresponding Linear and Angular Dimensions in the Two Eyes.

If for convenience the axes of x and ξ are taken in the retinal horizons, then in equation (1):

$$a=\alpha=0\ , \quad b=\beta=1\ .$$

And if we suppose that the apparently vertical meridians are symmetrical with respect to each other (as is practically the case anyhow as a rule), then

$$\frac{d}{c}=-\frac{\delta}{\gamma}=-\tan\epsilon\ ,$$

where ϵ denotes the angle between the apparently and really vertical meridians of each eye. Then

$$c=\cos\epsilon\ , \qquad \gamma=\cos\epsilon\ ,$$

$$d=-\sin\epsilon\ , \quad \delta=\sin\epsilon\ ;$$

and the equations of the retinal horizon will be:

$$y=0 \quad \text{and} \quad v=0\ ; \quad . \ . \ . \ . \ . \ . \ . \ . \ . \quad (1h)$$

and the equations of the lines that are apparently vertical will be:

$$x\cos\epsilon-y\sin\epsilon=0 \quad \text{and} \quad \xi\cos\epsilon+v\sin\epsilon=0 \quad . \ . \ . \ . \quad (1i)$$

Moreover, according to equations (1d) and (1e), the equations of pairs of corresponding lines passing through the point of fixation will be:

$$xm\cos\epsilon+y(l-m\sin\epsilon)=0\ ,$$

$$\xi m\cos\epsilon+v(l+m\sin\epsilon)=0\ .$$

If s and σ denote the angles which these lines make with the axes of x and ξ, then

$$\tan s = \frac{y}{x} = -\frac{m \cos \epsilon}{l - m \sin \epsilon},$$

$$\tan \sigma = \frac{v}{\xi} = -\frac{m \cos \epsilon}{l + m \sin \epsilon};$$

and hence:

$$\tan (\sigma - s) = \frac{2m^2 \cos \epsilon \sin \epsilon}{l^2 + m^2 \cos 2\epsilon},$$

$$\tan (\sigma + s) = -\frac{2ml \cos \epsilon}{l^2 - m^2}.$$

Putting

$$\frac{m}{l} = \tan \beta,$$

we can write:

$$\tan (\sigma - s) = \frac{\tan^2 \beta \cdot \sin 2\epsilon}{1 + \tan^2 \beta \cos 2\epsilon},$$

$$\tan (\sigma + s) = -\tan 2\beta \cos \epsilon.$$

Or, since ϵ is a comparatively small angle, and consequently $\cos \epsilon = \cos 2\epsilon = 1$ and $\sin 2\epsilon = 2\epsilon$, approximately, we may write:

$$\beta = -\frac{s + \sigma}{2},$$

$$\sigma - s = 2\, \epsilon \sin^2 \beta.$$

The angles s and σ are measured from the retinal horizons. In case they are measured from the visual planes, the angle γ between the retinal horizons must be added to the difference between these angles; and then we obtain for their difference the formula used on page 420, viz.:

$$\Delta = \gamma + 2\, \epsilon \sin^2 \beta \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \qquad (2)$$

Corresponding Lines of Sight and Corresponding Visual Planes. If the point of intersection of the lines of sight in each eye[1] is connected by a straight line with that one of a pair of corresponding points that belongs to that eye, the two lines thus obtained will be a pair of *corresponding lines of sight.* The images of points that happen to lie on a pair of corresponding lines of sight will be a pair of corresponding points on the two retinas.

[1] ¶That is, the centre of the so-called entrance-pupil of each eye. (J. P. C. S.)

Suppose two corresponding straight lines are drawn, one in the xy-plane and the other in the ξv-plane; then all the lines of sight for points in these lines will lie in a pair of planes passing through the point of intersection of the lines of sight—which may be called *corresponding planes.*

Any pair of straight lines drawn in a pair of corresponding planes will be imaged by corresponding lines on the two retinas. The images of the line of intersection of a pair of corresponding planes will be corresponding lines on the two retinas.

Let the coördinates of the points of intersection of the lines of sight in the two eyes be taken as follows:

$$x=0\,,\quad y=0\,,\quad z=e$$
$$\xi=0\,,\quad v=0\,,\quad \zeta=e\,.$$

The equation of a plane passing through the point (x, y, z) may be written in the following form:

$$fx+gy+\frac{h}{e}(e-z)=0\,.$$

If we put $z=0$ in this equation, evidently, it reduces to an equation of the form (1g), and the corresponding line in the ξv-plane may be found by the method given there, and hence the corresponding plane can be obtained also.

If we write

$$\left.\begin{array}{ll} A=ax+by & \mathfrak{A}=\alpha\xi+\beta v \\ B=cx+dy & \mathfrak{B}=\gamma\xi+\delta v \\ C=z-e & \mathfrak{C}=\zeta-e \end{array}\right\}, \quad \ldots\ldots\ldots \tag{3}$$

then all planes whose equations are of the form

$$\left.\begin{array}{l} lA+mB+nC=0 \\ l\mathfrak{A}+m\mathfrak{B}+n\mathfrak{C}=0 \end{array}\right\} \quad \ldots\ldots\ldots\ldots \tag{3a}$$

will be corresponding planes. For these equations have the same form as the equations of those planes that pass through the point of intersection of the lines of sight; and if we put $z=0$ and $\zeta=0$, then, according to the rules formulated in equations (1d) and (1e), we shall have left the equations of corresponding lines lying in the planes of xy and ξv. Consequently the planes are corresponding.

Corresponding lines of sight may be regarded as lines of intersection of two pairs of corresponding planes.

Equations of Straight Lines which are Seen Single. So far we have considered the positions of corresponding lines and planes simply with respect to the eye to which they referred, without taking into account at all the positions of the eyes with respect to each other and with respect to the objects in space. Now in order to do this, let us suppose that the positions of all points, including the eyes themselves, are referred to a common system of rectangular coördinates $\mathfrak{x}$, $\mathfrak{y}$, $\mathfrak{z}$. The expressions for x, y, z and ξ, v, ζ in terms of these new coördinates will be linear functions of these coördinates; and the same is true with respect to the magnitudes A, B, C and $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$ which are themselves linear functions of x, y, z and ξ, v, ζ.

In general, one straight line which will appear single will pass through any point in space. The proof of this statement will now be given. The equations of corresponding planes, as given by equations (3a), are:

$$\left.\begin{aligned} lA+mB+nC&=0\\ l\mathfrak{A}+m\mathfrak{B}+n\mathfrak{C}&=0\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3b)$$

These two equations together enable us to determine the position of the line of intersection of these planes; and, as above stated, this line will be seen single, and will therefore be a rectilinear horopter line.

Now if the coördinates $\mathfrak{x}_0$, $\mathfrak{y}_0$, $\mathfrak{z}_0$ of any point are substituted in equations (3a) for $\mathfrak{x}$, $\mathfrak{y}$, $\mathfrak{z}$, the coefficients l, m, n can always be determined so as to satisfy the pair of equations (3b). Any value whatever can be given to one of the coefficients by multiplying through by a common factor; and so all we really have to do is to determine two coefficients for which the pair of equations will generally be satisfied. We obtain:

$$\frac{l}{n}=\frac{B_0\mathfrak{C}_0-\mathfrak{B}_0C_0}{A_0\mathfrak{B}_0-\mathfrak{A}_0B_0},$$

$$\frac{m}{n}=\frac{A_0\mathfrak{C}_0-\mathfrak{A}_0C_0}{\mathfrak{A}_0B_0-A_0\mathfrak{B}_0}.$$

Thus we find the ratios of l, m, n to each other which will satisfy equations (3a); and these values will be uniquely determined unless the above fractions happen to be of the form $0/0$; which will be the case, provided

$$\left.\begin{aligned} A_0\mathfrak{C}_0&=\mathfrak{A}_0C_0\\ B_0\mathfrak{C}_0&=\mathfrak{B}_0C_0\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3c)$$

which generally implies also that:

$$A_0\mathfrak{B}_0=\mathfrak{A}_0B_0 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3c')$$

We shall see presently that these last three equations apply to points which are on the horopter-curve. Accordingly, leaving out these points, we see that only one straight line can be drawn through a given point in space that will be seen single; whereas through the points given by equations (3c) any number of such lines may be drawn.

Surfaces of the Second Degree on which the Lines that are Seen Single must lie. Let

$$\left.\begin{aligned} l_0A+m_0B+n_0C=0\,, \qquad & l_0\mathfrak{A}+m_0\mathfrak{B}+n_0\mathfrak{C}=0 \\ l_1A+m_1B+n_1C=0\,, \qquad & l_1\mathfrak{A}+m_1\mathfrak{B}+n_1\mathfrak{C}=0 \end{aligned}\right\} \quad . \quad . \quad . \qquad (4)$$

be the equations of two pairs of corresponding surfaces. Then the pair of surfaces on the right-hand side will intersect each other in one line of sight, and the pair on the left-hand side will intersect each other in the corresponding line of sight. Now multiply each of the two lower equations by a new factor k, and add them to the upper equations. Thus, we obtain:

$$\left.\begin{aligned} (l_0+kl_1)A+(m_0+km_1)B+(n_0+kn_1)C=0 \\ (l_0+kl_1)\mathfrak{A}+(m_0+km_1)\mathfrak{B}+(n_0+kn_1)\mathfrak{C}=0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \qquad (4a)$$

These are the equations of a third pair of corresponding surfaces which, however, also pass through the same pair of lines of sight as the surfaces represented by equations (4). Thus the two equations on the left-hand side of equations (4) must be satisfied by points on one of the lines of sight; and hence also the upper one of equations (4a) must necessarily be satisfied by the same points; which means that the points on that line of sight must likewise be on the surface represented by this equation. The same thing applies to the right-hand pair of equations (4) and the lower one of equations (4a).

Taken together, the two equations (4a) determine a straight line which is seen single, since each represents one of a pair of corresponding planes. Suppose now that the factor k is made to vary continuously; then the position of the straight line which is seen single will vary continuously also. All these straight lines thus obtained by varying k continuously will constitute a surface, whose equation will be found by eliminating the factor k from the pair of equations (4a). Accordingly the equation of the surface which contains this family of straight lines that are all seen single will be as follows:

$$\begin{gathered} (l_0\,A+m_0\,B+n_0\,C)\;(l_1\mathfrak{A}+m_1\,\mathfrak{B}+n_1\mathfrak{C}) \\ -(l_1\,A+m_1\,B+n_1\,C)\;(l_0\mathfrak{A}+m_0\,\mathfrak{B}+n_0\mathfrak{C})=0\;; \end{gathered}$$

which, after performing the indicated multiplication, may be written:

$$(l_0 m_1 - l_1 m_0)(A\mathfrak{B} - \mathfrak{A}B) + (l_1 n_0 - l_0 n_1)(\mathfrak{A}C + A\mathfrak{C})$$
$$+ (m_0 n_1 - m_1 n_0)(B\mathfrak{C} - \mathfrak{B}C) = 0 \quad \ldots\ldots\ldots\ldots \quad (4b)$$

Since the magnitudes denoted by A, B, C and $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$ are linear functions of $\mathfrak{x}$, $\mathfrak{y}$, $\mathfrak{z}$, this is the equation of a surface of the second degree, and, moreover, this surface is one on which straight lines of indefinite length can be ruled. Accordingly, it must be an *hyperboloid of one sheet*, which in the limiting case may collapse into a *cone* or a *cylinder* or a *pair of intersecting planes*.

Now equations (3c) and (3c′), that is, the equations

$$\left.\begin{aligned} A\mathfrak{C} - \mathfrak{A}C &= 0 \\ \mathfrak{B}C - B\mathfrak{C} &= 0 \\ \mathfrak{A}B - A\mathfrak{B} &= 0 \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (4c)$$

enable us to find those points through which any number of straight lines may be drawn which will be seen single; and if these equations are compared with equation (4b), it will be seen that they also represent hyperboloids of the same kind as given by that equation; and that, for certain values of the coefficients l, m, n, equation (4b) can be made to be the same as one of the equations (4c). For instance, take the following pair of equations:

$$\left.\begin{aligned} A\mathfrak{C} - \mathfrak{A}C &= 0 \\ B\mathfrak{C} - \mathfrak{B}C &= 0 \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (4d)$$

These surfaces have two points in common, namely, the two points where the lines of sight intersect; and hence they must intersect each other in a curve. For one of these points,

$$A = B = C = 0\ ;$$

and for the other,

$$\mathfrak{A} = \mathfrak{B} = \mathfrak{C} = 0\ .$$

Either set of conditions will satisfy equations (4d). It is obvious that the conditions

$$C = \mathfrak{C} = 0$$

will satisfy both equations also; and this means simply that the straight line in which the two surfaces $C = 0$ and $\mathfrak{C} = 0$ intersect must lie on both hyperboloids, and that therefore it must coincide with their line of intersection. Accordingly, this line of intersection consists of the straight line whose equations are given by $C = 0$, $\mathfrak{C} = 0$ and of another curved portion which will usually be a curve of double curvature.

By multiplying the upper one of equations (4d) by B and the lower one by A, and then adding them, we can eliminate $\mathfrak{C}$ and so obtain the following equation:

$$(A\mathfrak{B}-\mathfrak{A}B)\,C=0\ .$$

Therefore, unless C is equal to zero, the result of this elimination will be the third of equations (4c), that is:

$$\mathfrak{A}B-A\mathfrak{B}=0\ \ldots\ldots\ldots \qquad (4e)$$

But if C vanishes, then, according to equations (4d), either $\mathfrak{C}$ must vanish also, or we must have $A=B=0$ both at the same time. In the latter case equation (4e) would be valid also; and the conditions $A=B=0$ apply to the point in one of the eyes where the lines of sight all intersect.

Thus we see that equation (4e) will be satisfied also by points which are on the line of intersection of the surfaces represented by equations (4d), but not on the straight line $C=\mathfrak{C}=0$; and hence that the three surfaces given by equations (4c) must all intersect each other in a single curve of double curvature. Moreover, each pair of these surfaces will intersect also in a straight line, which however will generally not be on the third surface.

Now if

$$X=0\ ,\quad Y=0\ ,\quad Z=0$$

are the equations of three surfaces which all intersect each other in a common line, then this line will lie also on any surface whose equation is of the form

$$lX+mY+nZ=0\ ,$$

because, as the three original equations are all satisfied by points on this line, this last equation will necessarily be satisfied also by these same points. Now equation (4b) may be considered as having been formed in this way from the three equations (4c). And so the curved line in which the surfaces represented by equations (4c) all intersect each other will lie on the surfaces of the whole family of hyperboloids on which the lines are situated which are seen single.

This curve happens to be a so-called curve of the third degree, that is, it is a curve which may be cut by a given plane in three points. Now, as a rule, two surfaces of the second degree will intersect each other in a curve of the fourth degree; which is the case, for instance, with the two surfaces represented by equations (4d). And this curve of the fourth degree may be cut by a plane in four points or in two points; only, one of these points must be on the rectilinear branch of the

line. (The case of parallelism is considered as a case when the point of intersection is at infinity.) Consequently, there will be only three points of intersection left, or one point, which can lie on the curved portion. Thus, for example, the horopter-curve crosses the visual plane at the point of fixation and at the centres of the two eyes. So also the curve will have to cross the infinitely distant plane of space either in one point or in three points; and therefore it must have one pair or three pairs of branches extending to infinity in opposite directions.

The curve of the third degree is the horopter-curve; that is, corresponding lines of sight meet on this curve. Thus the three equations (4c) may be written also as follows:

$$\frac{A}{\mathfrak{A}} = \frac{B}{\mathfrak{B}} = \frac{C}{\mathfrak{C}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4f)$$

Now equations (4) are the equations of a pair of corresponding lines of sight; one of which is given by the pair of equations

$$\left.\begin{array}{l} l_0 A + m_0 B + n_0 C = 0 \\ l_1 A + m_1 B + n_1 C = 0 \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4g)$$

Suppose therefore that equations (4f) are satisfied by the point where this line of sight meets the curve of the third degree. Then when each of equations (4g) is multiplied by $\mathfrak{A}/A$, taking equations (4f) into account, the following pair of equations will be found to be satisfied also by the same point:

$$l_0\,\mathfrak{A} + m_0\,\mathfrak{B} + n_0\,\mathfrak{C} = 0\ ,$$

$$l_1\,\mathfrak{A} + m_1\,\mathfrak{B} + n_1\,\mathfrak{C} = 0\ ;$$

in other words, this point will be found to lie on the corresponding line of sight also. Hence, corresponding lines of sight intersect each other at points on the curve which is common to all three of the surfaces represented by equations (4c). This is the *horopter-curve*. The fact has been already stated that not all the portions of this curve are at the same time parts of the *horopter*.

Cones of the Second Degree which pass through the Horopter-Curve. If the two corresponding lines of sight represented by equations (4) meet each other in a point on the horopter-curve, then all the planes represented by equations (4a) that are determined by a pair of corresponding lines of sight will also pass through this same point; and consequently the lines in which these planes intersect each other will all go through this point too. These lines constitute a surface of the

second degree, and such a surface which is the abode of a family of straight lines of indefinite extent all passing through one point is known as a *conical surface of the second degree.*

Accordingly, every point on the horopter-curve is the vertex of a cone of the second degree, and the entire curve will lie on the surface of this cone. In special cases this cone may become a *cylinder* (that is, a cone with its apex at infinity), or it may collapse into a *pair of intersecting planes* (that is, a cone the elliptical base of which has one of its axes infinite).

Any straight line which meets the horopter-curve in two points will be on two of these cones, and hence it will be seen single.

When one of the cones can be transformed into a pair of planes, the horopter-curve will be composed of a conic section and a straight line which cuts the plane curve in one point. For in constructing the horopter curve we can use not only the pair of planes that represents a cone, but another cone which has its apex in one of these planes. This latter cone will intersect the pair of planes in a pair of straight lines and in a conic section; but one of the straight lines will not be a part of the horopter-curve.

Special Cases.—In order to calculate the real form of the horopter-curve in any given instance, it is necessary to express the magnitudes denoted by A, B, C and $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$ as actual functions of $\mathfrak{x}$, $\mathfrak{y}$, $\mathfrak{z}$. Suppose that the point of fixation is the origin of this system of coördinates, and that the visual plane is the $\mathfrak{x}\mathfrak{y}$-plane, the positive direction of the $\mathfrak{z}$-axis being upward. The bisector of the angle of convergence of the visual axes of the two eyes may be taken as the $\mathfrak{x}$-axis. Let the angle of convergence itself be denoted by 2γ; and let the distances of the points of intersections of the lines of sight in the right eye and in the left eye, from the point of fixation, be denoted by a and a_1, respectively. Then for the coördinates of the points of intersection of the lines of sight, we shall have:

In the right eye: $\mathfrak{x}=a\cos\gamma\,,\quad \mathfrak{y}=a\sin\gamma\,,\quad \mathfrak{z}=0\,;$

In the left eye: $\mathfrak{x}=a_1\cos\gamma\,,\quad \mathfrak{y}=-a_1\sin\gamma\,,\quad \mathfrak{z}=0\,.$

Now let us introduce another system of axes $(\mathfrak{x}_1,\ \mathfrak{y}_1,\ \mathfrak{z}_1)$, which is obtained by turning the first system around the $\mathfrak{z}$-axis through the angle γ, so that its $\mathfrak{x}_1$-axis will coincide with the visual axis of the right eye. Then

$$\mathfrak{x}_1=\quad \mathfrak{x}\cos\gamma+\mathfrak{y}\sin\gamma\,,$$

$$\mathfrak{y}_1=-\mathfrak{x}\sin\gamma+\mathfrak{y}\cos\gamma\,,$$

$$\mathfrak{z}_1=\mathfrak{z}\,,$$

these relations being such that, not only

$$\mathfrak{x}_1{}^2+\mathfrak{y}_1{}^2=\mathfrak{x}^2+\mathfrak{y}^2 ,$$

but, when $\mathfrak{x}_1=a$, $\mathfrak{y}_1=0$, we obtain the values given above for the coördinates of the point of intersection of the lines of sight of the right eye.

The $\mathfrak{x}_1$-axis here is the same as the z-axis in the system of coördinates xyz which was used first in deriving the equations (1) to (1i); so that

$$\mathfrak{x}_1=a-z+e .$$

With respect to the other system of coördinates, the xyz-system is turned through the angle ϑ which the retinal horizon makes with the visual plane; therefore,

$$x=\mathfrak{y}_1\cos\vartheta-\mathfrak{z}_1\sin\vartheta ,$$

$$y=\mathfrak{y}_1\sin\vartheta+\mathfrak{z}_1\cos\vartheta ,$$

The angle ϑ here is reckoned as positive when the upper part of the vertical meridian is turned to the right, that is, when the eyes are directed up on the left or down on the right. Consequently,

$$\left.\begin{aligned} x&=-\mathfrak{x}\sin\gamma\cos\vartheta+\mathfrak{y}\cos\gamma\cos\vartheta-\mathfrak{z}\sin\vartheta \\ y&=-\mathfrak{x}\sin\gamma\sin\vartheta+\mathfrak{y}\cos\gamma\sin\vartheta+\mathfrak{z}\cos\vartheta \\ z&=-\mathfrak{x}\cos\gamma \qquad +\mathfrak{y}\sin\gamma \qquad +a+e \end{aligned}\right\} \quad . \; . \; . \; . \; (5)$$

Hence, by using equations (3) in connection with equations (1h) and (1i) and certain other equations that come after them, the following equations will be derived:

$$\left.\begin{aligned} A&=y=-\mathfrak{x}\sin\gamma\sin\vartheta+\mathfrak{y}\cos\gamma\sin\vartheta+\mathfrak{z}\cos\vartheta \\ B&=x\cos\epsilon-y\sin\epsilon \\ &=-\mathfrak{x}\sin\gamma\cos(\vartheta+\epsilon)+\mathfrak{y}\cos\gamma\cos(\vartheta+\epsilon)-\mathfrak{z}\sin(\vartheta+\epsilon) \\ C&=a\,\mathfrak{z}-e=a-\mathfrak{x}\cos\gamma+\mathfrak{y}\sin\gamma \end{aligned}\right\} \qquad (5a)$$

Similarly, if the angle of torsional rotation for the left eye is denoted by ϑ_1, we obtain the following formulae for the magnitudes denoted by $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$;

$$\left.\begin{aligned} \mathfrak{A}&=+\mathfrak{x}\sin\gamma\sin\vartheta_1+\mathfrak{y}\cos\gamma\sin\vartheta_1+\mathfrak{z}\cos\vartheta_1 \\ \mathfrak{B}&=\mathfrak{x}\sin\gamma\cos(\vartheta_1-\epsilon)+\mathfrak{y}\cos\gamma\cos(\vartheta_1-\epsilon)-\mathfrak{z}\sin(\vartheta_1-\epsilon) \\ \mathfrak{C}&=a_1-\mathfrak{x}\cos\gamma+\mathfrak{y}\sin\gamma \end{aligned}\right\} \qquad (5b)$$

Simple Forms of the Horopter-Curve.—The form of this curve will be simplified whenever one pair of corresponding planes happens to be a pair of coincident planes. Then any line of sight for one eye, which lies in this plane, will intersect the corresponding line of sight for the

other eye, and thus determine a point on the horopter-curve lying in this plane. If the lines of sight are parallel, they will locate the infinitely distant points on the horopter-curve. In this case, therefore, part of the horopter-curve will be a plane curve or a straight line. Suppose that it is a plane curve; and that some point on it is taken as the apex of a cone on whose surface the horopter-curve is situated; then one portion of this conical surface must be a plane, and therefore the other portion can only be another plane. For it is not possible for a portion of the conical surface to be plane except in the limiting case when the cone collapses into a pair of intersecting planes. If these other planes, which do not contain the plane curved portion of the horopter, should all happen to intersect in one line, this can only be a straight line which must pass through a point on the plane curve. It follows at the same time that the curve itself must be of the second degree; otherwise, the cones whose vertices lie on the straight line could not be cones of the second degree.

Secondly, if the locus of the points of intersection of corresponding lines of sight is a straight line, then a portion of any cone, whose apex is at a point on the horopter-curve which is not on this straight line, will be a plane; that is, the cone really will be a pair of intersecting planes. Hence the rest of the horopter-curve must be a plane curve.

Obviously, too, when the horopter-curve is composed of a straight line a d a conic section, the centres of the two eyes must lie on the latter, and the plane in which the conic section is must be a pair of coincident, corresponding planes for the two eyes. For it is impossible for one eye to lie on the conic section, and the other eye on the straight line; because then a pencil of lines of sight could be drawn from the first eye to points on the conic section, lying therefore all in one plane; whereas the corresponding lines of sight for the other eye would be on a curved conical surface—which does not apply here. Or suppose that the two eyes were on the straight line; then it would have to be a pair of corresponding lines of sight, and then if there were some point on the horopter-curve, which was not on this straight line, as, for example, the point of fixation, the plane determined by this point and the centres of the two eyes would represent a pair of corresponding planes and would necessarily have to have an horopter-curve on it.

Hence, the condition, that the horopter-curve shall be composed of a conic section and a straight line intersecting it, implies that there must be certain values of l, m, n for which the equations

$$lA+mB+nC=0\ ,$$

$$l\mathfrak{A}+m\mathfrak{B}+n\mathfrak{C}=0$$

will be identical. By means of equations (5a) and (5b), these equations may be put in the following form:

$$f\mathfrak{x}+f_1\mathfrak{y}+f_2\mathfrak{z}+f_3=0\ ,$$

$$\varphi\mathfrak{x}+\varphi_1\mathfrak{y}+\varphi_2\mathfrak{z}+\varphi_3=0\ ,$$

and so we must have here:

$$\frac{f}{\varphi}=\frac{f_1}{\varphi_1}=\frac{f_2}{\varphi_2}=\frac{f_3}{\varphi_3}\ .$$

The last of these fractions is independent of l, m, n, whereas both numerator and denominator of the other three fractions are linear functions of these magnitudes. Thus by putting each of the first three fractions equal to the fourth, three linear equations in l, m, n will be obtained, without any absolute term. Consequently, the determinant of the coefficients of l, m, n must be equal to zero. In this way an equation will be obtained involving the magnitudes denoted by a, a_1, ϑ, ϑ_1 and γ, which must be satisfied when the horopter-curve has the form specified above. The reason why it is unnecessary to go through with this calculation here is because we are not particularly interested in any adjustments of the eyes which are not in accordance with LISTING's law.

The following is a geometrical method of expressing the above condition. Let the line joining the points of intersection of the lines of sight for the two eyes be denoted by F. This line may be considered as being one of the lines of sight of either eye. Thus if we think of it as belonging to the right eye, then the corresponding line of sight for the left eye may be denoted by H; or if we think of it as belonging to the left eye, the corresponding line of sight for the right eye may be denoted by G. If G and H intersect each other, then all three of the lines F, G, H must lie in one plane, and this plane, therefore, will be a pair of corresponding planes for the two eyes, since it contains two pairs of corresponding lines, namely, F and G for the right eye, and F and H for the left eye. Thus, for any adjustment of the eyes, by turning one of them around its visual axis, it will be possible to bring them in such a position that the horopter-curve will have the simple form specified above.

Suppose not only that LISTING's law is obeyed, but that the eyes are formed symmetrically, and that when their lines of fixation are parallel, the retinal horizons are in the visual plane. Then, evidently, the above condition will be satisfied, either (1) when the eyes are symmetrically adjusted so that the lines G and H are symmetrical also, meeting each other therefore in the median plane; or (2) when the visual plane is in its primary position, because then the retinal horizons corresponding to each other will lie in this plane. Theoret-

ically, these are not the only cases of this nature; for, supposing that Listing's law is obeyed perfectly, there will be certain positions of the point of fixation at very great distances away, when the eyes are directed downward and off to one side, for which the visual plane would be a pair of corresponding planes for the two eyes, which would therefore have to contain a plane ellipse as horopter-curve. However, these cases are of no practical importance at all, because, when the point of fixation is very far away, observations are too unreliable anyhow as to the points which are seen single. If there is no deviation of the vertical meridians of the eyes, the point of fixation in the case here mentioned will be at an infinite distance.

In such cases as the above, if small quantities are neglected, the following expression will be found for the distance (ρ) of the point of fixation as measured from the centre of an ideal eye supposed to be situated midway between the two real eyes:

$$\rho = \pm \frac{a \cos \gamma}{\sin \epsilon \sin \beta \cos \beta},$$

$$\tan \frac{\beta}{2} = \tan \frac{\alpha}{2} \tan \frac{\gamma}{2}.$$

where α denotes the angle of elevation which this imaginary eye would have, γ denotes its azimuth (p. 42), $2a$ denotes the inter-pupillary distance between the two real eyes, and 2ϵ denotes the angle between the apparently vertical meridians.

In the vicinity of the median plane where $\gamma = 0$, or in the vicinity of the primary position of the visual plane where $\alpha = 0$, the angle β vanishes, and the distance of the point of fixation becomes infinite. The angle α must be negative for positive values of ρ, that is, for positions of the point of fixation which are below the visual plane.

We shall proceed now to consider the first two cases mentioned above, in which the horopter is composed of a straight line and a plane curve. These cases have some practical importance for observations.

A. *Case when the point of fixation is at an infinite distance in the median plane.* Then in equations (5a) and (5b) we must put

$$a = a_1, \qquad \vartheta = -\vartheta_1,$$

$$\left.\begin{aligned}
A &= -\mathfrak{x} \sin \gamma \sin \vartheta + \mathfrak{y} \cos \gamma \sin \vartheta + \mathfrak{z} \cos \vartheta \\
B &= -\mathfrak{x} \sin \gamma \cos(\vartheta+\epsilon) + \mathfrak{y} \cos \gamma \cos(\vartheta+\epsilon) - \mathfrak{z} \sin(\vartheta+\epsilon) \\
C &= a - \mathfrak{x} \cos \gamma - \mathfrak{y} \sin \gamma \\
\mathfrak{A} &= -\mathfrak{x} \sin \gamma \sin \vartheta - \mathfrak{y} \cos \gamma \sin \vartheta + \mathfrak{z} \cos \vartheta \\
\mathfrak{B} &= \mathfrak{x} \sin \gamma \cos(\vartheta+\epsilon) + \mathfrak{y} \cos \gamma \cos(\vartheta+\epsilon) + \mathfrak{z} \sin(\vartheta+\epsilon) \\
\mathfrak{C} &= a - \mathfrak{x} \cos \gamma + \mathfrak{y} \sin \gamma
\end{aligned}\right\} \quad . \; . \; (6)$$

Coincident corresponding planes are found by putting

$$A \sin\gamma + C \cos\gamma \sin\vartheta = 0 \,,$$

$$\mathfrak{A} \sin\gamma + \mathfrak{C} \cos\gamma \sin\vartheta = 0 \,,$$

for then, on the supposition that $\sin\gamma$ and $\sin\vartheta$ do not both vanish at the same time, these two equations are identical with the following:

$$\mathfrak{x} \sin\vartheta - \mathfrak{z} \sin\gamma \cos\vartheta - a \cos\gamma \sin\vartheta = 0 \,, \quad . \quad . \quad . \quad . \quad (6a)$$

which, therefore, is the equation of the plane of the conic section. Moreover, for

$$\mathfrak{y} = 0 \text{ and } \mathfrak{x} \sin\gamma \cos(\vartheta+\epsilon) = -\mathfrak{z} \sin(\vartheta+\epsilon) \,, \quad . \quad . \quad . \quad . \quad (6b)$$

$$A = \mathfrak{A} = -\mathfrak{x} \sin\gamma \sin\vartheta + \mathfrak{z} \cos\vartheta \,,$$

$$B = \mathfrak{B} = 0 \,,$$

$$C = \mathfrak{C} = a - \mathfrak{x} \cos\gamma \,.$$

Accordingly, the points lying on the straight line given by equations (6b) will be corresponding points for the two eyes; and so this is the rectilinear portion of the horopter-line.

Not only must the (so-called) edges of the cylinder, on the surface of which the horopter-line is situated, be parallel to the rectilinear portion of this line, but the planes which intersect each other in a generating line of the cylinder must also be parallel to this same line. If the equations of corresponding planes are as follows:

$$A \cos\gamma \sin(\vartheta+\epsilon) - C \sin\gamma \cos\epsilon = 0 \,,$$

$$\mathfrak{A} \cos\gamma \sin(\vartheta+\epsilon) - \mathfrak{C} \sin\gamma \cos\epsilon = 0 \,,$$

then for $\mathfrak{y} = 0$ they will reduce to

$$\frac{a \tan\gamma \cos\epsilon}{\cos\vartheta} - \mathfrak{x} \sin\gamma \cos(\vartheta+\epsilon) - \mathfrak{z} \sin(\vartheta+\epsilon) = 0 \,.$$

Hence, as is evident from the second of equations (6b), the line of intersection of these planes is parallel to the rectilinear portion of the horopter-line, and lies in the median plane, as the latter does also.

On the other hand, according to equations (6b), the planes

$$B = \mathfrak{B} = 0$$

intersect each other in the rectilinear portion of the horopter-line; and hence the corresponding planes, whose equations are

$$A \cos\gamma \sin(\vartheta+\epsilon) + \kappa B - C \sin\gamma \cos\epsilon = 0$$

$$\mathfrak{A} \cos\gamma \sin(\vartheta+\epsilon) + \kappa \mathfrak{B} - \mathfrak{C} \sin\gamma \cos\epsilon = 0$$

will likewise intersect each other in lines that are parallel to the rectilinear portion of the horopter-line. The equation of the cylinder is obtained by eliminating κ from this pair of equations, and is therefore:

$$(A\mathfrak{B}-B\mathfrak{A})\cos\gamma\sin(\vartheta+\epsilon)-(\mathfrak{B}C-B\mathfrak{C})\sin\gamma\cos\epsilon=0\ .$$

After reduction, this equation becomes:

$$\left.\begin{aligned}\frac{a^2\sin^2\gamma\cos^2\epsilon}{4\cos^2\gamma\cos^2\vartheta}&=\mathfrak{y}^2\left[\sin^2\gamma\cos^2(\vartheta+\epsilon)+\frac{\sin\vartheta\cdot\sin 2(\vartheta+\epsilon)}{2\cos\vartheta}\right]\\&+\left[\mathfrak{x}\sin\gamma\cos(\vartheta+\epsilon)+\mathfrak{z}\sin(\vartheta+\epsilon)-\frac{a\sin\gamma\cos\epsilon}{2\cos\gamma\cos\vartheta}\right]^2\end{aligned}\right\}\qquad(6c)$$

which represents a cylinder, the sections of which made by the planes $\mathfrak{z}=$constant are conic sections. The x-axis of these conic sections is always real, and its length is:

$$X=\frac{a\cos\epsilon}{2\cos\gamma\cos\vartheta\cos(\vartheta+\epsilon)}$$

But the $\mathfrak{y}$-axis may not be real: and the square of its length is:

$$Y^2=\frac{a^2\tan^2\gamma\cos^2\epsilon}{4\cos\vartheta\cos(\vartheta+\epsilon)\left[\sin^2\gamma\cos(\vartheta+\epsilon)\cos\vartheta+\sin\vartheta\sin(\vartheta+\epsilon)\right]}\ .$$

In this latter expression, $\cos\vartheta$ and $\cos(\vartheta+\epsilon)$ will always be positive for any movements the eyes can make. But if $\tan\vartheta\tan(\vartheta+\epsilon)$ should become negative, and if its absolute value in this case happens to exceed $\sin^2\gamma$, the magnitude denoted by Y will be imaginary; and then the conic section will be an hyperbola. Generally, the value of ϵ will be a small positive quantity; and therefore the value of ϑ would have to be a still smaller negative quantity in order to fulfil the above condition. This could only happen when the visual axes of the two eyes were directed downward, with the point of fixation far away.

The Y-axis of this conic section lying in the visual plane coincides with that of the plane horopter-curve. In order to find the median axis of the latter curve, the value of $\mathfrak{z}$ as given by equation (6a) may be substituted in equation (6c); and then the coördinates can be found of the two extremities $(\mathfrak{x}_0, \mathfrak{z}_0)$ and $(\mathfrak{x}_1, \mathfrak{z}_1)$ of the required axis. This axis is always real, its length (X_1) being given by the equation.

$$X_1{}^2=\frac{1}{4}(\mathfrak{x}_1-\mathfrak{x}_0)^2+\frac{1}{4}(\mathfrak{z}_1-\mathfrak{z}_0)^2$$

$$=\frac{a^2\sin^2\gamma\cos^2\epsilon(\sin^2\gamma\cos^2\vartheta+\sin^2\vartheta)}{4\cos^2\gamma\cos^2\vartheta\,|\sin^2\gamma\cos\vartheta\cos(\vartheta+\epsilon)+\sin\vartheta\cdot\sin(\vartheta+\epsilon)\,|^2}\ .$$

Moreover,

$$\frac{X_1^2}{Y^2} = \frac{\sin^2\gamma + \tan^2\vartheta}{\sin^2\gamma + \tan\theta\cdot\tan(\vartheta+\epsilon)}.$$

Instead of using the cyclinder for constructing the horopter-curve, we may use also the cone of the *vertical horopter*, whose equation is

$$B\mathfrak{C} - \mathfrak{B}C = 0\ ,$$

that is,

$$[\mathfrak{x}\sin\gamma\cos(\vartheta+\epsilon) + \mathfrak{z}\sin(\vartheta+\epsilon)]\ [a - \mathfrak{x}\cos\gamma] - \mathfrak{y}^2\cos\gamma\sin\gamma\cos(\vartheta+\epsilon) = 0\ .$$

The section of this cone made by the visual plane $\mathfrak{z}=0$ will be a circle, whose equation is:

$$\left(\mathfrak{x} - \frac{a}{2\cos\gamma}\right)^2 + \mathfrak{y}^2 = \frac{a^2}{4\cos^2\gamma}.$$

This circle passes through the following points:

$$\mathfrak{x}=0\ ,\qquad \mathfrak{y}=0\ ;$$

$$\mathfrak{x}=\frac{a}{\cos\gamma}\ ,\qquad \mathfrak{y}=0\ ;$$

$$\mathfrak{x}=a\cos\gamma\ ,\qquad \mathfrak{y}=a\sin\gamma\ ;$$

$$\mathfrak{x}=a\cos\gamma\ ,\qquad \mathfrak{y}=-a\sin\gamma\ .$$

The first one of these points is the point of fixation. The second one is the point at the opposite end of the diameter. The other two points are the centres of the two eyes. These points determine the circle.

The median plane ($\mathfrak{y}=0$) cuts the cone in the pair of lines given by the equations:

$$\mathfrak{x}\sin\gamma\cos(\vartheta+\epsilon) = -\mathfrak{z}\sin(\vartheta+\epsilon)\ ,$$

$$\mathfrak{x}\cos\gamma = a\ .$$

The first one of these equations is the equation of the rectilinear part of the horopter-line; while the second equation represents a line which is perpendicular to the visual plane and meets it in the point on the circle which is at the opposite end of the diameter drawn through the point of fixation. Thus the coördinates of the apex of the cone are:

$$\mathfrak{x}=\frac{a}{\cos\gamma}\ ,$$

$$\mathfrak{z}=-a\tan\gamma\cdot\cot(\vartheta+\epsilon)\ .$$

In order to find the positions of the required lines and planes in the case of eyes whose movements are in accordance with LISTING's law,

let β denote the angle of elevation of the visual plane above its primary position; then

$$\tan\vartheta = \frac{\sin\gamma\sin\beta}{\cos\gamma+\cos\beta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

In this case equation (6a) which represents the plane of the horopter-curve, will become:

$$(\mathfrak{x}-a\cos\gamma)-\mathfrak{z}\frac{\cos\gamma+\cos\beta}{\sin\beta} = 0\,, \quad . \quad . \quad . \quad . \quad . \quad . \quad (7\text{a})$$

and under these circumstances the primary directions of the visual axes will be given by the following equations:

$$\mathfrak{y}=\pm a\sin\gamma \text{ and } \mathfrak{z}=(\mathfrak{x}-a\cos\gamma)\tan\beta \quad . \quad . \quad . \quad . \quad (7\text{b})$$

The equations of the actual positions of the lines of fixation are:

$$\mathfrak{z}=0 \text{ and } \mathfrak{y}=\pm\mathfrak{x}\tan\gamma \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7\text{c})$$

The point of fixation where these two lines intersect is at the distance a from the centres of the two eyes. On each of the lines represented by equations (7b) lay off the distance a as measured from the centre of the eye; then the coördinates of the point thus determined will be:

$$\mathfrak{x}=a(\cos\gamma-\cos\beta)\,,\quad \mathfrak{y}=\pm a\sin\gamma\,,\quad \mathfrak{z}=-a\sin\beta \quad . \quad . \quad (7\text{d})$$

On the other hand, consider a point which is midway between this point (7d) and the point of fixation whose coördinates are

$$\mathfrak{x}=0\,,\quad \mathfrak{y}=0\,,\quad \mathfrak{z}=0\,;$$

then the coördinates of this point will be just half of the coördinates of the point (7d), that is, they will be:

$$\mathfrak{x}=\frac{1}{2}a(\cos\gamma-\cos\beta)\,,\quad \mathfrak{y}=\pm\frac{1}{2}a\sin\gamma\,,\quad \mathfrak{z}=-\frac{1}{2}a\sin\beta\,. \quad . \quad (7\text{e})$$

Now these coördinates satisfy equation (7a), and hence the two points given by equations (7e) will both be in the plane of the horopter-curve.

Accordingly, when the point of fixation is in the median plane, the plane of the conic section which is part of the horopter-curve may be found by bisecting the angles between the primary position and the actual position of each line of fixation, and passing a plane through the two bisectors. This was the method used in drawing Fig. 70.

Moreover, the equation of a plane which goes through the centre of one eye and is perpendicular to the straight line which joins this point with that one of the points of equations (7e) that belongs to that eye will be:

$$(\mathfrak{x}-a\cos\gamma)\,(\cos\gamma+\cos\beta) - (a\sin\gamma\mp\mathfrak{y})\sin\gamma+\mathfrak{z}\sin\beta=0\,. \quad (7\text{f})$$

Now the equation of a plane, which is below the primary position of the visual plane as given by equation (7d), and at the distance $-a \sin \gamma \cdot \cot \epsilon$ from this plane, is:

$$\mathfrak{z} \cos \beta + a \cot \epsilon \sin \gamma = (\mathfrak{x} - a \cos \gamma) \sin \beta \quad . \quad . \quad . \quad . \quad (7g)$$

And so we see that the planes which pass through the rectilinear portion of the horopter-line, whose equations are

$$\mathfrak{x} \sin \gamma + \mathfrak{z} \tan(\theta + \epsilon) = 0 \,, \qquad \mathfrak{x} = 0$$

and the two planes represented by equations (7f) and (7g) all pass through a single point; for if the values of $\mathfrak{x}$, $\mathfrak{y}$, $\mathfrak{z}$ as obtained from three of these equations are substituted in the fourth, the result will be an identity, provided equation (7) is taken into account. It was on this property that the construction of the horopter-line was based, as given in Fig. 71.

B. *Case when the point of fixation is at an infinite distance in the middle plane.*—Another case which deserves special study is the case when $\sin \gamma$ and $\sin \vartheta$ both vanish at the same time; which was the case we left out of account when we were considering equation (6a). Then the visual axes will be parallel and directed to infinity. The distance (a) of the point of fixation and the abscissa $\mathfrak{x}$ will both be infinite, whereas the interpupillary distance $2a \sin \gamma = 2b$ (say) will remain constant. Putting $\mathfrak{x} - a = \xi$, we may write:

$$A = \mathfrak{z} \,, \qquad \mathfrak{A} = \mathfrak{z} \,,$$

$$B = -b \cos \epsilon + \mathfrak{y} \cos \epsilon - \mathfrak{z} \sin \epsilon \,, \qquad \mathfrak{B} = b \cos \epsilon + \mathfrak{y} \cos \epsilon + \mathfrak{z} \sin \epsilon \,,$$

$$C = -\xi \,, \qquad \mathfrak{C} = -\xi \,.$$

In this case the conditions of correspondence, namely,

$$A = \mathfrak{A} \,, \qquad B = \mathfrak{B} \,, \qquad C = \mathfrak{C}$$

will be perfectly satisfied for all points for which

$$b \cos \epsilon + \mathfrak{z} \sin \epsilon = 0 \,.$$

These points lie in a plane which is at the distance $-b \cot \epsilon$ below the visual plane; and so in these cases the horopter is formed by this plane.

C. *Case when the point of fixation is in the primary position of the visual plane.*

According to Listing's law, we must have:

$$\vartheta = \vartheta_1 = 0$$

and therefore by equations (5a) and (5b):

$$\left.\begin{aligned} &A=\mathfrak{z}\\ &B=-\mathfrak{x}\sin\gamma\cos\epsilon+\mathfrak{y}\cos\gamma\cos\epsilon-\mathfrak{z}\sin\epsilon\\ &C=a-\mathfrak{x}\cos\gamma-\mathfrak{y}\sin\gamma\\ &\mathfrak{A}=\mathfrak{z}\\ &\mathfrak{B}=\mathfrak{x}\sin\gamma\cos\epsilon+\mathfrak{y}\cos\gamma\cos\epsilon+\mathfrak{z}\sin\epsilon\\ &\mathfrak{C}=a_1-\mathfrak{x}\cos\gamma+\mathfrak{y}\sin\gamma \end{aligned}\right\} \quad . \; . \; . \; . \; . \quad (8)$$

The cone

$$A\mathfrak{C}-\mathfrak{A}C=0$$

becomes

$$\mathfrak{z}(a_1-a+2\mathfrak{y}\sin\gamma)=0\,, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (8a)$$

and hence it consists here of the pair of planes whose equations are:

$$\mathfrak{z}=0 \text{ and } \mathfrak{y}=\frac{a-a_1}{2\sin\gamma} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (8b)$$

The equation of the surface

$$A\mathfrak{B}-\mathfrak{A}B=0$$

becomes:

$$2\mathfrak{z}(\mathfrak{x}\sin\gamma\cos\epsilon+\mathfrak{z}\sin\epsilon)=0\,,$$

which is the equation of the pair of planes:

$$\mathfrak{z}=0 \text{ and } \mathfrak{x}\sin\gamma+\mathfrak{z}\tan\epsilon=0 \quad . \; . \; . \; . \; . \; . \; . \; . \quad (8c)$$

And, lastly, the equation of the surface

$$B\mathfrak{C}-\mathfrak{B}C=0$$

becomes:

$$-(\mathfrak{x}\sin\gamma\cos\epsilon+\mathfrak{z}\sin\epsilon)\,(a_1+a-2\mathfrak{x}\cos\gamma)+2\mathfrak{y}^2\cos\gamma\sin\gamma\cos\epsilon \\ +(a_1-a)\mathfrak{y}\cos\gamma\cos\epsilon=0\,,$$

which is the equation of an hyperboloid. The section of this surface made by the plane $\mathfrak{z}=0$ is a circle whose equation is:

$$\left(\mathfrak{x}-\frac{a+a_1}{4\cos\gamma}\right)^2+\left(\mathfrak{y}+\frac{a_1-a}{4\sin\gamma}\right)^2=\frac{a^2+a_1{}^2-2aa_1\cos 2\gamma}{4\sin^2 2\gamma}\,.$$

This is MÜLLER's horopter-circle which goes through the points:

$$\begin{aligned} &\mathfrak{x}=0\,, && \mathfrak{y}=0\,;\\ &\mathfrak{x}=a\cos\gamma\,, && \mathfrak{y}=a\sin\gamma\,;\\ &\mathfrak{x}=a_1\cos\gamma\,, && \mathfrak{y}=-a_1\sin\gamma\,. \end{aligned}$$

Accordingly, the rectilinear part of the horopter is the straight line determined by the intersection of the pair of planes referred to in equations (8b) and (8c), namely:

$$\mathfrak{y} = \frac{a - a_1}{2 \sin \gamma} \quad \text{and} \quad \mathfrak{x} \sin \gamma + \mathfrak{z} \tan \epsilon = 0 \ .$$

This straight line crosses the visual plane also at a point in the horopter-circle, and is parallel to the median plane $\mathfrak{y} = 0$. The point where it crosses the visual plane is equidistant from the centres of the two eyes, this distance being

$$\frac{\sqrt{a^2 - 2aa_1 \cos 2\gamma + a_1^2}}{2 \sin \gamma} = \frac{b}{\sin \gamma} \, ,$$

where $2b$ denotes the interpupillary distance. Putting

$$\mathfrak{x} = \frac{b}{\sin \gamma} \, ,$$

we obtain:

$$\mathfrak{z} = - \frac{b}{\tan \epsilon} \, .$$

But this is the distance of the horopter-surface below the visual plane, when the visual axes of the two eyes are parallel to the median plane; and thus we obtain the construction of the rectilinear part of the horopter-line, as given above.

The ancients tried to furnish some explanation of single vision and double vision. GALENUS[1] endeavoured to account for single vision by assuming that the nerve-fibres were connected in the chiasma of the optic nerve. This anatomical hypothesis was afterwards espoused by Sir ISAAC NEWTON,[2] ROHAULT,[3] HARTLEY,[4] W. H. WOLLASTON,[5] and JOH. MÜLLER.[6] Another way of getting round the difficulty of single vision consisted in assuming that we never saw anything except with one eye at a time. This was PORTA's explanation;[7] which was adopted by GASSENDI,[8] TACQUET, GALL and DU TOUR.[9] The latter based his opinion chiefly on the phenomena connected

[1] *De usu partium.* Lib. X, cap. 12.

[2] *Opticks.* 1717. p. 320. Query 15.

[3] *Traité de physique.* Paris 1671 and 1682. Part I, cap. 31.

[4] *Observations on man.* I, 207.

[5] *Phil. Trans.* 1824. I, 222.

[6] *Zur vergleichenden Physiologie des Gesichtssinns.* Leipzig 1826.

[7] *De refractione.* p. 142. 1593.

[8] *Opera.* Vol. II, p. 395.

[9] *Acta Paris.* 1743. p. 334.—*Mém. des savants étrang.* III, 514; IV, 499; V, 677.

with the rivalry between the two visual fields; his idea being that vision consisted in seeing things sometimes with both eyes and sometimes with only one eye.[1]

The so-called projection theory is different from both of the hypotheses above mentioned. According to it, single vision is a mental interpretation of the sensations of vision. This was KEPLER's explanation.[2] At the same time AGUILONIUS[3] proposed the theory that the visual images were always projected on a certain plane passing through the point of fixation. He called this plane the *horopter*, and argued that the images appeared single or double according as they were projected singly or doubly.[4] PORTERFIELD's conception was more like that of KEPLER, since, according to him, the reason why we do not see things double is because each eye projects the object to its correct place. Subsequently, this idea was formulated by saying that the object is supposed to be at the place where the lines of sight intersect. Expressed in this way, the law amounts to saying that there are no such things as double images. It is true, PORTERFIELD speaks of images of this sort which occur when the eye is in an unnatural state due to pressure or disturbance of some kind, but he assumes that these images are produced by some fault in the adjustment of the eye.

These three views of the matter, more or less combined with each other, are also at the basis of most of the more modern theories; although a distinct advance has been made by carefully studying the actual relations.

The law of the phenomena was first formulated exactly, and in the main correctly, by J. MÜLLER.[5] According to him, single vision and double vision did not depend on whether the images of the given point were projected on identical or non-identical points of the two retinas. His rule for the positions of identical points, which is on the whole correct, was that these corresponding places were equidistant and in the same direction from the centres of the two retinas. And although he does not expressly commit himself to any particular anatomical hypothesis (such as union of identical fibres in the chiasma of the optic nerve or in the brain), he does say that there must be some organic basis for this identity or correspondence.

Subsequent investigations, especially those made by VOLKMANN,[6] supplied more definite information as to the precise positions of the identical or cor-

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[1] ¶Attention should be called to a fragment entitled "De l'oeil et de la vision" written by HUYGENS and published in Vol. XIII, pp. 730-799 of his collected works (*Œuvres complètes de* CHRISTIAAN HUYGENS *publiées par la Société Hollandaise des Sciences*); wherein he indicates very distinctly the conditions of single binocular vision. "La nature a pourvu d'une maniere bien particuliere a ce qu'ils [les deux yeux] ne fissent pas paraitre l'objet double. C'est qu'elle a fait que chaque point du fond de l'œil a son point *correspondant* dans le fond de l'autre en sorte que lors qu'un point de l'objet est peint dans quelques deux de ces *points correspondants*, alors il ne paroit que simple comme il est." These two points, he says, are both "du mesme costé des axes et ne pas disposez semblablement a l'egard des deux nerfs optiques;" and "d'icy il est aisè de voir pourquoy un object éloignè doit paraitre double lors qu'on dispose les yeux pour regarder un autre object plus proche, et pourquoy au contraire l'object proche se doit doubler en voiant simple celuy qui est plus distant." —(J.P.C.S.)

[2] *Dioptrice*. Propos. LXII.

[3] *Opticorum Libri VI*. Antwerp. 1613.

[4] ¶M. v. ROHR, Auswahl aus der Behandlung des Horopters bei FR. AGUILONIUS um 1613. *Zft. f. ophthalm. Optik*, 11 (1923), 41-59. (J.P.C.S.)

[5] *Beiträge zur vergleichenden Physiologie des Gesichtssinns*. Leipzig 1826. p. 71.- *Lehrbuch der Physiologie*. 1840. II, 376-387.

[6] *Physiologische Untersuchungen im Gebiete der Optik* by A. W. VOLKMANN. Zweites Heft. Leipzig 1864.

responding points. But these observations were incompatible with the hypothesis of AGUILONIUS, according to which the horopter was a plane surface. VIETH[1] and JOH. MÜLLER had perceived that the visual plane must cut the horopter in a circle going through the point of fixation and the centres of the two eyes. And, later, A. P. PREVOST[2] and BURCKHARDT showed that, when the eyes were adjusted without torsional rotations, there was a straight line in addition to MÜLLER's horopter-circle, and that therefore the horopter could not generally be any surface. HERING[3] showed that as a rule the horopter was necessarily always a line; and thus the original conception of it which AGUILONIUS had in mind was proved to be erroneous. The general solution of the problem of the horopter is a purely mathematical exercise, which, however, does involve knowing about the movements of the eyes. This problem was solved by myself and by Mr. E. HERING practically about the same time.[4] In addition there was a contribution on the subject by

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[1] GILBERTS *Annalen*. LVIII, 233.

[2] *Essai sur la théorie de la vision binoculaire*. Genève 1843; and POGG. *Ann*. 1844. Bd. LXII, p. 548.

[3] *Beiträge zur Physiologie*. Heft III, pp. 196–199. Leipzig 1863. Heft IV, 1864.

[4] My first contribution on the subject was made before the Heidelberg Medical Society of Natural History, October 24, 1862; the manuscript being transmitted on November 8, 1862. In this paper the equations of the form of the horopter in general were given for the first time, although they were not expressed in their simplest forms, because the horopter was regarded as being a curve made by the intersection of a surface of the second degree and one of the fourth degree. Nor was the deviation of the apparently vertical meridian taken into account in this paper. The form of the horopter in the general case was briefly described. Before this merely preliminary contribution had been actually published (which was not until the autumn of 1863), the third part of Mr. E. HERING's *Beiträge zur Physiologie* appeared, and there the proof was given that the horopter must certainly be always a line at least (if not a surface); but the form of the horopter was not actually determined except in the simpler cases which had been previously considered. Then followed my article on the horopter in the *Archiv für Ophthalmologie*, X, 1, pp. 1-60, the proof of which had been corrected by the middle of March 1864; and in this paper the horopter was considered as being the intersection of two surfaces of the second degree, and the effect of the deviations of the apparently vertical meridians was taken into account. Without being aware of this publication, Mr. E. HERING had sent the printers the fourth instalment of his work in June 1864, in which also the horopter was considered as being the intersection of two surfaces of the second degree, and where the method of STEINER's geometry was used, which is particularly adapted to this problem. HERING's criticism here of my earliest contribution was due mainly to his failing to see that what I was talking about was what I have referred to above as the "horopter," whereas what he himself had in mind was the "horopter-curve." The two are not exactly identical, as I have explained in POGGENDORFFS *Annalen*, CXXIII, pp. 158-161. Finally, the fifth instalment of HERING's *Beiträge zur Physiologie* contained a further criticism of my second paper on this subject. I shall allude here to only one point in this latter criticism (see p. 350), about which Mr. E. HERING was really correct, namely, where he says that on page 44 of my paper the angle η has been generally put equal to η_1. I must confess this was an oversight made when I was just about to start on a journey; I was going over the paper very hurriedly for the last time, trying to condense the mathematical work as much as possible. Formerly, I had treated separately the two cases in which the statement objected to was correct; and so the mistake did not have any effect on the correctness of the subsequent deductions. Some of the other strictures which Mr. HERING makes are matters of personal interest only and may be answered by readers who wish to do so, without need of further reply from me. There are other points of controversy which can be settled only by repeated observations of numerous individuals. I have tried to cite as many of these observations in the text as I could find.

H. Hankel,[1] where the problem was treated analytically in much detail; without, however, taking into account the deviations of the apparently vertical meridians, which have a very important bearing here.

Ever since Wheatstone's invention of the stereoscope, the investigator's chief interest has been with regard to the fusion of the double images, because the theoretical questions as to the way in which the two eyes act in harmony are concerned especially with this matter. These questions cannot be finally settled until we come to the end of the next chapter. The importance of the movements of the eyes on the fusion of the disparate images, not only of material objects but of stereoscopic drawings, was shown first by Brücke.[2] On the other hand, by using electrical illumination, Dove[3] proved that this fusion could be produced also without any movements of the eye whatever, although to a much less extent. With some modifications his experiments were afterwards repeated and confirmed by Volkmann,[4] August[5] and Recklinghausen.[6] The works of Panum[7] and Volkmann[8] may be cited especially on account of the immense number of careful observations and measurements which they contain concerning the limits of fusion and the conditions that are required for it. Wheatstone's experiment, in which the impressions made on identical points could be utilized to complete various parts of the perceptual image of the observed material objects, was the subject of much controversy. It was verified by Nagel[9] and Wundt.[10] On the other hand, the argument against it (as urged by Volkmann,[11] E. Hering,[12] and W. Bezold[13]) was, that by paying sufficient heed to the double images and using the proper means to make them easily visible, they could always be seen separately. I have maintained that there is no necessary conflict between these two contentions.

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113. Galenus, *De usu partium.* Lib X, c. 12.
1593. Porta, *De refractione.* p. 142.
1611. Kepler, *Dioptrice.* Propos. LXII.
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1658. Gassendi, *Opera.* Vol. II, p. 395.
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1704. I. Newton, *Optice.* Query XXV.
1743. Du Tour, *Act.* Paris 1743, p. 334.
1759. Porterfield, *On the eye.* II, 285.

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1 Pogg. *Annalen.* CXXII, 575–588.

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3 *Monatsber. d. Berl. Akad.* July 29, 1841.

4 *Leipz. Berichte.* 1859, pp. 90–98.

5 Pogg. *Ann.* CX, 582–593.

6 *Ibid.*, CXIV, 170–173.

7 *Physiologische Untersuchungen uber das Sehen mit zwei Augen.* Kiel. 1858; and in Reichert und du Bois Reymonds *Archiv.* 1861. pp. 63–227.

8 *Archiv f. Ophthalmologie.* II, 2, pp. 1–100; and *Physiol. Untersuchungen im Gebiete der Optik.* Heft II.

9 *Das Sehen mit zwei Augen.* Leipzig and Heidelberg, 1861.

10 Henle und Pfeuffer *Zeitschr. f. ration. Medizin.* (3) XII, 249

11 *Archiv für Ophthalmol.* II, 2, pp. 72–86.

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Notes on §31 by v. Kries

1. According to a conjecture, which was afterwards made by Hering, and which is at variance with the results of Volkmann's, experiments as given on page 414, it is not necessary for the angular distance of a point on the retinal horizon as measured from the centre of the retina to be exactly the same in both eyes, in order that it may make the impression of being equally far from the point of fixation; but the angular distance must always be rather greater for that eye for which this distance is on the nasal side of the retina (or on the "lateral" portion of the field of view). This assumption was alluded to in the text. The principal basis for it is the so-called Kundt illusion, as was also stated in that place. If a person will look with only one eye at a horizontal line extending from left to right, and try to divide it exactly in half, he will discover that he is apt to place the middle of the line too far over on the median side of the eye which is being used for the test. This implies that a horizontal angular width on the nasal half of the retina should be somewhat less than a similar dimension on the other side, for the two to seem to be of equal length. It is justifiable to suppose that the two eyes are symmetrical in this respect; and on this assumption, the conclusion would be that corresponding points are related as Hering supposed.

Accordingly, we must consider once more some observations which have been mentioned previously, and which are concerned with the arrangement that objects should have in order to make them appear as if they were in a plane perpendicular to the line of fixation (that is, in Hering's so-called *Kernfläche*, p. 379). On Hering's assumption (*loc. cit.*), this would be the case, provided the objects were situated on the longitudinal horopter (*Langshoropter*). And so, as was stated there, this particular arrangement is sometimes spoken of concisely as the "empirical longitudinal horopter." As a matter of fact, Hillebrand's experiments show that the latter must differ from Müller's circle in much the same way as the horopter would be different from it, supposing that intervals appear smaller when they are on the temporal side of the field of view than they do when they are on the nasal side; that is, on the hypothesis that there is such a difference as Kundt's ex-

periments seem to indicate. It is quite common, therefore, to speak of this difference in the form of the horopter from the simple form which it was supposed to have at first as the HERING-HILLEBRAND horopter-deviation. Thus far there has not been any direct experimental demonstration of these relations by means of correspondence-experiments similar to those made by VOLKMANN; and, doubtless, there would be serious difficulties about verifying them in this way.

It should be added that, only in so far as HERING's rule (which has been repeatedly mentioned) may be regarded as being applicable, can any proof of a definite form of the horopter be obtained from experiments on depth-localization. If all the phenomena which belong here could be readily explained on the two assumptions which are involved (namely, the horopter-deviation and HERING's rule as to the localization on the *Kernfläche*), and especially if the quantitative relations between them were found to be consistent, there might, indeed, be some justification in considering these confirmations as amounting to some sort of proof of the *truth* of both assumptions. There are some conditions for which FRANK's experiments,[1] which were mentioned on page 378, do seem to indicate an agreement of this kind. However, when these observations were being discussed, it was stated then that they were not quite thorough enough to enable us to come to a final decision with respect to localization of depth. And so, in my opinion, all that can be said at present is that the KUNDT illusion seems to indicate that the horopter-deviation is very probably a fact; but that there is still, for the time being at least, much reason to doubt whether HERING's law is always applicable, and whether, therefore, observations on localization of depth can be regarded as proving the horopter-deviation. TSCHERMAK and KIRIBUCHI[2] were led by their experiments (see p. 379) to assume that to some extent the form of the horopter was dependent on the nature of the visual impression. It has been stated that the purpose of their experiments was to locate the positions where vertical lines were apparently in a plane perpendicular to the direction in which the eyes were looking. They made a determination of the horopter, which was based on HERING's assumption, that is, that the places where the vertical lines were had to be points on the longitudinal horopter. TSCHERMAK and KIRIBUCHI found that a different arrangement was required when the observations were made with plumb lines which were permanently in the field of view from that which was necessary when the objects were balls falling by gravity; which led them to make a distinction between what they

[1] FRANK, PFLÜGERS *Archiv*, 109. 1905, p. 63.

[2] PFLÜGERS *Archiv*, 81. 1900, p. 328.

called the plumb-line horopter (*Lothoropter*) and the drop-test horopter (*Fallhoropter*). But it is hard for me to see how the horopter, or rather this relation of correspondence, could be dependent in this way on some special property of the observed object. At least, it would seem to be incompatible with the assumption that a definite azimuth-value is associated with each point on the retina. Consequently, as has been said already, I am disposed to think that it would be more correct to regard these experiments as merely indicating the fact that there is no absolutely fixed connection between the *depth-localization* and the cross-disparities, but that the former is dependent on the special form of the optical stimulus or observed object. Strictly speaking, therefore, observations such as those here under discussion should not be considered as being horopter-determinations.

2. The phenomena mentioned on page 431 have been discussed and studied from a different point of view, namely, on the supposition that there is no connection between binocular perception of depth and the fusion of the images in a unitary impression, and that even when the double images are distinct, perception of depth is still possible. Accordingly, allusion was made to these researches in Note 11, at the end of the previous chapter.

3. Special conditions, evidently connected with the relations of rivalry between the two retinas, are responsible for the difficulty which many persons have in being able to see the double images (p. 437). When an individual who has not had much training in making such observations is bothered by these details, my experience is that the best way to enable him to see the double images is not to confuse him by making him look at single objects at various distances, but to let him regard some unitary body extending some distance away from him. Thus, for example, one end of a stout cord may be fastened to a wall about on a level with the observer's eye. The length of the cord should be about two metres, and the observer should stand about a metre and a half from the wall, and hold the cord against the root of his nose. Then if he will place his finger somewhere on the cord and gaze at it, he will seem to see two cords crossing each other at that place. If the observer's eyes were normal, I have never known this experiment to fail, although it might be quite impossible for him to see single objects double (such as fingers or candle flames), no matter how hard he tried.

4. The ability of distinguishing between the impressions in the two eyes (see p. 459) may be interpreted in quite a variety of ways. It takes some pains to differentiate between these various meanings,

but it is worth trying to do so, as it is a matter of some theoretical importance. Looking at it first from the point of view of the facts of actual observation, we may suppose that the chief question here is the one that is mentioned in the text; that is, whether it makes any difference, so far as impression of depth is concerned, whether non-corresponding places a and b are stimulated in the two eyes, or whether the places a' and b' are stimulated that correspond to a and b, respectively. This question simply amounts to asking whether it is possible to reverse the binocular estimate of depth (that is, to reverse the relief). As a matter of fact, in one case the impression of depth is positive, in the other case it is negative. The experiments which are cited in the text show that this reversal is something that does not take place, even when movements of the eyes are prevented. Subsequent experiments have repeatedly verified this fact.

In the experiments which AUERBACH and myself carried out on time-discriminations,[1] electric sparks were discharged both in front of the point of fixation and beyond it, at such distances from it that in both cases double images were observed at equal intervals from each other. In one case, therefore, these images were crossed (heteronymous), and in the other case uncrossed (homonymous). The experiments indicated that it took from 22 to 30 [seconds] to perceive the position as far as depth was concerned. The positions were never confused. More recently it has been proved again and again, that even when the observations are very brief, and no ocular movements are involved, the faculty of perceiving depth still exists; because in the case of persons afflicted with strabismus this faculty is a matter of particular concern. At present the simple apparatus devised by HERING is generally used to show that these patients possess this ability, and it appears to be satisfactory in most cases. The patient is required to look in a pasteboard tube, where there is a fixation-mark. Several openings are made in the tube both above and below, and through these apertures tiny marbles can be made to fall. Naturally, they will glide very rapidly across the field of view. The apertures can be adjusted so that the little objects will descend in front of the point of fixation, or beyond it. Anybody who has normal vision can look in the tube with both eyes and always tell positively whether the marble passed the point of fixation in front of it or beyond it This would seem to indicate that there is some discrimination between the impressions made on the two eyes, as to their significance in producing ideas of depth. But whether this discrimination is similar to our being able to tell whether we touch an object with the right hand

[1] *Archiv für Physiologie*. 1878.

or with the left hand—that is another question. The similarity between the two cases would be perfect, provided that, whenever a luminous stimulus acted on only one eye, the observer could tell which eye was affected. HELMHOLTZ does, indeed, make some reference to this matter, but apparently it was based more on a casual observation than on direct investigation. These relations have been recently carefully studied by HEINE.[1] He shows that in certain cases it is possible indeed to distinguish between impressions in one eye or the other. When a luminous point is caused to flash out in a dark room where it is visible to only one of the observer's eyes, he can generally tell which eye is the one that sees it. This is found to be true even when special precautions are taken to avoid possible sources of error, such as might be present, for example, if there were a difference between the directions in which the luminous signal would be seen by each eye. BRÜCKNER and v. BRÜCKE[2] repeated this experiment, and while they did not verify it in case of all the observers, they did so for many of them. At the same time they succeeded in ascertaining certain conditions which are involved in a discrimination of this nature. For instance, suppose an observer gazes at a white chart through two tubes which are painted black on the inside, and that the circular apertures at the far ends of the tubes are fused stereoscopically. If then a tiny object moves past one of the apertures, the spectator *never* can tell with which eye he sees it, according to BRÜCKNER and v. BRÜCKE. On the other hand, their investigations apparently show that a discrimination between the two eyes is possible, provided one of the eyes does not see anything at all; that is, provided the entire field was absolutely dark except for a single luminous point that was visible to only one eye; or provided the visual impressions in one eye are on the whole much less intense than they are in the other eye, as, for instance, when one eye has not been dark-adapted, or when the images are obliterated by convex lenses, etc. A very remarkable instance of this sort, which is mentioned by these investigators, is the peculiar sensation which is produced by bandaging one eye, and then, when this eye has become dark-adapted, removing the bandage in a room which is very dimly illuminated. We experience then a characteristic feeling as if there were something in front of the other eye which hindered vision. Their idea, therefore, was that it was not so much a question of being able to discriminate between the impressions in the two eyes as of having that feeling of non-vision or faulty vision, which

[1] HEINE, *Klinische Monatsblätter*. 39.—Idem, PFLÜGERS *Archiv*. 101.

[2] BRÜCKNER u. v. BRÜCKE, PFLÜGERS *Archiv*., 90. 1902. p. 290. Ibid., 91. 1902. 360.—Ibid., 107. 1905. p. 263.

they call an organ-feeling (*Organgefühl*).—In view of what is indicated by these facts, I should say that we ought not to be misled by the use of this term *Organgefühl* into supposing that the discrimination here manifested is connected with the optical sensations or has reference to them. Non-vision or poor vision may also be a psychic condition, which we speak of as being a special state of the optical sensations, and which may be related to a behaviour of the "visual substance" ("*Sehsubstanz*"), as HERING calls it. Anatomically, this substance consists of the fibres of the optic nerve and of the structures adjacent to this nerve which lie toward the centre of the retina. But we cannot suppose that this state has anything to do with some processes in the domain of the tactile nerves, such as those, for instance, which emanate from the conjunctiva or from the cornea. On the other hand, there would be a manifest inconsistency in attributing some psychic peculiarity to the impression of vision alone, by which we were enabled to tell immediately whether this impression was in one eye or in the other.

Hence, if the question is simply whether there is any difference in the result which is produced when a stimulus acts at one place in one eye or at the corresponding place in the other eye, and if so, how it is different, the answer is bound to be that there certainly is some difference. The result is not the same in the two cases in various respects. But we cannot prove that the difference is in some definite psychic quality belonging to one impression or the other. In many instances no psychic differences are involved or at least none that can be proved. And so, in my judgment, we are obliged to suppose that any regular difference in the result in the two cases is due to some characteristic physiological (or anatomical) difference, which in certain special conditions may have psychic manifestations (in estimating depth or in some feeling of not seeing, etc.), but which may also sometimes have no psychic correlation at all.[1]—K.

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§32. Rivalry between the Visual Globes of the Two Eyes

In the two previous chapters we have seen how in ordinary, natural binocular vision the images of material objects are projected in the space in front of our eyes; and yet that, by taking notice of the common field of view as such, it was possible also to discern the two different perspective projections made by objects on the two retinas and to realize how they are superposed on the surface of this field. The

[1] Further considerations leading to this conclusion will be reserved for the discussion of this subject in the Appendix.

ordinary mode of vision is employed mainly in looking at real concrete things and is concerned primarily with ascertaining what they are. Then the axes of the two eyes are directed toward the particular object that happens to interest us at the moment, so that we always see it single and distinct. But other objects, which are nearer or farther away, and which, as seen by the more or less indirect vision at the time, might appear double, are disregarded. In order to see double images, we must pay heed to our visual impressions as such, and try to dissociate them from the perceived objects. Instead of looking at real objects, the best way to see the double images and the corresponding phenomena of congruence or lack of congruence of the individual points on the visual globes of the two eyes, is to gaze at two different sketches in which there are lines and areas in different colours or illuminations, similar to the diagrams which were used above for finding corresponding places in the two fields.

The double images which were observed in the previous cases were more or less similar to the images which are sometimes obtained accidentally of one and the same external object. They were familiar to us, and we recognized them as indicating an object that was not on the horopter. Indeed, they might enable us to tell quite accurately how far away the object was that corresponded to them.

But now we have to consider those cases in which the two fields are filled with forms which are wholly different in appearance, and which do not admit of being combined into the image of a single object. Under such circumstances, both images will generally be seen at the same time superposed on each other in the field of view. Ordinarily, however, in the various parts of the field, one image will prevail more than the other, whereas in other parts the other image will predominate. Sometimes there will be alternations, so that, where for a while only parts of one image were visible, presently parts of the other image will emerge and suppress portions of the first image. This fluctuation, in which the parts of the two images mutually supplant each other, either side by side, or one after the other, is what is usually meant by *the rivalry between the visual globes.*

The simplest and most regular cases are those in which the whole extent of the visual globe of one eye has a uniform colour or illumination; because then the only objects that will be noticed will be those seen on the visual globe of the other eye. Thus, for instance, when one eye is closed, and a printed page observed with the other eye, the letters and the white paper in the field will be seen without perceiving the darkness of the other visual globe. It should be remarked here that in this case the paper does not look decidedly darker than it looks when it is exposed to both eyes; in other words, the black in one field

is not mixed in with the white in the other field, but simply has no influence at all on the appearance of the other image.

Now it is the same way when the eye which was closed is opened, and a sheet of white paper is held close up to it, so that the visual globe, which was dark before, becomes uniformly white all over. Then too the letters in the other field will not appear to be altered; and unless the uniformly white paper is brighter than the sheet of printed paper, the latter will not look any brighter when the other visual globe is white all over than it did when it was black all over. But if we turn around so as to let the sunlight fall directly on the white paper in front of one eye, then on opening this eye, we undoubtedly do get the impression that the printed sheet is made brighter by illuminating the other visual globe than by keeping it dark.

Even when there are merely large illuminated portions on the visual globe of one eye, and figures on the corresponding portions of the visual globe of the other eye, the result will be similar. Thus on looking, for instance, at the following set of letters

A B   B C

so that the two *B*'s are fused into a single one, the appearance obtained will be:

A B C

but the two outside letters will not seem to be appreciably darker than the middle *B* which is seen by both eyes. Hence, in this case the only part of the visual field on the left-hand side which has been noticed is that which is to the left of *B*, where *A* is; and the only part of the other field which occurs in the resultant impression is the portion where *C* lies. But the uniformly white ground in between the two *B*'s does not have any appreciable effect.

Suppose now that there are broad black and white figures on the visual globes of both eyes, and that the edges of these areas intersect each other in the common field of view; then the general rule is that the predominating field along an edge or in its vicinity will be that one of the two visual globes on which this edge lies. For example, consider the stereogram *V* on Plate VI. If the two black rectangles are superposed so that the white points at their centres coincide, a total effect will be produced about like that represented by Fig. 73; that is, the appearance will be that of a cross which is perfectly black

at thé centre, because there black is over black. The ground appears white, because on it white covers white. In each of the four arms of the cross the white of one field coincides with the black of the other field, but still they do not appear to be uniformly illuminated by a mixture of this black and white by any means. On the contrary, they are almost entirely black at their ends where they are adjacent to the white ground, and almost entirely white where they come in contact with the black central square; whereas in between there are transitions from black to white, although the mode of illumination here is not at all a stable state, and so it cannot be represented in a picture, because it changes very much. The extremity of each of the arms of the cross coincides with a portion of the white ground in the other field and suppresses it, the result being that it looks almost wholly black. But the edges of the black rectangle on the visual globe of one eye cross the rectangle in the other field near its centre; and here therefore the white in one field comes out plainly along the edge of the black in the other field.

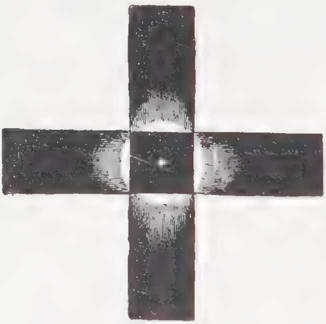

Fig. 73.

In the examples considered thus far, a figure with definite outlines has always been opposed to an absolutely empty uniform expanse; and invariably the outlines prevailed and suppressed the impression of the empty field. Now instead of having an entirely empty field, suppose it is covered all over by a uniform pattern of fine lines; as represented, for example, in stereogram W, Plate VI. If the eye on the left gazes at the black cross while the other eye is directed toward the network of lines, the effect usually produced at first is that the picture of the cross prevails, just as it would do if it were projected on an empty ground; and it will only be at its centre and beyond its edges that the pattern of fine lines may perhaps be visible. If we continue to look at the figure in this way for some time, without keeping the attention fixed in any definite direction, presently the pattern will perhaps begin to come out all over the field, covering the entire cross or some special portions of it at any rate. On the other hand, I must state that I find I am able at any moment to devote my exclusive attention to any part of the network of lines I choose, even to those parts which fall directly on the edges of the cross; and then the cross usually disappears altogether, and all I can see is the pattern of lines. I have simply to count the squares in a row or to compare them with

each other and notice whether they are all of one size or whether the lines are perpendicular, etc. As long as I devote my attention to this part of the figure, it stays in sight. On the contrary, the moment I let my attention be distracted to a corner of the cross or to one of its sides, the lines vanish more or less completely, and I see the cross steadily.

When the outlines in the two superposed figures are equally conspicuous, the conflict is still more decided. For instance, when the two pairs of parallel lines in Fig. 74 are superposed binocularly, most observers are apt at first not to see the vertical lines except at the place where they cross the others nor the horizontal lines in the interval between the pair of vertical lines or perhaps even beyond this interval. As we continue to gaze at the figure, the horizontal lines will emerge from time to time and the vertical lines disappear, and *vice versa*. However, in this case also, by fixing my attention on one pair of lines and examining them to see whether there are perhaps some irregularities in them, I can retain the image of one pair or the other according to my fancy.

Fig. 74.

The same conflict, only of a more complicated nature, is manifested between the two fields represented on stereogram *X*, Plate VI, in each of which there is a uniform system of parallel lines which differ simply in direction. If the lines superposed binocularly all crossed each other regularly, the effect would be similar to the network drawn on the right-hand side of stereogram *W*; but instead of that, we generally get the impression of an irregular blending of the two patterns, with one system of lines predominating at some places in the field, and the other system of lines at other places. These places themselves are subject to continual variations. The little black squares in the centres of the two figures are intended for fixation-marks, to enable the spectator to maintain the two superposed fields in a fixed relation to each other. Without having some conspicuous marks of this sort at special corresponding places, it would be practically impossible to do this, on account of the tendency of the lines of fixation to alternate incessantly between different degrees of convergence.

Sometimes for a brief interval one system of lines will appear all by itself, it may be over the entire field. Here also I am able to concentrate my attention on either of the two systems, whichever I choose, and to see it for a while exclusively, without seeing the other one at all. One way of doing it is by counting the lines in one system. I find too that this way of noticing one particular set of lines has nothing to do with any definite movements of the eyes; for I can let my eyes travel along the lines on which I am intent at the time and which I am seeing, or I can move them at right angles to the direction of these lines, that is, parallel to the other system, provided I pass from one line to the next without ceasing to see the lines which I want to see. Still, as Wundt states, it is indeed easier to retain the image of the system of lines which are in the same direction as the motion of the eyes. As a matter of fact, this is the ordinary mode of directing one's attention to a line. We deliberately let our eyes travel along the line so as to make sure of riveting our attention on it.

However, it certainly is difficult to concentrate the attention for some time on one of the systems of lines in stereogram X, Plate VI, without having at the same time some definite purpose in mind that tends to stimulate attention incessantly and to keep it active; such as counting the lines or comparing the intervals between them, etc. There are other circumstances also when it is hard to maintain a fixed state of attention continuously for some time. It is natural for the attention to be distracted from one thing to another. As soon as the interest in one object has been exhausted, and there is no longer anything new in it to be perceived, it is transferred to something else, even against our will. When we wish to rivet it on an object, we must constantly seek to find something novel about it, and this is especially true when other powerful impressions of the senses are tugging at it and trying to distract it. It seems to me that the explanation of the facts which have been described above is to be found in this peculiarity of our psychic activity.

The experiments which were last mentioned may be varied in many ways. For instance, if a sheet of printed paper is placed by the side of the pattern of squares on the right-hand side of Fig. *W*, Plate VI, and the two are superposed in the field of view, there is no difficulty either in reading the words or in seeing the systems of lines. The same thing is true when a finely executed map or a photograph is superposed on a printed page in the field of view, provided the patterns on one side are not too conspicuous in brightness as compared with those on the other side, and provided the two figures are not too much alike. For instance, if a person tries to combine two different pages both printed in the same type, involuntarily some letters on one page

will be connected in his two eyes with some letters on the other page, and so the letters may easily get mixed.

There are some other points which should be expressly mentioned. Even when the objects exposed to one of my eyes are very dim and faintly delineated, I can succeed in seeing and observing them continuously, although they may be superposed on conspicuous outlines in the field of the other eye. Thus, I can trace the fibres in a sheet of paper and detect the little spots in it, although conspicuous black figures may be drawn in the other field. Moreover, I can place a sheet of thin white paper over a page of print so that the latter is scarcely perceptible through it, and yet I can read it when either of the two figures in stereogram *W*, Plate VI (the system of lines or the black cross) is superposed on it. Or I can hold a mirror in front of one of my eyes and make the bright image of the window coincide binocularly with a printed page which is comparatively dimly illuminated, and still I can read the latter, without its being suppressed by the much brighter image of the window. I need not add that it is just as easy for me to notice the image of the window in the mirror without seeing the printed page. The reason why in an experiment of this kind a very dimly illuminated object in one field cannot be always discerned when the other eye is exposed to a brilliant illumination, is because the bright light in one eye operates to contract the pupils of both eyes, and the result is that the retinal image of the dark field is really very much darker than it would be if the other eye were not exposed to the bright field.

These experiments show that man possesses the faculty of perceiving the images in each eye separately, without being disturbed by those in the other eye, provided it is possible for him, by some of the methods above indicated, to concentrate his whole attention on the objects in this one field. This is an important fact, because it signifies, that *the content of each separate field comes to consciousness without being fused with that of the other field by means of organic mechanisms*; and that therefore, *the fusion of the two fields in one common image, when it does occur, is a psychic act.*

The distinction here can be made clear by simply comparing the binocular fusion of the two systems of oblique parallel lines in stereogram *X*, Plate VI, with the monocular union of these two systems as represented by the pattern shown in stereogram *W*. The lines in one system may be counted in this latter case also, and the intervals between them compared, but it never does happen then that the lines of the other system vanish from the picture, as is usually the case in binocular fusion under these conditions. When the compound system of lines in Fig. *W* is observed with one eye, we have simply a

sensory impression which cannot be altered by any concentration of the attention, even if we should happen to notice one set of lines more than the other. If the two corresponding diagrams on stereogram X really had been fused into a single, simple sensory impression, it would be impossible to resolve it into its component parts by any effort of the attention alone. Similarly, if a plate of transparent glass is employed to superpose the reflected image of the bright sky on a printed page, there are certain degrees of illumination for which it is impossible to read the writing in the monocular field of view; whereas it may be read binocularly without any difficulty, even when a silvered mirror is used to reflect a much brighter image of the sky on the printed page exposed to the other eye.

The rivalry between the two fields, as it occurs in the binocular fusion of the images above mentioned, is analogous to the careless, vacillating, uninterested state of the attention, accustomed to flit from one impression to another, until the various objects are gradually passed in review. That this variation does not depend on some organic mechanism of the nervous system, as has been conjectured by PANUM and E. HERING, at least on nothing more than underlies our mental activities, appears evident to me from this fact of introspection, namely, that, by purely psychic means of concentrating the attention, which are well understood and similar to those instanced above, the variation can be instantly stopped, without producing any noticeable change in the external conditions (such as changing the direction or movement of the eyes, etc.). PANUM is right when he states that it is not enough simply to intend to concentrate the attention on the image that is about to disappear or has already done so. Here he considers the attention as being an activity entirely subservient to the conscious will of the observer; but this is probably true only to a certain extent. We can move our eyes voluntarily also; and yet, although a person may intend to make his eyes converge, he cannot execute this intention so immediately without some special training. And at the same time perhaps at any moment he may be able to execute his intention of gazing at a near object, in which case his eyes will converge. A person may desire to hold his attention on some definite object, and although he may be conscious of intending to do so, the instant his interest in the object is exhausted, he will find it just as hard to execute his purpose. But he may propose some new questions in regard to that same object so as to kindle his interest in it once more, and then his attention will remain fixed on it. The situation therefore is like that in the case mentioned above; it is a mediate, not an immediate, volition. We can execute acts by our will in which the eye or the attention maintains the direction we desire, and yet we cannot deter-

mine the direction of the eye or of the attention by an act of the will directly aimed at it without any intervening agencies. On the other hand, I am compelled to differ with Panum again and to insist that the other characteristic property of the attention certainly does hold good also with respect to the rivalry between the visual globes of the two eyes, and that by proper methods it can be concentrated on the feeblest visual impressions, notwithstanding that the most powerful impressions in the other visual field tend to distract it. In this case, of course, the greater the disparity between the intensities of the two impressions, the harder it is to keep the attention on the weaker one.

Incidentally, as was clearly shown by the experiments with instantaneous illumination described in the preceding chapter, we are able to observe a certain number of objects simultaneously and to fill out some portion of the visual globe in this way. And hence it would be natural to suppose that the field of view will be filled at first with those objects which make the stronger impression, or that, when the stimuli in the two conflicting fields are of equal intensity, an alternation will occur in an effort to obtain some connected and intelligible impression, in which case it would not be necessary for the impression in one eye alone to be always predominant throughout the entire field of view. The continual wavering of the lines of fixation is characteristic of this effort to obtain an intelligible impression. It is well-nigh impossible to keep the two images continually superposed in the same position.

It is not exactly the same thing when the two different images can be regarded as being the visual tokens of an external object, because then the attention is turned immediately to the perception of the thing itself, without being diverted by the difference between the two retinal images.

The extraordinary influence exercised by contours in the rivalry between the two visual globes is also essentially a matter of psychological habit, in my opinion. For when we consider how the eye has to scrutinize the field of view in order to get a thorough knowledge of it, it would plainly be a waste of trouble to undertake to examine all the various points of a uniformly illuminated area of considerable extent one after the other, because after all no new information would be gained in this way. It is sufficient to let the gaze roam around the outline of the surface and to inspect the prominent places wherever they may be. When this has been accomplished, the information acquired will be as accurate as the eye can afford. And so in scrutinizing the field, the attention first, and then the eye itself, has to be

directed especially to those contours which were seen by indirect vision. It is well known how hard it is to discover a tiny object on an extensive bright surface unless it has first been noticed in indirect vision. Goethe speaks, for instance, of the lark "lost in the azure depths of space" (*"im blauen Raum verloren"*). On the other hand, the gaze is attracted immediately by a somewhat larger object, especially if it happens to be outlined distinctly enough to get a glimpse of it by indirect vision; and if a person observes how he considers an object before he knows what it is, he can easily notice how his eyes traverse the contours. Thus the result of both habit and practice must necessarily be to turn the attention to the contours. In the case of contrast phenomena also, I have pointed out what a particularly important factor the contours are.

It might be natural to suppose that the parts of the retina where white and black border on each other would be strongly stimulated whenever, as a result of movements of the eye, elements of the retina pass over from the black into the white. These elements, having had some relaxation, would undoubtedly be stimulated more strongly than those which had been already exposed to the white radiation. However, I do not believe that this circumstance is of any essential importance here, because it was shown in the experiments described above that the direction of the ocular movements had no decisive influence, and because the very first moment the contours in the double images fall on the eyes their effect is appreciable, before any after-images have had time to develop.

On the other hand, Panum's theory, that the contours themselves stimulate the retina more strongly, does not seem to me to rest on any secure basis of facts; and, besides, it appears to be entirely unnecessary for the explanation of the phenomena that are observed here. In the case of contrast phenomena the difference of illumination or colouration certainly does come out more strongly along a contour where two fields are juxtaposed than it does when the fields are separated. Indeed, the difference appears to be greater than would be expected under the circumstances. But without taking after-images into account, the phenomena of simultaneous contrast may be explained as being due to the fact that the illumination at two points on the retina can be compared much better and more certainly when they are adjacent to each other than when they are farther apart; because, as the eyes move, the adjacent points are liable to be exposed to the same illumination in rapid succession. The fact that such a difference appears relatively too large, and so leads to errors in our judgment of the colouring, is in accordance with the general rule by which we are apt to consider distinctly perceptible differences as being larger than

those which are vaguely distinguishable. Perhaps, a distinctly perceptible difference of this kind might be regarded as being a more powerful psychic stimulus, and possibly this may be a partial explanation of its greater tendency to hold the attention. But if after-images are left out, I cannot see any reason for assuming a more powerful nervous stimulus.[1]

When the two eyes are exposed to fields of different colour or luminosity, similar phenomena of conflict are manifested. If a person looks through a pair of highly coloured glasses, for instance, through a red glass with one eye and through a blue glass of about the same luminosity with the other eye, the external objects will seem to be spotted with red and blue, the two colours frequently alternating with each other. Generally this curious, restless fluctuation of colour is most lively at first, but presently, as the sensitivity for colours becomes dulled, the appearance ceases to be so fickle and assumes a grey colour, although it continues to waver here and there and from time to time between a more reddish and a more bluish hue. Some observers are disposed to regard the colour in this case as being a mixture, which would therefore be pink for this particular combination. I have tried over and over again in various ways to see the mixed colours, but I must confess I have never been able to do so with any certainty whatever. The peculiar characteristics of the objects have something to do with which one of the two colours is visible. Brighter objects will be more apt to look red, and darker ones blue. Possibly this may be because the red sensation generally predominates with higher luminosity, and the blue sensation with lower luminosity. Naturally, objects that are red of themselves will also look red, and those that are blue will look blue; because anything seen through a glass of the same colour as it is will look brighter than it does seen through a glass of another colour. Here, too, the matter of paying attention to one field or the other has much to do with the effect. It is very difficult to fix the attention simply on the colour of one of the fields, without being aided by contours belonging to that field; but various observers (for example, Funke,[2] J. Dingle, Voelckers,[3] Volkmann,[4] E. A. Weber,[5] Welcker,[6] and I myself) have been able to concentrate first on one eye and notice

[1] ¶See A. Chauveau, Rivalry between the visual fields in the stereoscope. *C. R.*, 152 (1911), 659-665. C. O. Roelofs, Über Wettstreit und Schwankungen im Sehfelde. Graefes *Arch.*, 104 (1921), 230-263. (J. P. C. S.)

[2] *Lehrbuch der Physiologie*. 1. Aufl. Bd. II, 875.

[3] Müllers *Archiv*. 1838, pp. 61, 63.

[4] *Neue Beiträge zur Physiol. des Gesichts*. pp. 97, 99.

[5] *Programma Colleg.* 118.

[6] *Über Irradiation*. 1852, p. 107.

what is seen by it, and then on the other eye in the same way. And when this is done, the colour of the particular glass which is in front of that eye will show up on the objects. FECHNER[1] found it harder to produce this variation by his own voluntary effort; and he concluded that it was due to an involuntary movement or compression of the eye, which, according to his observations, was simply conducive to the change of the colour, without, however, aiding it to take place in the desired direction. The experiment succeeds very much better still when the coloured glasses are placed so as to reflect into the eye images of dimly illuminated objects lying off to one side. Then, as the attention is turned to one of these reflected images, although it may be no more than a faintly visible shadow effect, immediately the colour of that particular mirror will appear on the visual globe at the proper place. And if an image reflected in the other glass happens to be visible in the field at the same time and at the same place, and the attention is concentrated on it, the other colour will come out also.

I devised a systematic method of making this experiment by mounting a piece of blue glass B and one of red glass R vertically on a table, as represented in Fig. 75. A sheet of printed paper turned toward B is placed against a dark screen C. On the inner side of a similar screen D some other pattern is attached; for example, a table of numerals, which cannot be easily confused with the letters on the other screen. A white screen is placed at A; and the observer's eyes are shown at O and O'. The illumination is regulated so that the letters and numerals as seen in the images reflected in the coloured mirrors will just be visible when the screen A is highly illuminated. The reflected images of the letters and numerals will appear to the observer to lie on A. Now when I try to make out the letters, the ground invariably appears to be blue. On the other hand, when I try to read the figures, it appears red. And so by concentrating my attention on the image on one retina, I make the corresponding coloured ground appear. It may be observed that the contours, which in this case make the single impression prevail, are borders between black and white, and yet, as the colour of the ground becomes visible at these places, its intensity is not affected one way or the other. Or, when the whole mixed illumination is taken

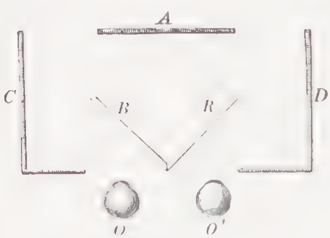

Fig. 75.

[1] *Abhandl. der Sächs. Ges. d. Wiss.* VII (1860), 399–408.

together, it will be found that the letters on the left appear pure blue on pale blue, and the numerals on the right pure red on pale red. In the case of contrast phenomena, the attention would be directed simply to the opposition between black and white, and not to the blue or red; whereas it is just the opposite in the binocular experiments which have been described here.

A simpler form of the experiment, which I find works very nicely, consists in gazing up at the sky with a red glass in front of one eye and a blue glass in front of the other eye; only, they must be inclined to the axes of the eyes, as in Fig. 75, so that faint traces can be seen of the images reflected in the coloured mirrors by objects which are off to one side. Now move one glass just a little, then the other, and the reflected images will move likewise. These moving images may be exceedingly dim and indistinct; yet, by watching one of them closely, all at once the colour of that particular mirror will be seen to stand out on the sky. It is really a marvellous sight when suddenly, as by word of command, the blue sky becomes red all over, or the red sky blue all over.

There is absolute divergence of opinion between various observers as to whether or not the compound colour is visible when two fields of different colours are superposed binocularly. On the one hand, there are H. Meyer, Volkmann, Meissner and Funke, who have never been able to see the compound colour; and I must include myself also among them. On the other hand, Dove, Regnault, Brücke, Ludwig, Panum, and Hering are equally positive that they have seen it, not only when the colours were pale and dim, but when they were saturated. Dove stated that he saw it when the colours were those of the prismatic spectrum, which are the most saturated of all colours. A real spectrum was projected on a screen and viewed binocularly through two telescopes at once, the image being inverted in one instrument and erect in the other. According to him, polarisation colours are especially suited for observations of this kind. The method which he used for this purpose consisted in adjusting thin sheets of mica or gypsum in the proper position in front of a plate of black glass, which reflected the light at the angle of polarisation. A Nicol prism was then held in front of one eye and rotated in such a position as to transmit the maximum amount of the polarised light reflected from the glass plate. Another Nicol prism was placed in front of the other eye, but it was rotated through an angle of 90° as compared with the other prism, so that none of the reflected light would traverse it. Under these circumstances, the sheets of crystal will be seen in colours by both eyes, but the colours in one eye will be exactly complementary to those

in the other eye. Now in an experiment of this nature, both Dove and Regnault report that they have actually seen these complementary colours fused binocularly into white. On the other hard, I have performed this experiment over and over again, and each time it has consistently and absolutely failed. It makes no difference whether I use spectral colours or polarisation colours, invariably there is the same conflict and alternation between the various simple colours, but I am not able to see the compound colour, such as would be produced if I had used pigments or the colours of tinted glasses. Incidentally, I found that a quartz plate cut perpendicular to the axis made a great improvement in these experiments. When the Nicol prisms were rotated in front of the eyes, new colours would make their appearance. But I always see both colours separately, one through the other, as it were; and I can always tell instantly, without having to shut one eye, what colours are there. In this case the bright white background of the reflecting plate, which shows the mixed colour that is said to make its appearance, is there for comparison with the colours; and this is one reason why it is easy to notice the great difference in these experiments between the binocular union of different colours and their real union.

It is a hazardous thing, I know, to contradict so many eminent and reliable observers on a matter about which individual differences are probably extremely great; and yet I do venture to mention here some circumstances which in my own experiments, it is true, did sometimes give the appearance of a mixed colour, although on more careful investigation it was found that there was really nothing of the sort, at least so far as my eye was concerned.

However, the following preliminary statement must be made first. Suppose a person is gazing at the binocular combination of two colours, and at the same time can see also each component separately. For instance, suppose the axes of his eyes are parallel and he is looking at a blue field which adjoins a red field over to one side, so that there is a double image of the line of separation between the two, blue being superposed on blue on one side, and red on red on the other side, whereas in the middle red and blue are superposed on each other. Then the blue in the middle can undoubtedly be distinguished from the pure blue on one side by its having more or less red added to it in the field of view; and anyone knowing the rules for mixing colours and being accustomed to see violet or purple compounded from blue and red, might perhaps take for violet this blue compounded with red. Even in the monocular field, owing to the contrast between a real violet and some adjacent blue, or because the blue seems to be a covering spread over the field or to belong to the total illumination, it may be

that the observer will resolve the actually existing violet into blue and red. Examples of this kind were given in §24 in the preceding volume. Thus there are some circumstances in which red and blue, that are actually combined monocularly into violet, yet may appear separated, just as binocularly superposed red and blue always do appear in my own case. And so such an observer might possibly be betrayed into thinking that, when he sees red and blue simultaneously, he really does see violet or purple. But if the real compound colour made by the two observed colours is exhibited, the difference between it and the other will be very striking. The best and most accurate method of producing this compound colour is as follows. Arrange a pair of blue squares and a pair of red ones side by side like the squares on a chess board; for instance, suppose the upper corner on the right and the lower one on the left are red, and the other two blue. Now take two double refracting, achromatic prisms of Iceland spar, and place one in front of one eye and the other in front of the other eye. Adjust them until the two images in each prism are one above the other. The double images of the coloured fields will partially overlap; and so along the middle horizontal line between the two upper squares and the two lower ones, there will be a central strip in the field of view of each eye consisting of a monocular mixture of red and blue, which will appear therefore to be pink. Now make the axes of the two eyes parallel, and gaze at the two fields so that their images will be binocularly superposed. In this case there will be binocular overlapping between the upper blue on the right and the upper red on the left, the middle pink on one side and the middle pink on the other side, and the lower red on the right and the lower blue on the left. Now when I perform this experiment, I am positive that not the slightest trace of pink, as it appears in the central strip, can be detected in the binocular combination of blue and red, and that nothing can be seen there except each of the two colours separately.

Panum insists that it is very important that the two colours that are to be binocularly mixed should not be too vivid or too unlike; otherwise, the rivalry between the two fields will be too intense and too variable, and so the observer will be hindered from recognizing the compound colour. Accordingly, I have employed H. Meyer's method, which was described previously in connection with contrast phenomena. The coloured fields which were to be combined were first covered over with a sheet of fine white paper so that the colours underneath could only be dimly discerned. At first, when I superposed these very pale colours, I really thought I did see the mixed colour. But when the real mixed colour was placed alongside the two fields, I was again aware of the conflict that existed between the two binocularly superposed fields.

If we have a varied assortment of papers of all colours, including the greys, it is sometimes possible to find two colours which when mixed by a double refracting prism will give a resultant colour precisely like that of one of the other samples. Under these circumstances the experiments are easier to make and even more impressive. I placed a sheet of glazed green paper by the side of a sheet of pink paper, so that the line where the two colours came in contact was vertical; and then, horizontally across them, I laid a strip of grey paper, whose colour was like that obtained by mixing the other two colours. The whole was covered finally by a sheet of fine white paper. When I examined these fields through a double refracting prism, in which the two images were shifted apart horizontally, grey was mixed with grey along the horizontal strip, but in the centre, both above and below, pink and green were mixed, producing a grey also, which blended imperceptibly into the horizontal band of grey. But, when the prism was removed and binocular double images were produced, the band where grey was over grey was very distinctly different from the places where pink and green were superposed. And yet when I withdrew the grey strip in the middle, I could not continue to detect the binocular conflict between the two coloured fields; then all I noticed was what was common to both colours, namely, the white.

In other cases after-images are responsible for the production of what appears to be a mixture. The arrangement used above is well adapted for this purpose, that is, a band of grey paper above, with pink and green below, the former on the left and the latter on the right, these colours being such that when mixed by a double refracting prism they give the effect of the upper grey. When the two lower fields are superposed binocularly, all I can see at first is a lively conflict between them. However, after continuing to look at them for some time, presently the binocularly mixed field gets to appear like the grey above, differing from it just a little, sometimes on the red side and sometimes on the green side. But then if the red is covered with green and I close one eye, the after-image of the green seems to me to be on green, whereas in that part of the field where pink was before, now the pure saturated green is visible. Now it is very obvious here that the green modified by fatigue has really become very much like the grey in the upper band. The same thing happens in the pink when the green is covered. Thus the apparent mixing of the colours into white in this case is because, owing to the appearance of complementary after-images, the colours themselves, so far as sensation is concerned, have become much more like the grey, until at last the colours are so similar that the difference between them and their rivalry with each other will no longer be noticed as at first when the difference was more vivid.

There are some cases in which the induction of the colour of the background, mentioned in Vol. II, §24, may produce an apparent effect of binocular mixing over a small field of another colour. For instance, I placed a horizontal band of blue on a red background, and, keeping the fixation steady for a long time by fusing a little black dot on the blue with a similar dot on the red, I gazed at the binocular double images. All I observed at first was the conflict between red and blue in that part of the field where these colours overlapped; but, finally, I noticed that real violet occurred. However, when I shut one eye, I could discern with the other eye alone the induced red on the blue band.

Lastly, there is a case, mentioned by H. Meyer and Panum,[1] in which I find the most striking appearance of all of an effect similar to monocular mixing. On the right there is a yellow field with a horizontal pink band on it, and on the left a blue field with a vertical band of the same pink colour. If the yellow and blue are superposed binocularly, so that the two pink strips appear to form a cross, the arm of the cross on the left, which falls mainly in the yellow field, will undoubtedly look much yellower than the opposite arm, which falls mainly in the blue field. Where the two fields overlap in the centre, pure pink will be visible, or rather it looks to me here as if the yellowish pink of one band passed, so to speak, underneath the bluish pink of the other band, without being blended with it. Panum thinks that the yellowish and bluish tinges of the pink are due to its being binocularly mixed with the colour of the opposite field in each case. A point to be noted is that the variation in the two pink bands is most in evidence when the gaze is allowed to wander; because then the band lying on the yellow ground gets the blue after-image of the yellow, and the band lying on the blue ground gets the yellow after-image of the blue. However, even when the eyes are steadily fixed, the effect is undoubtedly produced, although not to the same extent. Yet it can be shown that here also the phenomenon is primarily one of contrast. Thus, even when one eye is closed so as to preclude anything like binocular mixing, the change in the colouring of the pink still persists. The pink band continues to be as yellowish as it was before when the eye opposite the yellow field is closed. At this instant, it is true, the yellow which pervades the pink like a kind of yellow mist, does disappear, but the apparent colouration of the pink itself still persists without the slightest alteration. Similarly, if the other eye opposite the blue is closed, the pink band on the yellow appears the same bluish

[1] *Physiologische Untersuchungen über das Sehen mit zwei Augen.* Kiel 1858. p. 41. Figs. 27 and 29.

red. It follows, therefore, that the change in the pink cannot be due to binocular mixing, or at least that it cannot be due to that alone, but is a constant effect. From the very beginning, even in monocular vision, the pink on the blue field appears more yellowish by contrast, while that on the yellow field appears more bluish. Undoubtedly, the contrast effect is much more vivid at the instant when the two fields are binocularly superposed; but once it has been brought out in this vivid way, it does not disappear again, even when one eye is shut, and binocular coincidence is abolished. We were at much pains to explain in Vol. II, §24, that in any case of contrast judgment of colour was unreliable within a certain interval. Owing to secondary considerations, we are apt to consider the observed colour as lying more on one side of this interval than on the other. It is possible in the present instance that the binocular overspreading of the complementary colour on the ground where the pink band is may be a contributory cause of this nature. I may add that I shall have occasion presently to speak of the theory of binocular contrast again.[1]

As to the theory of the binocular combination of colours, the only difference between it and the monocular mixing of colours, on Young's theory, is that in the former case the nerve fibres for the three fundamental colours are distributed over both retinas, whereas in the latter case they are distributed only over one retina. Either the three different kinds of fibres at a given point on one retina have the same local sign, or else, on the supposition that the local signs are different, there is no possible experience that could enable these fibres to be stimulated by objects which were in different parts of the field. Hence, there cannot be any reason for separating the localization of these sensations with reference to the directions in the visual field of the eye in question. Accordingly, the various sensations of these fibres are blended into a resultant sensation, that is, the sensation of a mixed colour; and usually this will be the visual token of some definite property of the locally simple object which happens to be at that place on the visual globe of the eye. And yet, as we have seen, even when colours are mixed monocularly, there are certain cases when we imagine we see one of the combined colours through the other. It may be due to the irregular distribution of the light, or to the movement of some image that is limited locally, or to the presence of some portion of the colour all over the field of view, but it happens whenever we are induced to separate a coloured illumination or mantle from some coloured object.

[1] Concerning the subject of binocular colour mixing, see Note 1 at the conclusion of this chapter.—K.

When corresponding portions of the two retinas are illuminated differently, the impression that is produced is one that can never be obtained by uniformly illuminating a simple object on all sides. And yet, perhaps as a result of training and not from any innate mechanism of the nervous system, both colours will be attributed to the same region in the common field of view; and so two colours will be seen in the same field, each being perceived as separate from the other. This visual picture is certainly very much like those cases of monocular mixing in which two coloured objects are seen, or appear to be seen, one behind the other at the same place on the visual globe. Many observers, including myself, never see this effect any other way. The attention may waver, being diverted first to one field and then to the other, making us aware of a conflict. Incidentally, something like this conflict, only much less pronounced, may be also noticed in the monocular field by using an unsilvered plate of glass to reflect the image of an object at the same place where another object as seen through the glass happens to be. The two images in this case should both be equally bright and well defined, but entirely different in pattern. Then we may look at either of the two, and the other one will retire more or less out of sight, although it may never disappear completely, as it does when the images are binocularly superposed. If necessary, the two images can easily be separated by moving the mirror slightly.

On Young's theory, the apperception of mixed colours is invariably the result of projecting three different sensations of colour at the same place on the visual globe. Even when colours are monocularly mixed, this apperception will depend on a mental act, which will vary according to circumstances; that is, it will depend on the decision we make as to whether these circumstances are to be considered as a visual token of some simple quality of one object or of two different qualities of two objects. While this is the case, yet, on the other hand, it might conceivably be possible that the difference between the impression produced by combining two colours binocularly and that produced by combining them monocularly may be disregarded, and the two colours considered as being united in the former case in the same way as they are in the latter case. According to Young's theory, the mixed colour is really nothing more than the integration of three different kinds of impression, which have otherwise no mutual action on each other, but which all have the same localization. Naturally, therefore, the mental decisions on which their union or separation depends may be very different for different observers, according to each individual's particular training and variety of experience. Such being the case, it goes without saying that the union of colours which are very much

alike, and which have therefore much in common and not much that is different, will be easier to effect than the union of extremely dissimilar colours. Besides, there may often be minute differences between the impressions on the two eyes produced by the same real object. For instance, one eye may be more fatigued than the other, or the light may be very brilliant or coloured, perhaps entering one eye from the side and being diffused in it, etc. And so we may get in the habit of equalizing some of these minor differences unconsciously. Indeed, if a field producing such an impression is placed close by the side of another one in which two like colours are superposed, the conflict between the two impressions will be noticed, even when they are not very dissimilar.

Lastly, the binocular combination of two fields which are different as to colour or illumination is exhibited in the case of stereoscopic drawings in an extremely remarkable and characteristic way. Thus, for example, in a stereogram intended to represent some object, suppose that a certain area in one of the pictures is shown in white and in the other picture in black, or suppose that this particular place is coloured differently in the two pictures (although it is better for the colours not to be too much unlike); then when the two views are combined stereoscopically, this area will shine with a certain *lustre*, while all the other parts of the body where the illumination and colouring in the two pictures are the same will appear *dull* by comparison. Incidentally, this appearance of lustre or dullness has absolutely nothing to do with whether the surface of the pictures themselves is really dull or lustrous, provided that in the latter case they do not send any reflected light to the observer's eye.

For example, the outlines of the model of a crystal may be represented by two drawings on a stereogram, in one of which the lines are white on black and in the other black on white; and when they are combined in a stereoscope, the impression will be produced of looking at an object consisting of some dark shining substance like graphite lying on a graphite surface. A stereogram of this nature is given in Fig. Q, Plate IV.

Similarly, also, places can often be found on photographic stereograms representing brilliant objects (such as the bright foliage of plants, satin, etc.) where the reflections of light were unequally bright in the two views, so that the effect of lustre is produced when they are fused. One of the most remarkable examples of this kind is afforded by instantaneous photographs of ripples on a surface of water illuminated by direct sunlight. On looking at a real object which glitters in this way, it can often be noticed that more light is reflected by certain spots in one eye than in the other.

I am inclined to think that this is likewise the explanation of the appearance of lustre at these places in a stereoscopic view where the illumination in the two pictures is different. Light falling on a dull surface is radiated uniformly in all directions, so that the surface looks just as bright from one place as from another. And so, under normal conditions of vision, it always appears just as bright in one eye as in the other. But the reflection of light from a lustrous surface is more or less regular. Such a surface may exhibit numerous tiny rugosities of various dimensions; but when it is smoothed and polished so as to have an approximately definite direction on the whole, the incident light will be reflected from it mainly in the same direction as all the light would be reflected by a mirror. Now under such conditions, it is quite possible for one eye to be in the direction where the light is reflected, while the other eye is not; the result being that the surface will look very bright to one eye and very dim to the other eye. And so in looking through a stereoscope, if the appearance of the image of a part of a body is very different in one eye from its appearance in the other eye, the resultant visual impression will be just the same as would be actually produced by a lustrous surface, but never by a dull surface. Consequently, this place in the stereoscopic view appears to be lustrous.

Similarly, also, when a body that sheds lustre is surrounded by coloured objects, it may reflect light of one colour to one eye and light of another colour to the other eye, and thus be seen in different colours by the two eyes; whereas, under normal conditions of vision, the colour of a dull body will always appear necessarily the same for both eyes. And so in a stereoscopic view, if the colour of the same surface is different in the two pictures, the result will be a visual impression such as only a lustrous object can produce. As a rule, the colour of the lustrous body itself will be mingled with that of the light of the two reflections, and the latter seldom contain just the one pure colour only. And so the differences of colouration in these reflections from lustrous bodies for the two eyes are not apt to be very great; and that is why the effect of lustre can be produced better by combining colours which are not very much unlike than by combining very brilliant colours which are very far apart. The latter will exhibit conflict rather than lustre.

According to Wundt's experiments, the best way of combining two coloured fields so as to get the effect of lustre is when there is about the same contrast between each colour and the background. If, however, the contrast is much greater for one colour than it is for the other, this effect will be weaker, because then the former colour will prevail in the conflict between the two fields and subdue the latter. For instance, if two coloured squares of the same size, one bright yellow and the other dark blue, are laid together on a white or black back-

ground, and then binocularly superposed, the contrast between yellow and white in one case, or between blue and black in the other case, will not be great enough; and so the lustre will be much weaker than it would be if the two coloured squares were laid on a grey ground, where the contrast was the same for both.

Moreover, the effect of drawing some pattern on one of the coloured squares would be to give it an advantage in the conflict, and so to impair the effect of lustre.

Without using stereoscopic pictures at all, binocular lustre may also be produced by simply looking at variegated objects through glasses of two different colours. The object, for instance, might be some pattern executed in blue and red, which was viewed with one eye through a blue glass and with the other eye through a red glass. As seen through the blue glass, the blue portions will appear bright and the red portions dark; whereas with the other glass it will be just the reverse. And so the pattern viewed in this way will show lustre to a high degree. A remark which Dove makes in this connection is worth noting. He states that when one colour or the other happens to prevail entirely in the conflict between the two eyes, the lustre disappears; but at the moment of transition, when both colours are seen side by side, the lustre shows.

A characteristic thing about metallic lustre is that frequently the regularly reflected light itself is coloured already, and not white like the light of transparent substances. Thus bodies that exhibit the iridescent colours of thin films, such as the brilliant plumage of birds and certain highly coloured refrangible substances like indigo, are apt to show metallic lustre.

The phenomenon of stereoscopic lustre is particularly important in connection with the theory of the activity of the retinas of the two eyes. The statements of various observers as to the result of the binocular fusion of unlike images are so different, that, were it not for this phenomenon, doubtless, we never should have known positively that the visual impression produced by the action of two different kinds of light on corresponding places on the two retinas was absolutely different from that produced by the action of two homogeneous kinds of light on the same retinal places. If one eye sees black, and the other eye sees white on the corresponding part of the visual globe, the impression will be that of a surface shedding a pale lustre. But if the white light, which fell previously on one side only, is distributed uniformly over both sides, that is, if grey is combined with grey, the impression will be that of a dull grey, absolutely different from the lustrous white effect in the first instance. The same thing is true with respect to the lustre produced by binocular union of different colours.

The same conclusion, indeed, may be inferred from the fact that the impression obtained by the binocular fusion of two stereoscopic pictures is that of a body, and not as if all the lines were on the same sheet of paper. But undoubtedly in this case the movements of the eyes have an important influence, which we never do get rid of entirely except in the case of instantaneous illumination by the electric spark.

I might add that I have taken stereograms that show stereoscopic lustre and viewed them by the illumination of the electric spark, and that then also the impression of lustre is perfectly produced. This is an important fact, because it shows that the lustre does not depend on the alternation of colouring and illumination that is responsible for rivalry. My experience is that, when the attention is relaxed, there are never more than about eight alternations per second in the conflict, and generally the frequency is much less than this. On the supposition that the luminous impression on the retina lasts a small fraction of a second, no appreciable change can occur during this time as the result of rivalry between the two fields. And yet in this brief interval we can notice that the two different impressions on the two visual globes are seen at the same time and in the same place in the common field of view.

Incidentally, the impression of lustre may be produced by images and objects even in monocular vision. This happens, for example, when the illumination changes rapidly in consequence of movements of the observer. Then the elements that constitute stereoscopic lustre are not observed simultaneously, but in quick succession. So also, objects in motion may give the appearance of lustre, provided the illumination at particular places on them varies in quick succession. This is the explanation of the glitter of ripples on the surface of water. Even when the variation of the illumination of the parts of the surface is simply an imitation of the way light is scattered by diffused reflection, it is sufficient to give the effect of lustre. WUNDT produced monocular lustre by looking through a plate of glass at a dark square on a dark ground, where an image of a brighter square on a bright ground was very nearly superposed by reflection in the first surface of the glass. If the reflected image was apparently exactly at the place where the dark square was, the lustre disappeared, and then only the mixed colour was visible. But when the reflected image appeared to be behind the other one, the lustre showed. If the reflected image was in front of the other, it seemed to shine better. The idea obtained in this case was as if one were beholding another square which was beyond the first one, and were seeing it through the latter as if it were a reflected image of it. It was this that gave the appearance of lustre. These experiments indicate very clearly that the special qualities of the colouring do not matter so much, and that the im-

portant thing is to produce the illusion that another image is reflected in the observed surface.

Sometimes the appearance of transparency is produced also by the binocular superposition of two fields of unlike colour. Wundt called attention to this effect. For instance, if a bright yellow square and a dark blue one, both lying on a white ground, are binocularly superposed, but not exactly coincident, the blue appears to be transparent where it is superposed on the border between yellow and white. But where the yellow is superposed on the border between blue and white, this transparent effect will be lacking. On the other hand, when the ground is black, it is the yellow, and not the blue, that looks transparent. The general rule seems to be that the field which looks transparent is the one for which the contrast with the ground is the greater. This is in accordance with the objective law, that anything seen through a translucent medium, which is itself distinctly perceptible, is always seen indistinctly; whereas the border of this medium, not being concealed by some other translucent substance, will usually be well defined.

Lastly, some phenomena have yet to be discussed, which should be, or at any rate may be, interpreted as being *contrast* between the sensations in the two eyes.

Let us mention first a matter which Fechner noticed especially; and that is the extraordinary acuteness of perception of minute differences in the instantaneous *colour-tuning* (*Farbenstimmung*) of the two eyes or mode in which the eyes react to colours, when the binocular image of a tiny luminous object seen against a black ground is resolved into separate double images by changing the adjustment of the eyes. Suppose, for example, that one eye has been closed for some time while the other eye was exposed to luminous white surfaces; immediately after opening the closed eye, there will be two double images of a white band on a black ground, and the one belonging to the fatigued eye will appear darker and at the same time more violet than the other one belonging to the dark-adapted eye. But if the surface exposed to the open eye had been coloured, the colour of the image in this eye afterwards would be complementary to that of the inducing field, but the colour of the image in the other eye would be like that of the inducing field. While the two images in this case are being compared, the complementary colour in the fatigued eye will continue visible very much longer than it would do if both eyes had been "colour-tuned" alike by having both been exposed to the same colour in the same way. For example, without the help of double images in this fashion, it is extremely hard to perceive that there is a bluish tinge in

the after-image of a white surface of moderate brightness; and yet this bluish tinge will be evident at once, as soon as it can be compared with the apparently bright orange-yellow image seen by the eye that has been resting. When there is too much difference between the brightness of the two images, the comparison may be greatly facilitated by proportionately reducing the brightness of the image in the exposed eye, either by looking at it through a tiny hole in a piece of black paper, or by viewing it through a double refracting prism which will resolve the image of the bright band into two, each half as bright as the original one. Or the image may be viewed through a grey glass, provided we are certain beforehand that the glass itself is absolutely colourless.

These experiments prove that a very accurate comparison can be made between the sensations of colour at approximately corresponding places on the two retinas. Apparently, indeed, the comparison can be made more accurately in this way and for a longer time than when the colours have to be compared at the same place on the retina of one eye alone. Thus, suppose it is desired to compare the colour which corresponds to the sensation of the retina for white, say, with the colour which it seems to resemble in the eye which has not been fatigued; then it will be necessary to develop a good after-image by gazing steadily at a white object on a black ground, which must then be projected on a uniform white ground. In this method the necessity of keeping the fixation steady involves considerable strain, and that may have some effect on the course of the process. And, besides, there is the further disadvantage of not being able to reduce the intensity of the bright image as desirable. But, worst of all is the fact that the limited after-images on one retina quickly disappear and therefore cannot be perceived except for a brief space; because it is hard anyway to notice constant differences of luminosity or colour between two different places on the retina which have not been revived by change.

We saw in Vol. II, §24, that the tendency always is to regard differences of luminosity or colour which can be perceived distinctly as larger than those which are just vaguely perceptible, and that, in fact, most contrast phenomena were due to this peculiarity. In this particular case, the fact that the unchanged image always assumes the opposite phase of colour and brightness from that of the changed image, is a manifestation of a contrast effect of this kind. Thus the pure white in the unfatigued eye is made to look yellow by the side of the violet-grey in the eye which has been exposed to white; or if the latter is coloured pink by the after-image of green, the former will be made to look green, etc.

Instead of one of the double-images being coloured by an after-image, it may also be coloured directly by viewing it through a piece

of coloured glass. But here, too, as is characteristic of contrast phenomena, we find that a faint colour is apt to produce a much more marked contrast effect than one that is highly saturated. A piece of greenish window glass or yellowish bottle glass will enable us to see the complementary colour on the image in that eye much more distinctly than it can be seen by looking through a piece of highly coloured glass, although in the latter case the image in the other eye can be reduced to the same luminosity as that of the coloured image by viewing it through a suitable piece of grey glass.

As a matter of fact it is possible to have a contrast between colours lying on corresponding places on the two retinas. Place a strip of black on a white ground, and after separating it in double images, insert a blue glass in front of one eye and a grey one in front of the other eye, the two glasses being about equally dark. Then one of the images of the black band will appear to be surrounded by prominent blue, and the other image by prominent white, while over the rest of the ground blue and white will be superposed more or less uniformly. In this case the white that comes out around the edge of the black strip will be decidedly yellowish. On removing the two pieces of glass, yellowish white will be found to appear where blue prevailed before, and bluish white where it was previously yellowish.

The effect of substituting a yellow glass for the blue glass in this experiment will be to interchange yellow and blue in the two images wherever they occur.

No doubt it must seem very curious that the effect of the border around the black band is to attract the attention to the adjacent white and separate it so completely from the overlying blue in the common field of view that this white does, in fact, look yellowish. This yellowish white, by the way, exhibits also the characteristic of a contrast colour by persisting for a brief time even after the eye behind the blue glass has been shut tight. It may be recalled, in connection with the phenomena of coloured shadows (Vol. II, §24), that, when once the judgment has decided about the nature of the colour, this impression persisted even after the contrasting colour, whose presence had been responsible for the mistake, was removed from the field of view.

In the preceding experiments the contrast was developed by comparing two colours in the rival visual fields. But the effect of monocular contrast may be enhanced also by binocular comparision with the complementary contrast. Place a piece of pink paper by the side of a piece of green paper so that they touch in the middle; and on each of them lay a strip of white paper near the border between them. Then gaze at the two strips with both eyes; as a rule, no contrast colour-

ation will be noticed on either of them, unless after-images have already been developed of the two colours. Now if one eye is closed, while the other looks at one of the white strips through a black tube, a faint complementary colouration will, indeed, be observed. But if a black tube is held in front of each eye, so that the right eye sees one of the white strips and part of the pink ground, and the left eye sees the other white strip and part of the green ground, but without the two strips being binocularly superposed, the complementary colourations on the white bands will come out to an extent which can scarcely be seen by any other method. If, without keeping the gaze riveted on any one place, the experiment should be continued for some time, the contrast effect goes on increasing; and then, of course, the after-image of the ground will be more and more intense. But the right eye sees only red ground and the left eye sees only green ground, and hence, no matter how the eyes move, the background can develop nothing but green in the right eye and red in the left eye; and so the effect of contrast must needs be heightened.

The above would be a *successive contrast* depending on after-images. If at the beginning of the experiment the eyes were focused as soon as possible where the white strips should be, the contrast colours will be seen too, but they will be much less intense. However, in the way this experiment was performed, it was particularly easy to see after-images of the ground by comparing the colouring in the two visual fields; and so I thought it necessary to devise some method by which it could be certain that there was no after-image of the ground. Accordingly, I attached two vertical parallel strips of paper to a plate of glass, the one on the right being black above and grey below, and the one on the left grey above and black below. The plate of glass itself was placed over a flat surface which was covered with red paper on the right and with green paper on the left. Thus the strip of paper on the right was on a red ground and the strip of paper on the left on a green ground. However, before beginning the experiment, a sheet of white paper was inserted between the glass plate and the coloured ground so as to cover the latter entirely. Then I gazed at the two grey-black strips and binocularly superposed them on each other so that the upper and lower halves of the resultant image consisted of the black half of one strip and the white half of the other strip mutually overlapping. A white mark was made in the centre of each strip which served for a fixation-mark, so that when these two marks were fused binocularly I could be absolutely sure of keeping the common image of the grey-black strips. After these preliminary arrangements had been made, the sheet of white paper was withdrawn so as to leave the coloured areas behind it exposed; and then I could undoubtedly

detect some traces of contrast colouring, but they were exceedingly faint. The grey on green had a reddish look, and the grey on red was rather greenish. However, all that was necessary to make the colours both come out in full intensity was simply to move the eyes a little from right to left and back again. The faint contrast colourings which were detected at first were fainter than they would be in monocular contrast; and when white was substituted for grey, they were fainter still.

Accordingly, the pure effects of simultaneous contrast on the two grey strips were diminished by binocular comparison. By bringing the grey in the visual field of one eye close to that in the visual field of the other eye, binocularly, the two greys could be compared more accurately than was possible before in the monocular field, where the two strips were separated from each other by wide intervals of green and red. Therefore the phenomena of successive contrast depending on variation of the sensation by after-images are entirely different in behaviour in this respect from those of simultaneous contrast, which were regarded as mistakes of judgment. In binocular comparison the former show up better still, whereas in the case of the latter the effect of this comparison is to correct the errors of judgment.

In the form of experiment which was described above, the grey strips were not allowed to be binocularly superposed on the coloured ground, but were, so to speak, fused with the black. However, by a change of convergence of the eyes, the images of these strips can be shifted so far apart that they merely touch without overlapping. Adjust them in this way, with the sheet of white paper interposed at first, and notice how the grey looks alike on the two strips. Then withdraw the sheet of white paper and expose the coloured ground which was behind it. The strip surrounded by red, which is binocularly superposed on green, will appear decidedly green; the other one surrounded by green, and superposed on red, will appear decidedly red. The impression of a binocular mixture of the grey with each of the two colours is really quite startling. Now if the sheet of white paper is again interposed between the glass plate and the coloured background, the colourations instantly disappear, as they would necessarily do if the colours of the ground had been mixed with the grey.

But another experiment proves that what we obtain here is not a mixture. When the strips are seen in their complementary colours, suppose I close my right eye, so that only the strip surrounded by green remains visible. Then, although a kind of red veil seems to extend over it, due to having the red binocularly superposed on it, its own natural colour is left, that is, grey, and yet as reddish as it was before; which would not be possible if the reddishness of the grey were simply the effect of its being (binocularly) mixed with red; for

as soon as the red disappeared from the mixture, the original colour would be obliged to assert itself, more likely becoming greenish by contrast. I myself am disposed rather to think that the explanation of these experiments is as follows. We know already that when grey is binocularly superposed on black in the visual field of each eye, the hues of the two greys can be compared with much precision, and that the result of this direct comparison will be to diminish effects of monocular contrast that might have a tendency to make us suppose that the two greys were different. On the other hand, in the experiment just described, grey, which was surrounded by red and which it would be natural therefore to regard as greenish, was binocularly superposed on green; while the other grey, coloured reddish by contrast with the surrounding green, was binocularly superposed on red. The mere fact that the two greys were binocularly superposed on two different vivid colours may make any comparison between them very unreliable, and therefore heighten the contrast.

If a white surface is afterwards interposed, enabling the eyes to revise their judgment as to the white, the contrast vanishes immediately. The contrast between the two grey areas will also be made to disappear at once by interposing a black surface, and then they may be accurately compared without danger of mistake. On the other hand, when one of the eyes is closed, there is nothing left by which the judgment might be corrected, and so the contrast persists.

The results of these experiments thus far may be summarized as follows:

Let α and β denote the two images side by side near each other in the binocular field, as seen by eye A and eye B, respectively; these images being superposed on the backgrounds a and b, respectively. Then a very accurate comparison may be made between the objective colourings of α and β, or between their colourings as modified by after-images, whenever the grounds a and b are both of one colour. But when the colours or illuminations of a and b are different from each other, the comparison will be very unreliable. When the former condition exists, it interferes with monocular simultaneous contrast, whereas the latter condition is conducive to it.

Just as in the case of a number of experiments on monocular contrasts, there are some other experiments on binocular contrast where the fact of our being accustomed to separate the objective colours of bodies from the colour of a surrounding illumination constitutes a factor.

FECHNER's so-called paradoxical experiment[1] should be described here first. Look at a white surface and open and close your right eye alternately; then, at the moment you close your eye, so that the white surface is exposed only to your left eye, it will look a little darker than it did when both eyes were open. It is natural to suppose that by cutting off the light from one eye the image would be darkened, as it actually is; but the effect is so extremely slight that many people can scarcely see it at all. Now vary the conditions by interposing a rather dark piece of grey glass in front of the right eye. Then, on opening the right eye, the white surface will look darker than it did before, and on closing this eye, it will look brighter—exactly opposite to the previous case. That is, the luminous area appears to be darker when more light falls on the eyes, and to be brighter when less light falls on them! If the experiment is repeated, each time using a brighter piece of grey glass, presently this paradoxical effect ceases and changes to the first effect which was obtained without using any glass at all; that is, the surface begins to appear brighter when the eye that was closed is opened. On the other hand, when the experiment is tried with darker shades of grey glass, a limit is reached finally when it ceases to matter whether the eye behind the glass is open or shut, because the light that enters this eye is too slight to have any effect. The maximum effect is obtained, therefore, with a glass of medium darkness. The glasses which FECHNER himself used in this experiment transmitted between 3 and 5 percent of the incident light. Instead of having an assortment of various shades of grey glass, AUBERT's instrument called an episcotister answers the purpose and is very convenient.[2]

In order to be sure that changes in the size of the pupil had no effect in this experiment, the observer should gaze with his free eye through an opening of smaller diameter than the pupil. In all these experiments small openings in pieces of black paper may be used for darkening the image instead of dark glasses.

One interpretation that might be given to this paradoxical experiment is to suppose that, under certain conditions, the sensation of light in one eye tended to lower that in the other eye, as if there were some

[1] *Abhandl. der Sächs. Ges. d. Wiss.* VII (1860), 416–463.

[2] (This note was added by HELMHOLTZ on page 856 of the first edition.) The episcotister is composed of two black discs made of brass, which are mounted together, one in front of the other, and in each of which four 45° sectors are cut out. The discs can be adjusted relatively to each other so as to leave four slits open whose angular widths may be anywhere between 0° and 45°. By rotating them rapidly the same appearance and effect can be produced as is obtained by using a piece of grey glass, and the amount by which the light is reduced can be computed easily and exactly. The instrument was described by AUBERT in his *Physiologie der Netzhaut*, pp. 30, 34, 283. A similar device had been previously used by TALBOT. See POGG. *Ann.*, XXXV (1835), p. 459.

antagonism between the two retinas. However, by making a slight modification in the experiment, I have been able to show that something of an entirely different nature is involved here.

The observer should take a position where there is some white object before him clearly outlined in the field of view. A white door opposite the windows will answer the purpose. He must look at the door and select a piece of dark glass that is found to be suitable for performing FECHNER's experiment nicely. This piece of glass is held in front of the eye and a sheet of white paper interposed just behind the glass between it and the door, so that it will hide the door and occupy the whole field so far as this particular eye is concerned. By turning the paper more or less obliquely to the incident light, it is easy to regulate the illumination so that it will look just as bright as the door beyond it. Now if the experiment described above is repeated, the result will be just opposite to what it was there. The effect of opening the closed eye behind the glass and the sheet of paper will be to make the door appear a trifle brighter, as if a sort of luminous haze had descended on it. This is the binocular image of the white paper superposed on the door. Having verified this, the observer should then take the paper away and expose both eyes to the door, in which case the door will seem to be considerably darker than it was, although the brightness has not changed at all at those places in the two visual fields where the door appears to be.[1]

This variation of FECHNER's experiment shows that there is no question here of a change in the sensation of the light, but that it is simply a matter of changing our opinion as to the real colour of the white object. If one field is dark all over (as is the case when this eye is closed), or if it is dimly and uniformly luminous all over (as is the case when the sheet of white paper is seen through the dark glass), this uniform illumination, extending far beyond the confines of the field of view that corresponds to the door, is not attributed to the actual colour of the door, but our judgment of this colour is derived entirely from the information received through the other eye which perceives the outlines of the door. Variations of illumination in the first eye can do no more than produce the effect of a dark or bright mist settling down on the door and the other objects. But if the outlines of the door can also be perceived by the shaded eye and appears to it to be dark grey, this grey will seem to belong to the door just as much as does the white in the other eye; and the result is then that the door itself looks darker, like a grey body sparkling with white. But, of course, this shading of the door will not be shown if the darkening produced

[1] Mr. HERING has also noticed that it all depends in this experiment on whether the surfaces seen by the shaded eye are limited or unlimited. See *Beiträge zur Physiologie*, pp. 311, 312.

by the glass is so slight that the additional light entering the other eye is merely noticeable as light; or if, on the other hand, the darkening due to the glass is so great that the objects can scarcely any longer be seen through it.[1]

Similar results are obtained in the case of monocular vision in the experiment made by SMITH and BRÜCKE (see Vol. II, p. 288), which FECHNER calls the "side-window experiment." I found that this experiment could be made in another way, by which the conditions for obtaining the effect could be better regulated than they could be in the original method. I had a plane parallel plate of uranium glass cut in half. This glass does not seem to have any colour at all by candle light, because it absorbs only the violet and some of the blue rays, and there is not much radiation of this kind from the light of a candle. Unless the substance of the glass itself happens to be brightly illuminated, white objects seen through it by daylight appear to be slightly yellowish. But when the glass itself is exposed to the direct rays of the sun, an intensely green fluorescence will be radiated from all parts of it. Now suppose that I place one of the plates in front of one eye and the other in front of the other eye, and that no light can reach the eyes except that coming from the object. If this is a white area on a black ground, and if I see it in separate double images, the two images of the white area, of course, will be seen in the same yellowish white colour. But then if I let the direct rays from the sun fall on one piece of glass, the visual field of that eye will be filled with the green fluorescent light; and after my eye has moved a little, the image of the white area in it will look pink, although it is still flooded with green light, while its image in the other eye will appear brighter and greenish, although objectively it is pure white. Thus in this case, for the eye that is looking through the fluorescent piece of glass, where the whole background is uniformly overspread with faint green light, the limited white area is so completely separated from the diffused green, that even this white assumes the pink colour that is produced when the eye is fatigued with green. In contrast with it the other image, which is not green, appears greenish.

In SMITH's original experiment, as described in Vol. II, page 289, it was the red light penetrating through the sclera that made the image on that side look darker and blue-green, and the other one red. This red light can be rendered visible by illuminating the eye from one side, and then gazing at black letters on a white page, when the former will often look bright red. At the same time, on resolving the image of a black spot on a white ground into separate double images, the image

[1] See Note 2 at the end of this chapter.—K.

belonging to the eye that is illuminated from the side will, of course, look reddish by comparison with the one in the other eye. On the other hand, if green or blue light is concentrated by a lens at a place on the sclera, the white image in this eye will be pink or yellow. As there has been some controversy about the explanation of this experiment,[1] this modification of it with the plates of uranium glass, which enables us to survey all the concomitant conditions more clearly, may prove to be more convincing.

Accordingly, the phenomena of binocular contrast admit of easy explanation from our point of view. But if contrast colours are considered as being caused by changes of sensation due to the spread of the stimulation at one place on the retina to the adjacent places, as was a very prevalent theory at one time, we are obliged to infer that binocular contrast also is due to some action of the sensations in one retina on those in the other retina. Accordingly, this was supposed to be an argument in favour of the innate anatomical connection between corresponding fibres of the two optic nerves.[2]

Dove, who discovered the phenomenon of stereoscopic lustre likewise offered an explanation of it, which needs to be mentioned. He draws a distinction between the shining white light reflected from the outer surface of a body and the coloured light that is radiated from the superficial layers of the substance. His idea is that the effect of lustre is caused by our seeing the illuminated substance behind the illuminated surface; in other words, it is due to light of two kinds, one shining through the other. When two colours are combined, say, red in one field and blue in the other, Dove believes that we imagine that they are at different distances, because it is necessary to focus the eyes differently for each colour. I have not adopted this explanation, because subsequent experiments on judgment of distance by accommodation, particularly in a case like this where the convergence of the two eyes must be kept constant, appear to me to indicate that it is extremely unlikely that any such apparent difference between the distance of the colours can be perceived. Another difficulty about this theory is the fact that lustre can also be produced by combining white and black. Dove's explanation of this effect is that the tendency of white illumination is to cause a contraction of the pupil, which is always a concomitant result of higher degrees of accommodation, whereas the tendency of black is to cause a dilatation of the pupil; and so he infers that different feelings of accommodation are produced

[1] Fechner über den seitlichen Fenster- und Kerzenversuch. *Berichte der Königl. Sächsischen Ges. d. Wiss.* 1861. pp. 27–56.

[2] See Note 3 at the conclusion of this chapter.—K.

by looking at white and black. But in these particular experiments the peculiarity consists in the fact that one eye is gazing at white while the other eye is gazing at black, the consequence being that the two pupils would be of the same average size; and, secondly, any accommodation here would relate only to the edges of the coloured areas, and not to the central portions, and it is difficult to see how a difference in the feeling of the accommodation can arise from the fact that in one of the images white is on the right and black is on the left of the border between them, or white is above and black is below this border, while in the other image it is just the reverse. And so I have ventured to propose the explanation which I have given above and which I think is simpler than the one which was originally offered by the celebrated discoverer of this phenomenon.

Historical.—Some of the earlier investigators were aware of this conflict or rivalry between the visual fields of the two eyes. Du Tour used it as an argument in favour of his theory that only one eye sees at a time, and that therefore things are seen single in spite of both eyes being used. Haldat insisted, on the other hand, that he had seen binocular mixing of colours, and he tried to establish a connection between this phenomenon and the hypothesis of an anatomical union of corresponding fibres of the two optic nerves, which had been accepted by Newton and afterwards by Wollaston and J. Müller. Haldat's view was adopted by Mönnich, Janin, and Walther. On the other hand, J. Müller himself, who had been mainly instrumental in developing the theory of identical places on the two retinas and the consequences of this theory, and who certainly would have been primarily interested in the phenomenon of binocular mixing of colours, never once alludes to it. All he could see was the binocular conflict between them. The wide divergence of views on this subject on the part of more recent observers has been discussed previously. There seem to be great differences between individuals in this respect. As long as the sensation of a compound colour was considered as being a simple effect of two concomitant causes, apparently a sensation of this nature could occur only in one and the same fibre of the optic nerve; and so the observation of actual binocular mixture of colours seemed tantamount to proving that there must be an anatomical fusion of each pair of corresponding nerve fibres. Besides, on such an hypothesis binocular mixture of colours was a necessary consequence. It is true that this particular point loses much of its importance because, as stated above, it comes in conflict with Young's colour theory.

A distinct advance was achieved when Dove found out the objective significance of binocular fusion of different luminosities or colours by discovering the phenomenon of stereoscopic lustre. Although Brewster adopted Dove's theory of this effect, he seems to have been under some misapprehension about it, because he really argues against it. But the simpler theory given above was first suggested afterwards by J. J. Oppel. Without knowing of his explanation, I myself reached the same view of the matter, and emphasized the importance of the phenomenon on account of its bearing on the theory of the sensations at corresponding places on the two retinas.

The phenomena of binocular contrast were not studied until more recently. Fechner, especially, has discussed them very fully. Some previous scattered observations had been made by E. Brücke, H. Meyer, and Panum.

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Notes on §32 by v. Kries

1. According to Schenck[1] and Stirling,[2] a very satisfactory method of obtaining binocular mixture of colours (see page 505) consists in exposing two objects of different colours but of the same rather complicated form, one being visible to one eye and the other to the other eye. For instance, postage stamps, which often have the same design printed in different colours, are particularly adapted for this

[1] Schenck, *Sitzungsber. der phys.-med. Gesellsch. zu Würzburg,* 1898.

[2] Stirling, An experiment on binocular vision with halfpenny postage stamps. *Journal of Physiology.* XXIII (1901), 27.

purpose. The exact correspondence in the forms of the two images makes it easier, of course, to fuse them stereoscopically, and at the same time it aids us in mixing the colours binocularly. As SCHENCK says, some persons who may be totally unable to perceive a mixed colour in any other way can succeed in doing it by this method. A long time ago, after numerous vain attempts, which had made me very sceptical about the whole phenomenon, I happened to use some painted coins in a way quite similar to SCHENCK's method, and did succeed in getting a binocular mixture of colours which was thoroughly convincing. In my own case the method works better when the lines of fixation are crossed. The colour mixture which I obtain in this way is perfectly uniform and stable. After watching it for some time and then screening one eye without changing the fixation of the other one, I am persuaded that the colour which I saw in the mixed image is very different from that which appears in monocular vision merely as the result of the long-continued fixation and modulation (*Umstimmung*) of the eye.

Consequently, as far as I am personally concerned, I must admit the possibility of mixing colours binocularly, although I cannot succeed in accomplishing it except under very special conditions. I think it has to to be acknowledged, without any reservations, that there is such an effect. This being the case, and without disputing such possibilities of illusion as are mentioned by HELMHOLTZ, it would seem natural to suppose that some individuals have much less difficulty in obtaining this effect than others, and that the explanation of the conflicting statements on the subjects is due primarily to real idiosyncrasies.[1]

2. FECHNER's paradoxical experiment (page 522) was subsequently repeated by SCHÖN and MOSSO[2] and more carefully studied in some of its details. They discovered a new effect consisting in a periodic fluctuation of the relations of conflict or mixing, such that a luminous area viewed with one eye screened appeared to get alternately brighter and darker.

By the way, PIPER's observations, in which he found that the sensations of brightness in monocular and binocular vision were not

[1] W. TRENDELENBURG, Versuche uber binokulare Mischung von Spektralfarben *Zft. f. Sinnesphysiol.*, 48, (1913), 199-210. Also, PFLUGERS *Arch.*, 201 (1923), 235-249 — S. DAWSON, The experimental study of binocular colour mixture. *Brit. J. of Psychol.* 8 (1917), 510–551; and The theory of binocular colour mixture. Ibid., 9 (1917), 1–22. — G. F. ROCHAT, Etude quantitative du fusionnement binoculaire des couleurs complémentaires *Arch. néerl. de physiol.*, 7 (1922), 263–267. (J. P. C. S.)

[2] SCHÖN und MOSSO, Eine Beobachtung uber den Wettstreit der Sehfelder. *Archiv f. Ophth.* XX. 2. 1874. p. 289.

connected in the same way in twilight vision and daylight vision,[1] ought to be included here also. In the first case (dim light, dark-adapted eye), the brightness was found to be decidedly greater in binocular vision than it was in monocular vision. Consequently, the threshold values in the former case are only about half as large as they are in the latter case.

As a special form of the rivalry in binocular vision, some reference should be made here also to the case when an after-image is produced by illuminating one eye only, the appearance of which is then watched by closing this eye and opening the other one. Bocci,[2] especially, has made observations of this kind, which show that under these conditions the after-image is often visible. In other words, there is a combination of impressions in which the after-image in the closed eye is involved on one side, and the sensations mediated by the seeing eye are involved on the other side. Obviously, we cannot expect these observations to reveal any information as to the (retinal or cerebral) seat of the after-images.

3. This is the place (see page 525) to mention another observation relating to the connection between the two eyes or their central counterparts. Sherrington[3] tested the frequency of an intermittent light needed to obtain a steady sensation or to get rid of the flicker; and in doing so, he varied the phase-relations between the illuminations of the two eyes in a number of ways. It developed that the essential question here was how these relations were adjusted for each eye separately, and that, so far as fusion or time-discrimination was concerned, each eye apparently operates independently by itself.

4. *Supplementary Note.*[4] The phenomena of binocular colour-mixing have been carefully studied of late, especially by Trendelenburg.[5] These experiments show beyond doubt that with suitable objects of observation a real mixture of colours does take place; that is, there is actually a binocular mixing of colours. As was also found by the earlier investigators, the mixed colours obtained by binocular fusion are practically such as would be obtained by mixing the colours

[1] Piper, *Zeitschrift f. Psychologie* etc. 31. p. 161; 32. p. 161 (1903).

[2] Bocci, *Annali di ottalm.* XXV. 1896. p. 445.

[3] Sherrington, On binocular flicker and the correlation of activity of corresponding retinal points. *British journal of psychology.* I. 1905. p. 26.

[4] ¶Prepared by Professor v. Kries for insertion in the English Translation at this place, and communicated to the Editor in January 1924. (J.P.C.S.)

[5] W. Trendelenburg, Versuche über binokulare Mischung von Spektralfarben. *Zft. f. Sinnesphysiologie* 48. 1913. p. 199.—Idem, Weitere Versuche über binokulare Mischung von Spektralfarben. *Archiv f. d. ges. Physiologie.* 201. 1923. p. 235.

monocularly (that is, by letting the two kinds of light fall on the same place on the retina). However, it ought to be added that this agreement is not perfect. There are some differences between the sensations produced by mixed colours when the observations are made with both eyes and those obtained when only one eye is used. It is true, they are not large but still they are very positive and very uniform. Thus, if a mixture is to look the same way in binocular vision as it does in monocular vision, the lights must have the same wave-lengths in both instances (for example, in yellow-purple and white mixtures), but they must be mixed in different proportions. This result was found also by ROCHAT.[1] Everything connected with the interaction of the two eyes is of so much interest that these differences are very important to consider. However, any attempt at a theoretical explanation would perhaps be premature at this time.—K.

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§33. Review of the Theories

Having thus presented the facts that have been ascertained in regard to the perceptions of vision, we ought now to examine their theoretical bearings once more, in order to decide between the several theories that have been proposed and to see which of them are consistent with these facts, and which are not and therefore less likely to be true.

It ought to be said in the beginning that our knowledge of the relevant phenomena is still too limited to justify us in accepting any one theory to the exclusion of all the others. This being the case, it seems to me that in trying to decide between the various theories the tendency heretofore has been to yield too much to a predilection for certain metaphysical modes of thought, instead of being guided simply by the facts themselves; especially as fundamental questions continue to arise in the realm of psychology which have long since been completely barred from the domain of the phenomena of inorganic nature.

In my judgment, many natural philosophers have been far too ready to presuppose all kinds of anatomical structures in the theory of the perceptions of vision and also to postulate new qualities of the nervous substance that are contrary to what we actually know about the physical and chemical properties of bodies in general and about the nerves in particular. Hypotheses of this kind never attempt to do more than account for some one, or perhaps for a few, of the

[1] G. F. ROCHAT, Étude quantitative sur le fusionnement binoculaire des couleurs complementaires. *Arch. neerlandaises de physiologie de l'homme et des animaux.* 7. 1922 p. 263.

phenomena of vision; and while they are intended to have the appearance at least of being scientific explanations, they either ignore entirely the absolutely unquestionable interplay of psychic phenomena or dismiss them as of comparatively slight importance.

I acknowledge that we are still far from a real scientific comprehension of psychic phenomena. We may agree with the idealistic philosophers (*Spiritualisten*) and take the ground that it is absolutely impossible ever to comprehend them, or we may take precisely the contrary view of the materialistic school, according as we are inclined toward one speculation or the other. The natural philosopher must stick to the facts and try to find out their laws; and he has no means of deciding between these two kinds of speculation, because materialism, it should be remembered, is just as much a metaphysical speculation or hypothesis as idealism, and therefore it has no right to decide about matters of fact in natural philosophy except on a basis of facts.

But, no matter what view is taken of the psychic activities, and no matter how hard it may be found to explain them, there is no doubt as to their actual existence, and to a certain extent we are familiar with their laws from daily experience. It is safer, in my opinion, to connect the phenomena of vision with other processes that are certainly present and actually effective, although they may require further explanation themselves, instead of trying to base these phenomena on perfectly unknown hypotheses as to the mechanism of the nervous system and the properties of the nervous tissue, which have been invented for the purpose and have no analogy of any sort. The only justification I can see for proceeding in this way would be after all attempts had failed to explain the phenomena by known facts.

But, in my judgment this is not the case at all with the physiological explanation of visual perception. On the contrary, the more attentively I have studied the phenomena, the more I have been impressed by the uniformity and harmony everywhere of the interplay of the psychic processes, and the more consistent and coherent this whole region of phenomena has appeared to me.

And so I have had no scruples in connecting and unifying the facts in the preceding chapters by explanations which were founded essentially on the simpler psychic processes of the association of ideas. There is nothing novel about this way of looking at the matter, as I have had occasion to state in the various historical surveys which I have given at the end of each chapter. The views of certain modern physicists and physiologists who have adopted this method, such as Wheatstone, Volkmann, H. Meyer, Classen, and Wundt, have doubtless encountered more opposition than agreement; but aside from the antipathy to philosophical and psychological investigations

that prevails nowadays, I daresay that this opposition has been due to the fact that, as there was no comprehensive treatise on all the phenomena in this territory, questions were continually springing up from the parts of this region which have not been explored tending to cast doubt on the subjects which had been investigated by the scientific men mentioned above. Accordingly, I have taken advantage of this opportunity to go over the entire territory from this point of view and give a complete survey of it.

I venture to state briefly the principles which I have employed in this explanation. The fundamental thesis of the empirical theory is: *The sensations of the senses are tokens for our consciousness, it being left to our intelligence to learn how to comprehend their meaning.* The tokens which we get by the sense of sight may vary in intensity and in quality, that is, in luminosity and in colour. There may also be some other difference between them depending on the place on the retina that is stimulated, a so-called *local sign*. The local signs of the sensations in one eye are entirely different from those in the other eye.

We feel also the *degree of innervation* which we cause to be communicated to the nerves of the ocular muscles. The apperceptions of space-relations and motion are not necessarily derived from the visual perceptions, or at least not from them only, because persons who have been blind all their lives can get these apperceptions very accurately and perfectly through the agency of the sense of touch also. For the purpose of our argument, therefore, we can assume that we are endowed with this faculty.

Evidently, any other sensations, not only of sight but of the other senses also, produced by a visible object when we move our eyes or our body so as to look at the object from different sides or to touch it, etc., may be learned by experience. The content of all these possible sensations united in a total idea forms our *idea* (*Vorstellung*) of the body; and we call it *perception* when it is reinforced by actual sensations; else, it is said to be a *memory-image*. In a certain sense, therefore, (although not in that ordinarily intended when we use this word), such an idea of an individual object is likewise a *concept* (*Begriff*), because it includes all the possible single aggregates of sensation which can be produced by this object when we view it on different sides and touch it or examine it in other ways. This is the actual, the real content of any such idea of a definite object. It has no other; and on the assumption of the data above mentioned, this content can undoubtedly be obtained by experience.

The only psychic activity required for this purpose is the regularly recurrent association between two ideas which have often been con-

nected before. The oftener this association recurs, the more firm and obligatory it becomes.

So far, therefore, as the ideas we get of objects by visual images are correct, the explanation is simple according to the principles above given. But then the question arises as to how it is possible to have *illusions of the senses*. There are two classes of these illusions which we must distinguish. First, there are those illusions in which the impressions on the sense are produced under unusual external conditions. This is the case in looking at optical images in mirrors or lenses or in looking at a stereogram in a stereoscope. Here the impression made by definite objects is produced under unusual conditions. We may be aware of this, and yet, by the law of association of ideas, the impression arouses the idea of the other sense-impression that was generally connected with it, that is, the idea of the object in question.

The other class of illusion is where we get a false view of some real thing, by employing the organ of sense in some unusual way. In trying to explain this type of illusion, it is well to remember that, as soon as we discover that some particular method of using the sense-mechanism is better adapted than any other for giving clear, sure perceptions of objects, we always try to use this so-called *normal* method as much as possible, if not exclusively. Then when the organ of sense happens to be used in some abnormal way, the impressions obtained will naturally arouse the ideas of such objects as the same impressions, or impressions as nearly like them as can be, would have presented to us if the organ of sense were being used in the normal way.

When the eye is used in the normal way, we must consider: (1) that it is in the *fovea centralis* of each eye where discrimination between closely adjacent images is clearest; (2) that the impressions will not remain clear unless the eyes are moved continually to prevent the development of well-defined after-images; and (3) that on an extended surface, uniformly illuminated all over, whatever there is that can be seen distinctly will have been so seen when all the parts of its contour have been seen distinctly. The consequence is that, when the eyes are being used in the normal way, the two lines of fixation will be converged on the point which happens to attract the attention at the time, and the eyes will be accommodated for this point. But instead of ever letting them stay still for any length of time, they are being continually shifted (in obedience likewise to the incentive to move that is characteristic of the attention), being required especially to go over the contours of the observed objects.

This is the explanation of the habitual connection between the movements of the two eyes and between these movements and the accommodation. It is very hard to combat this habit, and yet, as we

have seen, it can be overcome at any moment by voluntary effort, if the eyes are gradually subjected to conditions in which the aims of vision can only be achieved by unusual combinations. This likewise is the reason why it is so difficult to go counter to acquired habit and maintain steady fixation for any length of time. It also explains why the attention is so strongly attracted by prominent outlines, which have so much influence on the movements of the eyes, and why we have so much difficulty in concentrating our attention on a careful analysis of the phenomena connected with indirect vision, the blind spot, double images, etc. For we have formed the regular habit of looking directly at the places that occupy our attention. It is mainly owing also to the way we are in the habit of moving our eyes that we seldom ever notice the double images of objects around us although they are so far apart; and thus many persons continue to be unaware of them even after they have grown to be adults.

I have shown in the previous chapters that the connection that exists between the torsional rotation of each eye and the direction of the visual axis falls in this same category, and that by varying the conditions of vision this connection can itself be altered to suit the optical purpose. I have endeavoured to prove that that purpose was the sureness of orientation by which we are able to tell that a stationary object has not changed its place even though its image on the retina may have been shifted, and that as far as this purpose could be achieved, it was achieved when the movements of the eye were executed in conformity with LISTING's law.

It is possible to show that, at the instigation of voluntary effort, exceptions can occur in case of all these laws of ocular movements, when there are optical ends to be gained by it; and if that is so, then these laws cannot be dependent on anatomical contrivances that act mechanically. Still so far from its being impossible, I am inclined to think that it is even probable that the growth of the muscles, perhaps too even the efficiency of nervous transmission, is so adapted to the demands made on it during the life of the individual, and perhaps by inheritance during the life of the species, that the requisite movements that are the most suitable become also the easiest to execute. In any event, this anatomical mechanism, if it exists at all, is merely conducive, and not obligatory.

Another thing that can be acquired by the ocular movements is a knowledge of the arrangement of the observed points in the field of view. In other words, these ocular movements enable us to find out what local signs in the sensations are characteristic of points that are directly contiguous to each other. Moreover, the special law of the ocular movements determines which magnitudes of space in the field

of view can be accurately compared with each other as to size, and which cannot. Two figures can be compared accurately when a mere movement of the eye is all that is needed in order to form their images on the same points or lines of the retina. On the other hand, when we undertake to compare geometrical magnitudes of this kind whose images cannot be formed on the same parts of the retina, errors will develop, some of which will be constant and others not. The constant errors may be partly attributed to the fact that, at least during childhood when the eye was in process of development, more distant objects were the ones we were most in the habit of seeing, with the ground extending out toward them. I happen to recall here the deviation of the apparently vertical meridian and the mistakes made in trying to draw squares.

Lastly, the influence of the law of ocular movements is shown by the procedure of the lines in the field of view that are apparently straight lines (or shortest lines). Assuming that the line of fixation is in its primary position, which may be considered as being the most common and important adjustment of this line, then the lines in the field that appear to be straight are those lines which, according to the law of ocular movements, can be shifted along themselves.

The derivation of this law was not made to depend on any definite assumption as to the nature of the local signs. It would still be applicable, even if these signs were scattered all over the retina in any haphazard fashion, without assuming any similarity whatever between the local signs of adjacent points. Of course, then it would be far more difficult to acquire the necessary training. However, from analogy with other organic contrivances, as well as on other grounds, I am disposed to think that it is not unlikely that the resemblance between the local signs of adjacent points is greater than it is between those of points that are farther apart, and therefore that the nature of a local sign is a continuous function of the coördinates of the retinal point. But no matter what this system of local signs may be, nor what their own nature may be, doubtless, they are specially contrived so as to facilitate orientation. However, here also the deductions from the empirical theory, with which the phenomena are thoroughly in accord, simply require that any mechanism of this kind shall be instrumental in training the eyesight, without being obligatory as to the ultimate results.

The number of sensitive elements comprised between each pair of points on the retina will belong then to these anatomical contrivances also. This may not be unimportant, especially in the estimate of a very minute distance; in accordance with the general law that, in the absence of other means of judging, magnitudes which can be

clearly distinguished appear to be larger than those which cannot be. It was shown above that the number of sensitive elements has nothing whatever to do with the estimate of larger distances.

Incidentally, so far as the empirical theory is concerned, the form of the retina is absolutely immaterial. Nor does it matter how the image lies on the retina or whether it is distorted, provided it is sharply defined. This theory is concerned exclusively with the projection of the retina outside by the ocular media.

The direction of observed objects with respect to the observer is ascertained by the help of the *feeling of innervation* in the nerves of the ocular muscles. This feeling, however, is continually *regulated* by the result, that is by the shifting of the images produced by the innervations. When a person gazes through a prism and executes movements of his body and hands as they appear in his field of view, he soon learns to see through the prism correctly, notwithstanding the wrong directions of the incident rays. The phenomena of giddiness also indicate a change in the adjustment of the effect of certain innervations.

We are more uncertain about estimating the *absolute* degree of convergence of the eyes than we are about estimating movements of the two eyes when they are in the same direction. Possibly this is because a more lasting fatigue may be produced by convergence without being counterbalanced by the fatigue that comes from divergence; whereas it is probably not easy to turn the eyes to the right for any length of time without relieving them occasionally by turning them to the left, so that the fatigue will be more evenly distributed over the antagonistic muscles.

We are consistently in the habit of disregarding the subjective factors in our sensations. Thus, in looking at a near object, all the visual impression and feelings of innervation are taken in a lump and the sum of them considered as being simply the sensible token of an object's being situated there. We do not stop to analyze our impressions and examine which of them belong to this eye or to that eye, or what is the position of one eye or the other. Partly on this account, and partly for the reason mentioned in the preceding paragraph, the judgment of the direction of an object with respect to the observer is based on the *common* or *average* direction of the two eyes; and this is the case even when the object is actually seen only by one eye. This is in conformity with the general rule, that when our impressions are obtained by using the organ of vision in some unusual way (monocular vision), our judgments are formed by the similarity to the impressions we get under normal conditions (binocular vision). Accordingly, this leads to the rule formulated by J. TOWNE and

E. Hering as modified by me for the case of torsional rotations when the eyes are tilted obliquely.

This brings us to the subject of *binocular vision*. As long as we confine ourselves to the territory of objective phenomena, and are considering how we see bodies or stereoscopic pictures, the empirical theory affords a simple explanation which is easy to understand. Except in some very recent works on this subject, even those writers that prefer intuition theories have generally been willing to acknowledge the influence of experience in this region. The illusions that are obtained here may be explained by the uncertainty in estimating the convergence of the eyes. When the pictures presented to the eyes are such as could be produced only by real objects for some definite degree of convergence, they are interpreted accordingly, even though the actual convergence at the particular moment may be different from that. Moreover, owing to this uncertainty about the feeling of convergence, we cannot be sure also of our judgment of the changes of torsion of the convergent eyes when the plane of fixation is raised or lowered. Thus unless we are made aware of the existence of this rotation by the deviations of the lines in the observed image, our judgments will be formed without taking it in account; and then we have the illusions which have been described by Recklinghausen and by Hering.

But now if, without any change of the point of fixation, the attention is turned to the superficial distribution of the objects over the field of view, the appearance of this distribution will not be the same in both eyes, and the two images that are seen cannot be perfectly congruent. Thus, if some particular points in these images are congruent, others must be disparate, and the result is that the latter will apparently be in different places in the common field of view, causing double images. Points on the two retinas or on the two visual globes, whose images coincide in the common field of view, are said to be *identical* or *corresponding* points.

Now as to the nature of corresponding points, the facts of the case are such as to warrant the following statements at least:

1. Generally speaking, the images of corresponding points are projected to the same places in the common field of view, whereas the images of non-corresponding points are projected to different places. Still in cases where two images are fused so as to get the apperception of a material object, minor exceptions may occur to both statements contained in this rule.

2. The sensations produced by stimulating corresponding points on the two retinas are not identical, but different. This is a necessary inference from the fact that the correct relief is always obtained from

a stereoscopic line-drawing even when it is illuminated instantaneously by the electric spark. Were the sensations of corresponding points equal, so that there was no possibility of discriminating between them, it should be possible to obtain the reverse relief just as easily and just as frequently as the other.[1] The same conclusion is reached from the fact that a difference in the illumination or colouring of corresponding surfaces produces another apperception, namely, that of lustre, such as is never obtained when the colouring of the two surfaces is the same, no matter how the colour is chosen. That neither the ocular movements nor the rivalry between the two visual fields has anything to do with this phenomenon, is shown especially by instantaneous illumination with the electric spark.

3. The effect of habitually abnormal adjustments of the eyes is shown by a change in the relation of correspondence between the two retinas of a person who is cross-eyed.

And so I am led to conclude that any anatomical hypothesis which assumes that the sensations in the two eyes are completely fused with each other, particularly, therefore, any hypothesis which assumes that the fibres proceeding from corresponding places on the two retinas are united in a single fibre that transmits the impressions in the two eyes unseparated to the brain, is inadmissible and incompatible with the facts. The only form of such an hypothesis that would seem to me allowable would be one in which the two impressions came to perception in the brain separated to some extent, and yet also partly united in producing a common or equal effect. Thus, suppose that the fibre A coming from the right eye was split into the fibres a and α, and that the corresponding fibre from the other eye was split into the fibres b and β; and that a and b pass separately into the central organ and produce different impressions, whereas α and β are united to make a third impression which is common to both.

Without considering it either probable or necessary, I might agree that a modified theory of this kind was admissible. On the other hand, the deductions that follow from the explanations which have been given in the previous chapters furnish what appears to me to be a completely satisfactory interpretation without resorting to any such hypothesis. The lines of fixation in normal vision are always directed to the same objective point as that on which the attention is focused at the time. But at all other places on the retinas impressions are

[1] DONDERS states (*Anomalies of accommodation and refraction*, London, 1864, pages 162, 166) that the pseudoscopic picture often appears instead of the stereoscopic one when the eye is perfectly motionless. However, in an article which has just appeared in the *Nederlandsch Archief* (1866), where he has employed precautions similar to those mentioned on page 455 above, he describes results that are like those obtained by AUBERT and by me.

produced that are sometimes equal and sometimes not. Therefore, the main fact of all is that the localization of the impressions in the two foveas is a consistent one. On the other hand, if, in consequence of some impairment of the ocular muscles, it is impossible to produce the adjustment of the eyes required for this purpose, and if some other adjustment has become habitual, it is this latter adjustment that determines what point on the retina of one eye will correspond with the *fovea centralis* of the other eye, in the case of both eyes.

The identity of the pairs of meridians is determined by the places where the images are most frequently formed of rows of the same points. When the plane of fixation is in its primary position, which may be regarded as its mean and most natural position, this occurs, in the first place, on the retinal horizons. In the next place, in the case of many normal eyes, the lines on the floor extending to the horizon apparently have a decisive influence on the positions of the corresponding vertical meridians.

When these two pairs of corresponding meridians have been located, the other alignments of the two visual globes, together with the positions of the congruent points on them, can be determined by the ocular movements, exactly as has been described in the preceding pages.

Since, therefore, the comparison of dimensions on the two visual globes and of the positions of congruent points is a result of the development of the eyesight, there may be *minute discrepancies* in these measurements, when a very vivid apperception of bodily unity in the two images takes possession of us. On the other hand, when the intervals between the double images are very decided, the interpretation of them may be approximately correct, without being inconsistent with the fact that they were perceived as separated in the field of view. Whatever tends to prevent the fusion of the double images into the perceptual idea of a body, or facilitates the comparison of their positions in the field of view, as, for instance, avoidance of any movements of the eyes or practice in observing double images, will make it easier to see them. Double images that are just over the threshold of perceptibility can sometimes be seen even by the light of the electric spark, when movements of the eyes cannot possibly have any influence, and then again sometimes they cannot be seen. It depends on where the attention happens to be focused at the time. All these circumstances agree very well with the explanation that has been given, and can be deduced from it.

Finally, the phenomena of *rivalry* depend on the characteristic of our consciousness that prevents us from taking in more than one impression at a time or more than such an aggregate of impressions

as are united in a simple idea. We are continually meeting instances of this peculiarity, but aside from these every-day experiences, it is very clearly manifested in the well-known difference of time between the visual and auditory perceptions in making an astronomical observation of the transit of a star; and it is shown also by the limited number of objects that can be perceived by the sense of sight by the light of the electric spark during the brief period that its impression persists. The form of the union of the impressions in the visual fields of the two eyes is the apperception of material things. Where this does not succeed on account of the nature of the two images, the attention will waver, as shown by the rivalry between the two visual globes, unless the attention is riveted by sharply defined outlines in one of the fields. I have described previously the various methods of keeping the attention fixed on one of the fields and preventing it from fluctuating. This is likewise a proof that this conflict is simply a phenomenon of the attention.

From this review of the proposed explanations it follows that the only psychic processes involved therein are the involuntary ones connected with the association of ideas and with the voluntary flow of ideas which are not directly subject to our consciousness and our will; although, by making self-conscious ideas and aims concur with them, we can exert a certain influence on their course. For this very reason the effects of these ideas are so powerful as to be practically beyond our control, the will and the consciousness being confronted as if by some *force of nature*, exactly as in the case of the sensations that we obtain directly from outside. Thus, whatever is joined with the sensations in the results of psychic processes of this kind seems to us to be also the effect of an external agency, just as the immediate sensation itself is, and not something discovered by conscious free reflection, *thought out* by our own selves. The empirical theory has been subject to much misconception in this respect on the part both of its advocates and of its opponents; and therefore I desire to call special attention to this point. If anyone objects to including these processes of association and the natural flow of ideas among the psychic activities I will not quarrel over names. Possibly the empirical theory might be united here with that form of the intuition theory which was proposed by Panum, for instance, except for the fact that what he regards as being natural endowment appears to me to have been acquired by experience.

Now let us take up the various *intuition theories*. The cardinal fact about them all is that the localization of the impressions in the field of view is derived through some innate contrivance, and either the

mind is supposed to have some direct knowledge of the dimensions of the retina, or it is assumed that, as the result of the stimulation of definite nerve fibres, certain apperceptions of space arise by virtue of an innate mechanism which cannot be further defined. The first form of this theory was developed especially by J. MÜLLER. He says[1]:

"The concept of space cannot be acquired (*erzogen*); rather it would seem that the apperception of space and time is a necessary assumption, is itself a form of apperception for all sensations. As soon as there is any sensation, the sensation is in these forms of apperception. However, so far as the *plenum* of space is concerned, we '*sense*' everywhere nothing but our own selves spatially, if we are speaking here simply of sensation and sense. And by the judgment we distinguish from objectively filled space so much of the spatial parts of our own bodies as are in the state of stimulation (*Affektion*), with the accompanying consciousness of the external cause of the sense-stimulation. On each visual globe the retina sees itself only as it is extended in space in the state of stimulation. Even when the eye is in perfect repose and seclusion, it 'senses' itself dark in space."

KANT's opinion was that space and time are forms of our apperceptions with which we are endowed originally; but we see from the above that MÜLLER's view extends this idea by supposing that even the special localization of each impression is given by the immediate apperception. This opinion was adopted by most German physiologists, who devised many kinds of explanation of visual phenomena based on the special peculiarities of the form of the retinal images. Thus RECKLINGHAUSEN[2] endeavoured to explain the obliquity of angles that look like right angles by the fact that the surface of the retina is not perpendicular to the visual axis of the eye, and that therefore the optical image of a right angle as formed on the retina might be an oblique angle. This condition of the retinal images was therefore supposed to be capable of being perceived directly. E. HERING[3] and A. KUNDT[4] went so far as to suppose that the mind beheld directly the distance between a pair of retinal points, not along the arc of the retina, but along the chord; and, as has been mentioned already, they attempted to explain the illusions of monocular localization in the field of view in this way. We showed that this hypothesis was not at all adequate to explain the special phenomena for which it had been devised.

Strictly speaking, the assumption of the intuition theories which has just been discussed is tantamount to giving up the attempt to explain the phenomena of localization. Of course, in that case there is

[1] *Zur vergleichenden Physiologie des Gesichtssinns*. pp. 54 ff.

[2] *Netzhautfunktionen im Archiv für Ophthalmologie*. V, 2. pp. 128–141.

[3] *Beiträge zur Physiologie*. Heft 1. pp. 65–80.

[4] POGG. *Annalen*. 1863. CXX, pp. 118–158.

no use arguing the matter any further; and J. Müller, especially, is not to be blamed, if, before any observations whatever had been made concerning the law of ocular movements, and when nothing but absolutely vague deductions could result from trying to explain localization by means of them, he was not inclined to proceed further in his efforts of explanation. On the other hand, as I have been at much pains to show in the foregoing pages, the characteristic features of the eyesight, which receive no further explanation at all on the intuition theory, can be derived from the law of ocular movements as far as we have yet been able to ascertain the characteristic features of this law.

This idea, that we were originally endowed with the localization of the impressions in the field of view, necessarily implies that we must also have been originally endowed with the faculty of divining what points on one retina give the same localization as those on the other retina; that is, what points on the two retinas are *corresponding* or *identical* points, as they are called in the intuition theory. Therefore this doctrine of the innate and anatomically dependent identity must be considered as a necessary consequence of the intuition theory; but this is just where the essential difficulties of this view are encountered, as we have pointed out before. And so this region has been the main battleground of the questions at issue.

In the first place, by observations of real material objects, and especially after Wheatstone's invention of the stereoscope, it might have been learned that we do not by any means always see double images when we should expect to see them on the strict theory of identity, and that they disappear under the influence of the apperception of bodily extension. It is true that Brücke rightly insisted on the great importance of the ocular movements here. Still even when this influence is eliminated, the fact remains that even the most practised observer will fuse inseparably certain similar double images that are adjacent to each other, although he can distinguish similar images in the monocular field with the utmost ease when they are just as close together or images in the binocular field when they are different in colouring. It was a still greater shock to the advocates of the theory of identity when Wheatstone announced the fact that under some conditions it was even possible to separate the impressions at identical retinal points and project them close together at two different places on the object. But this latter fact, as I have proved above, is a necessary consequence of the former; and it can be actually observed when the experiments are properly devised. Only, it is not fair to do as those who have disputed Wheatstone's statement have invariably done, that is, require that more be accomplished in separat-

ing identical impressions than can be accomplished in uniting disparate impressions under the same conditions.

Realizing the important bearing of the facts, PANUM was in favour of modifying the theory of identity in such a manner that every point a on one retina was supposed to be identical with a certain corresponding circle of sensation A on the other retina [*supra*. p. 457]; so that, where the outlines were similar, it *might* be possible for the image of the point a to be fused with an image at any single point in A. However, in this case the perception of depth was supposed to be different according as a was fused with one point in A or with another point. Whether a fused with this point or with that, was supposed to depend on the place in the circle of sensation A where there happened to be a contour similar to the contour going through a. PANUM used the phenomena of rivalry to show the dominating power of contours in the common field of view of the two eyes, although he doubtless considered the victory of the contours to be too complete and too permanent. According to him, rivalry takes place mainly between dissimilar colours and contours that are, however, of nearly equal intensity. Like colours and contours try to fuse.

Considered merely as a comprehensive statement of the facts, which after all is the point that PANUM himself stresses as the more essential and important thing, these propositions are in the main correct. The only objections I have to offer would be: (1) Personally, I have not been able to verify the real existence of binocularly mixed colours even in the case of the experiment he describes. (2) Mr. PANUM did not use any satisfactory methods for focusing the attention, and consequently he failed to recognize the important rôle that the attention plays in this conflict between the two visual fields and in the discrimination of the double images. (3) He regards the movements of the eyes in the fixation of the images as being partly involuntary reflex movements, whereas in my own case, although I may notice, perhaps, a tendency to certain customary adjustments, it does not have the slightest effect on the voluntariness of the movement, supposing I desire to change the place of the point of fixation somewhere else. (4) The decisive factor in the fusion of the double images is not simply the similarity between the contours or how nearly they happen to be in a corresponding situation, but it is also the presence or absence of other points of comparison for making a correct estimate of the apparent position of the two contours in the common field of view This latter fact had already been shown by BERGMANN's experiments,[1] and it is shown in a similar way by the experiment with stereogram U,

[1] *Göttinger gelehrte Anzeigen.* 1859. pp. 1055–1063.

described on page 457; even if Volkmann's experiments should be disregarded, since Panum has raised an objection to them on the ground that some slight changes, insignificant as they are, were made in the contours by the added lines and points, thereby preventing fusion at these places. But, as shown by both Bergmann's experiments and my own, the mere presence of a pair of corresponding lines both situated on the same side of a pair of disparate lines and not affecting the similarity of the contours of these lines in the least, may prevent the latter from being fused, as they would be if the pair of corresponding lines were not there.

After being amended and revised, the explanations given by Mr. Panum in his second publication on this subject[1] hardly amount to anything more than elevating each class of observations to a special faculty of the nervous mechanism. Thus he attributes to the two eyes or to their nervous mechanism a *binocular energy of colour mixing*, whereby colours seen binocularly may be united into the mixed colour. But along with this there is another so-called *binocular synergy of alternation*, whereby colours seen binocularly may also not be united, but may come in conflict. The latter is supposed to prevail when the stimulations acting on the two eyes are very intense or when the susceptibility of the organ of vision is very keen. Disparate images may be united by a third *binocular synergy of single vision by corresponding circles of sensation*. And, finally, perception of depth is produced by means of a fourth specific *synergy of the binocular parallax*.

The contours of figures are regarded as being particularly strong nervous stimuli, and the adjustments of the eyes as being essentially reflex movements occurring involuntarily. Moreover, with respect to these synergies, Mr Panum especially insists that they are to be regarded as physiological, not psychic, forces.

I must admit that I do not exactly understand how Mr. Panum imagines that the main thesis of the doctrine of identity can continue to be maintained along with the fusion of disparate points in corresponding circles of sensation. Mr. Volkmann has called attention to this real or apparent contradiction. Mr. Panum declares that his arguments were that the impressions belonging to corresponding circles of sensation *might* fuse, but that impressions on identical places *must* fuse. But still it would always follow from this that, whenever the impression a on one retina fuses with that on a disparate place β, a would necessarily have to fuse also with that of the identical place α on the second retina, and, consequently, α and β, two places in the same image, *must* fuse also with each other unless one of them is extinguished, which certainly does not happen in a number of cases;

[1] Reichert und du Bois-Reymond, *Archiv für Anat. und Physiol.* 1861. pp. 63–111.

for example, in case of the experiment described above. In stereograms like *M* and *N*, Plate III, two lines which are identically situated, but which do not fuse, are made prominent by contours. Neither disappears by conflict with the other; otherwise, even when they were illuminated by the electric spark, there would be no stereoscopic perception of depth due to one of the lines being united with a disparate line in the other diagram. Similarly, when the disparate edges of two differently cóloured areas fuse together, there must always be certain identical points in between them where the conflict of the colours that are made prominent by the adjacent contours will be evenly balanced, so that both colours will be seen, in this case projected to different points of the material object which is being viewed. Incidentally, so far as I can see, this point of dispute is of little importance for the theory; and, moreover, as the result of my own observations, I must consider it as settled in favour of WHEATSTONE's assertion. Even if the *necessity* of the fusion of the impressions on identical places is not insisted on, these points will always continue to have a real significance from the fact that the nearer similar impressions on the two retinas are to indentical places, the easier it is for them to fuse. Moreover, in my opinion this is the only correct description of the relation of identity, no matter what its basis may be considered to be; and by bringing out this relation clearly and inventing descriptive terms for it, Mr. PANUM has helped to make a real advance in the theory of binocular vision, which I gladly recognize. I may add that I should certainly be the last person to find fault with his hesitation and caution in making a theoretical generalization from the observed facts; nor should I have criticized his efforts here to present a theory, which he himself asks shall be considered of secondary importance, if I had not been obliged anyhow to speak of the possible modes of explanation in this region and if PANUM's theoretical views had not formed the basis also of HERING's more recent theory, which will be considered presently.

From the synopsis of Mr. PANUM's explanations as given above, the reader can see that, at least so far as fusion and rivalry of the images is concerned, they are really only in the form of explanations, since the facts are summarized in an abstract concept. If they have any bearing on the causal relation, it is in a negative way, by insisting on not including psychic processes, although the argument for not doing so is invariably supported by facts which have not been observed thoroughly. Incidentally, these explanations attribute forms of activity to the nervous substance such as may perhaps be found in the region of the lower psychic activities, but nothing similar to which has ever been discovered in the domain of inorganic nature.

The main features of Panum's theory, in a clearer and more firmly developed form, are to be found in E. Hering's proposed theory of binocular vision. It is the most logical of all the forms of the intuition theory, and therefore it deserves a more thorough discussion. Hering's theory represents a considerable advance, because it starts out with a better comprehension of the apparent visual directions of the observed objects, and thus avoids essential difficulties which were encountered by the earlier theories.

Mr. Hering assumes that when the individual points of the retina are in the state of stimulation, there are three different kinds of space-feelings besides the colour sensations. The first one corresponds to the altitude-value (*Höhenwert*) of the given place on the retina, and the second to the azimuth-value (*Breitenwert*). The altitude-feeling and azimuth-feeling, which together give the feeling of direction for the locality in the common field of view, are equal for corresponding points on the two retinas. There exists also a third space-feeling of a special kind, which is supposed to have equal and opposite values for each pair of identical retinal points; whereas for any pair of retinal points which are symmetrically situated these values are equal and of the same sign. The depth-feeling of the temporal halves of the two retinas is positive, that is, corresponds to increase of depth; and that of the two nasal halves is negative, that is, corresponds to decrease of depth.

It was stated above that a necessary requirement of any theory of identity was that it should not be incompatible with the facts. By making the above assumption, Hering's theory satisfies this condition to begin with. The impressions on corresponding places of the two retinas are, indeed, partly equal, that is, as to the feeling of direction; but they are partly different, namely, with respect to the feeling of depth. Thus far Hering's assumptions, while, of course, they would not be necessary, might be regarded as advantageous even for the empirical theory which I advocate. The training of the eye-sight in the development of the sense of sight would be decidedly easier to explain by an assumption of this kind. Only, in that case, the "space-feelings" would have to be regarded as local signs, the space-significance of which would have to be learned by experience. Still it would be an obvious advantage to have equal signs for whatever was called equal.

There would only be one change that would have to be made in Hering's assumptions. That would be with respect to the deviation of the apparently vertical and identical meridians in the case of eyes in which this deviation occurs, as shown by the experiments of Mr. Dastich and myself. Thus, we should have to take the altitude-values and azimuth-values as being equal for identical places also;

but the positive and negative values would have to be separated, not by the corresponding apparently vertical meridians, but by the meridians that were really vertical. Thus, when the eyes are adjusted symmetrically, a line whose images are formed in the really vertical, but not identical, meridians will be seen perpendicular to the visual plane, as has been previously stated; whereas one whose images are formed in the two apparently vertical identical meridians will be inclined toward the observer, the upper end of the line being nearer to him than the lower end. As far as I see, this modification would have no other influence on the further consequences of the theory.

But now in HERING's theory also we encounter again the mystery of the doctrine of identity: *Equal or unequal luminous stimuli acting on coincident points* (that is, corresponding points) *always excite only a simple luminous sensation*. Accordingly, as is constantly emphasized in HERING's book, they *must* necessarily be united; and yet, on the other hand, even disparate images of corresponding circles of sensation *may* be united. Moreover, in HERING's case this proposition strikes me as being more the result of a polemic attitude toward opponents of his theory of identity, who were perhaps too critical (*eingreifende*), than a necessary requirement of the theory. I do not see why it might not be eliminated from the theory without any detriment to it. In place of it the statement could be substituted that images of similar contours and similar colouring are easier to fuse, the nearer they are to identical places.

Instead of assuming an organic basis of this single vision in the case of disparate places on the two retinas, as Mr. PANUM did, Mr. HERING, on the contrary, assumes a psychic basis. His argument in favour of this is that practice and a certain training of the attention are requisite for separating compound sensations. Now this proposition is thoroughly true, and there are many other apparently contradictory phenomena in this region that may be explained by it besides those instanced by Mr. HERING. The following is a special difficulty in the way of his theory here. Suppose that a and α are corresponding places on the two retinas, and that b is a place in the same eye as a and adjacent to it; then if equal images are formed at b and α, the reason why these images fuse, according to Mr. HERING, is because they are equal in quality, very much alike in direction-feeling, and do not differ much except in depth-feelings, and because we do not take time to consider them separately; but, if we do attend to them, we hasten to focus them both—which, by the way, is supposed to happen, according to him, by a sort of reflex movement—and then we see them single. Now I ask why is it then so much easier to discriminate when two images of the same kind fall on the retina at a and b? In this

case the images are not only qualitatively equal, with the same slight difference in their direction-feelings as there is between b and α, but the difference of depth-feeling is just as slight as the other difference, whereas the difference between b and α in this respect is a very large one. Thus, according to Mr. HERING's argument, the result is that the sensations a and b would still have to fuse very much more readily than those of α and b, although that is exactly opposite to experience. Mr. HERING's answer to that might be, that on trying to focus a or b, we find that we can only focus one of them at a time, and thus we have *learned* to distinguish between a and b, but not between α and b. But if he should say this, he would have come over completely to the standpoint of the empirical theory, according to which we must *learn* to distinguish and interpret the sensations of the local signs.

And it is just here at the very place where Mr. HERING himself is forced to take refuge in the psychic theory in order to escape from the difficulties raised by his own argument, that he takes occasion to attack VOLKMANN's and other psychological explanations. However, VOLKMANN's mistake in this instance, if one chooses to call it so, practically amounts simply to this, that in speaking of the psychic processes that are involved here he employs the terms by which they are called when they are elevated to the realm of self-consciousness. And yet to a certain extent there are no other terms that can be used, because all anybody can do is to give provisional names to processes when he means to imply that that is all that is known about them. Certain processes of this kind are known only by their results, and therefore when we refer to them as *unconscious psychic processes*, the meaning is quite clear. It is the only way we have of describing them without resorting to awkward circumlocutions in each instance.

According to HERING, when two impressions are fused binocularly, the total sensation has the mean value of both the direction-feeling and the depth-feeling. Since the depth-feelings of identical places are equal in magnitude but opposite in sign, the mean value of the depth-feeling amounts to zero when identical impressions are fused. Obviously, the mean value of the depth-feeling will be positive or negative, according as the double images of an object are on the same side or on opposite sides, respectively; in the former case, therefore, the object will appear to be farther away than one whose two images are identical, while in the latter case it will appear to be nearer.

If it were a positive necessity for every impression on one retina to be always united in equal intensity with the corresponding place on the other retina, the mean depth-value of this union would invariably be zero. In the conflict between the two visual fields the impression of the field with the contour in it suppresses the impression of

the other field, and that is why the depth-value of the contour is left free by itself to enter into union with the corresponding contour in the other field with its own proper value. This explanation is refuted also by the modifications of WHEATSTONE's experiment which were described above, and in which unlike contours that are not united fall on coincident places, each of them coming out in the stereoscopic picture with its own depth-value, even when the illumination was produced by the electric spark. This shows that neither of them is extinguished in the conflict.

Now this is the assumption on which Mr. HERING builds his construction of space. He assumes that by an immediate act of sensation all image-points for which the depth-value is zero appear on a plane, the so-called "*Kernfläche*" *of visual space*. Let us suppose that the point in this plane that corresponds to the centres of the two retinas is the origin of a system of rectangular coördinates, the ordinates corresponding to the depth-values being perpendicular to this plane; then the three coördinates of any observed point ought to be proportional to the altitude-values, azimuth-values, and depth-values of the space-feeling due to the binocular impression, and thus, according to HERING, we ought to obtain a distribution of the observed points in the visual space, in which the way the points were arranged would at least be in keeping with their real arrangement, although the relations between the various linear distances might still have to be checked frequently and corrected by experience. Since the parts of the observer's body appear also in the visual space as thus constructed, we get also at the same time the apperception of the space-relation between the observed objects and the observer's body.

These are the essential features of HERING's theory. In the earlier intuition theories only the distribution of the observed points in the field of view was considered as being innate, whereas the perception of depth-dimensions was supposed to be due to an act of judgment. The hypothesis that an immediate sensation of depth-relations might be the result of binocular parallax, had been suggested first by PANUM, although it had not been worked out in any more definite form. Mr. HERING has tried to perfect this hypothesis in the manner described, and in doing so he has extended the territory of the intuition theory beyond its original confines. The system which he has developed shows evidence of a clear and logical faculty; it takes complete account of all the previous facts and also of some important new ones contributed by Mr. HERING himself. It is just because this theory strikes me as being a good type of this class that I have ventured to direct my criticism against it particularly.

The first objection which I should have to offer, and which to my way of thinking seems absolutely insuperable, is that I cannot conceive how a single nervous stimulation can produce a completed idea of space without antecedent experience. However, I realize that this objection is probably of too metaphysical a nature to be considered on scientific grounds, and so I simply register it here for the benefit of those readers who share my view. I turn, therefore, at once to the arguments against this theory which are supplied by the experimental data themselves.

I have already stated that the assumptions of the PANUM-HERING theory of the two visual fields are in conflict with the facts. The assumption that the impressions in the two fields must fuse into one sensation, in which first one impression or the other may be uppermost alternately, but only by gradually rising and falling, is disproved by the fact that it is possible to perceive stereoscopic lustre by instantaneous illumination. The assumption that, in the cases where disparate contours fuse, the identical images of them on the other retina are suppressed, is disproved by the success of WHEATSTONE's experiment when it is properly performed, and especially by its success with instantaneous illumination, where the movements of the eyes cannot possibly have any influence.

Another fundamental hypothesis in HERING's theory is that the points whose images are cast on identical places of the two retinas (or rather the points whose depth-values are zero) always appear to lie in one plane, and that whether the point in the object which is observed shall be in front of this so-called *Kernflache* of the visual space or behind it, must depend on whether its stereoscopic parallax is positive or negative, respectively. A series of experiments were described on pages 319 foll., which showed that simple systems of lines, which had exactly the same binocular parallax, could be combined stereoscopically so as to look either like a curved surface or like a flat surface, without the presence of any other tokens of depth-apperception whatever. The effect depended on whether the transverse lines tended to make the binocular images appear like a close object as viewed with convergent lines of fixation or like a distant object as viewed with the axes of the eyes parallel.

Moreover, if a system of plumb lines lying on the cylindrical surface of the longitudinal horopter appears to Mr. HERING to lie in a plane (which he intimates is not absolutely true even for the case of his own eyes), then, as I have shown, it must be on account of some individual peculiarity of his eyes, because it was not the case with any of the observers whom I investigated nor was it so in my own case. I have shown that for most observers the error made in estimating the

convergence of the eyes, which is apparently the fundamental thing connected with this phenomenon, is far too small to account for the result which Mr. HERING claims to have obtained.

A main difficulty in HERING's theory, or rather what I should be inclined to call an impossibility, is with respect to the depth-feelings. As long as the impressions on one retina are united with corresponding impressions or disparate impressions on the other retina, where the difference between the depth-feelings is all that matters, I am not aware that there is any essential difficulty except those just mentioned. But when the image on one retina does not fuse but remains standing by itself, and dominates in the conflict with the image on the other retina, Mr. HERING not only assumes, but he is bound to assume, that the depth-feeling of the victorious impression in the conflict gains the mastery without being fused with that of the corresponding place on the other retina.

Mr. HERING thinks he can adduce some other experiments also,[1] in which monocular images of this kind might be made to appear with the depth-impression belonging to them by themselves:

(*a*) If the point of fixation is in the median plane, and if there is another point beyond it or in front of it, the latter will be seen double, the two images also being apparently beyond the point of fixation or in front of it, and not far from the real place where their object is. This observation does not conflict with HERING's theory; at the same time it proves nothing in its favour, for we are just skillful enough to be able to tell nearly correctly the place where an object is which is seen by double images, provided the latter are not too far apart. The decisive factor here is experience, not depth-feeling, as is proved by the subsequent experiments where the two come in conflict, and where experience always triumphs, it seems to me, or generally at any rate, as Mr. HERING acknowledges.

(*b*) Two little marbles are suspended by cords side by side. The visual axes of the two eyes are made to intersect beyond them, and so three marbles will be seen: one in the middle, which is seen binocularly; one on the right, which is seen monocularly by the left eye, and one on the left, which is seen monocularly by the right eye. According to HERING, the marbles on the sides ought to appear nearer the observer than the one in the middle. Now I have tried this experiment myself, and I find that the result he gets depends on the way the head is held. If I look at the marbles with my head tilted backward, that is, with the visual plane inclined below its primary position, the lower end of the middle cord, where the marble is which is seen by both eyes,

[1] *Beiträge zur Physiologie.* 5. Heft. pp. 338–342.

will appear to be nearer to me, as was explained on page 326; and then the middle marble will look nearer too than those on either side of it. When my head is bent forward, the appearance is just the opposite; and then, of course, this is the result that ought to be obtained on HERING's theory, but the reason for it is entirely different. If the head is bent first forward and then backward, the position of the marble in the middle will change back and forth in the same way.

(*c*) Fasten a vertical wire by the side of a pin, a little to the left of it, and somewhat nearer the eyes. Then if you gaze at the head of the pin, you will see two images of the wire, the one on the right being in your left eye, for which the depth-value should be negative, and the one on the left being in your right eye, for which the depth-value should be positive. Consequently, the image on the right ought to appear much nearer than the pin, and the image on the left much farther away. Mr. HERING admits that an apperception of depth such as this is extremely difficult to see, and is fugitive, the reason being, he believes, because the slightest wavering of the convergence will correct the judgment as to the place where the object is. However, not to do him any injustice, I prefer to let him describe the result of this experiment in his own words:

"In the first place, and generally whenever my eyes move, no matter how little, I see the two images of the nearer wire, separated indeed, but *both* nearer than the single image of the focused needle. But if my eyes stay perfectly steady, and my entire attention is concentrated with all my might on the focused needle, suddenly the illusive image in the left eye recedes *behind* the pin and appears on that side of it with so much energy that I can only compare this impression with the powerful impression that is produced when stereoscopic images suddenly extend in depth. The phenomenon occurs with the greatest certainty at the very moment when I am least expecting it. However, the slightest faltering of the gaze or simply *the thought* that the other illusive image is nearer will instantly restore the first one again to its place *in front of* the *Kernfläche*; for then the fact that the two images are related to one and the same object comes in and disturbs the pure sensory impression. But the moment that, owing to the eye's being stationary, the illusory image enters an adverse phase of the conflict, such as was discussed above, the phenomenon disappears. Thus many things conspire to upset the experiment. Generally I cannot recommend it except for persons who have had *much* training in indirect vision, and who really can fixate steadily, not simply believe they can. The most delicate double vision cannot be acquired in one year, hardly even in two."

Several pages earlier, speaking of disturbances to which the sensation is liable in these experiments, Mr. HERING says *à propos* of this same matter:

"Besides, when the illusive images are at all extended, the conflict does not always exhibit equal phases in all parts of the image, but the latter is victorious at some places in the conflict and vanquished at others; and the

consequence is that any sure and fixed localization is quite impossible. Thus pieces of the image lying on the proper coincidence place in the other retain may force their way into the illusive image with their opposite depth-values, and become, as it were, parts of it; and if this happens, *the localization may even prove to be opposite to that which was to be expected* a priori."

Now the latter part of this description agrees exactly with my own observations in performing this experiment as carefully and conscientiously as I could. I have gazed at the pin so long and so fixedly that at last everything was extinguished by the negative after-images. There is a stage when all that can be seen are the nebulous individual parts of the double images of the wire emerging now and then in the course of the conflict with the ground on which they lie and with the after-images; and then I have noticed that these parts appear sometimes to be far and sometimes to be near, one just as often as the other and just as energetically. But I have never been able to persuade myself that this phenomenon occurred in the main as it ought to occur according to the HERING theory; and I never should have ventured to lay the foundation of a new theory of vision on an observation made with images that are half-extinguished in this fashion. However, I admit that I may have been unskillful. Only, Mr. HERING will have to forgive me for not being able to say that I have been convinced by this "overwhelming (*zwingenden*) proof of the correctness of the theory," as he himself puts it.

(*d*) As stated on page 446, there is no trouble about explaining PANUM'S experiment showing the stereoscopic union of a pair of vertical lines in one field with a vertical line in the other field. An image of this sort is the correct optical expression of a pair of lines in space, one of which is covered by the other as seen by one eye.

(*e*) Shut one eye, and with reference to this eye alone, consider any plane that is perpendicular to the surface of the face.[1] Then the side of the plane next the nose would have to have negative depth-values, and the side next the cheek positive depth-values, and so the plane ought to appear very much inclined to the visual axis. The reason why it does not do so, Mr. HERING says, is because, thanks to the experience that has taught us how the observed plane is situated with respect to our bodies, we cause the *Kernfläche* of the visual space to turn through an octant in our apperception, and thus the correct position of the observed surface is restored again.

However, the experiment may be so modified that this avenue of escape is cut off. Cut a strip of black paper of the same width as the

[1] ¶In the original this sentence begins as follows: *Wenn man nur eine Auge öffnet und mit dem anderen allein irgendeine zur Anlitzflache senkrechte Ebene betrachtet, usw.* Apparently the translation as given above expresses what is intended.—J.P.C.S.

interpupillary distance, and hold it in front of the face, so that each eye can see only those objects in the field that are on the same side as the eye. Thus, except for a small central portion lying in the blurred image of the two edges of the strip of paper, the entire field of view will be seen monocularly. As the gaze wanders to and fro, no conflict worth mentioning will occur between the black of the paper and the bright images in the room; and there are no possible movements of the eyes that could help us in estimating the real distances of the observed objects. Nor would the difficulty in this case be overcome by turning the *Kernfläche* through an octant. Thus all the conditions in this experiment seem to me to be favourable for bringing out clearly and simply Mr. HERING's hypothetical depth-feelings; and we should expect now to see the two parts of the wall on the border between the two visual fields meeting each other at quite a small acute angle like a knife-edge turned toward the observer. (According to Mr. HERING's theory, this angle ought to be equal to the angle of convergence of the eyes.) But there is no sign of this to be seen, and the wall looks just as flat as it does when it is viewed by both eyes.

However, the other illusions, that depend on the direction of the apparently vertical meridians or on any accidental difference between the torsional rotations of the two eyes, etc., all come out clearly in this experiment. Are we to say that experience that tells us that the wall is flat destroys the one illusory sensation? Why then does the other experience that tells us that the horizontal lines on the wall are all straight and the vertical ones all parallel, of which I am continually conscious until the instant I interpose the paper screen, not destroy also the illusions due to the torsional rotation and the deviation of the meridians?

Even in the very cases where the contours of the observed images correspond perfectly to those of a real object, so that the depth-feelings are in perfect accord with the observations that may be made by movements of the eyes, just as in the case of pseudoscopic experiments, perceptions of depth will not be obtained if the shadows cast by the objects conflict with these perceptions; and yet the connection between the form of a body and the shadow it casts is certainly a matter of experience. And even when the shadows are not contradictory, and it is simply a question of remembering how the pseudoscopically observed body appeared previously, many persons will not be able to obtain the pseudoscopic impression at all without having had some previous training in observing the binocular parallax, while others can only succeed in doing so by looking for a long time and varying the direction of fixation.

These facts all go to show that Hering's depth-feelings do not act unless the factors furnished by experience also demand a perception of depth, and that the moment they come in conflict with the empirical interpretation of the visual phenomena or even with the recollection of the form of the individual object, they cease entirely. Are we not then forced to conclude that those depth-feelings, if they exist at all, are at least so weak and so vague that their influence is negligible in comparison with the factors derived from experience? and, therefore, that the apperception of depth must arise just as well *without* them as *with* them, or in opposition to them, as is supposed to happen on Hering's assumptions?

And this brings us, finally, to a last essential difficulty from which no intuition theory of space-apperception has ever escaped yet, unless it confined itself to entirely general propositions; and that is, these theories are always obliged to assume that actually existing *sensations* can be squelched by an experience showing them to be unfounded. But there is not one single authentic instance of it. In case of all illusions of the senses produced by sensations that were stimulated abnormally, the illusory sensation is never abolished by the better knowledge we have to the contrary or by our insight into the cause of the illusion. The pressure images, the luminous streamers at the place where the optic nerve enters the eye, the after-images, etc., remain where they appear to be in the field of view, just as the image in a mirror continues to be seen behind the mirror, although in the case of all these phenomena we are well aware that they have no real existence. Of course, the attention can be distracted permanently, if desired, from sensations that have no relation whatever to external objects; for example, from the sensations of the fainter after-images or of the entoptical or other objects. Moreover, in estimating their intensity, fairly large errors are liable to occur on account of contrast; or we may make a mistake in apportioning them between two objects of which they are supposed to be the common effect, as is sometimes the case with contrast phenomena. In fact, as long as the distinction between conscious conclusions and inductive conclusions was not quite clear, one of the main objections constantly urged against the empirical theory was that the illusions of the senses were *not* destroyed by an insight into their mechanism nor by experience to the contrary. What would become of our sense-preceptions, if we had the power not only of not noticing a part of them that did not fit exactly into the chain of our experiences, but of converting it into its opposite?

Consider, for instance, the case of two double images of one and the same object, both situated on the same side of the median plane. On HERING's theory, one of them excites a positive, the other a negative, sensation of depth, not of any slight amount either, but, as his theory of stereoscopic phenomena assumes, of considerable and very clearly perceptible magnitude. But because we know that the double images belong together and are images of *one* object, of whose distance we are more or less well aware, we are supposed ordinarily not to perceive the difference between their sensations of depth, even if we try to see whether one looks nearer to us than the other. But now suppose that a slight difference of colour is produced in the two images, either by previously fatiguing one eye for one colour or by illuminating it from the side; then there will be a real difference of sensation between the two double images. But even when this difference is of the minutest possible sort and may not be perceptible at all except by the aid of binocular contrast, it comes out in spite of our knowing distinctly that the two images are images of the same thing and must therefore be of the same colour, and notwithstanding that the colouring is no real colouring, but only a subjective appearance, and that we are aware of this likewise.

Then think of the whole system of localization, which, according to HERING, is given originally by direct space-sensation. After the theory has been amended and improved in all sorts of minor ways so as to adapt it better to actual conditions, the most we could ever do would be to make it give a correct localization of objects for a single position of the lines of fixation. In all the innumerable other cases it would be more or less wrong and would have to be amended by experience. Thus HERING's hypothetical assumptions possibly do make it easier to explain the visual perceptions in one single instance, by making it all the harder to explain them in every other case. And, at any rate, the conclusion must be that if the factors derived from experience are able to give the correct information as to the relations of space even in spite of opposing direct space-sensations, they must be still better and more easily able to give the correct information about them when there are no such obstacles to be overcome.[1]

[1] I have been obliged to make this criticism of Mr. E. HERING's views for the sake of the facts of the case, but I trust it will not be regarded as an expression of personal irritation on account of the attacks which he has made on my latest articles. I believe that any logical mind, starting from the point of view of an intuition theory of vision, as Mr. HERING did, will be obliged to adopt hypotheses such as are at the basis of his theory. And the reason why I have attacked his views especially is because they seemed to me to contain the clearest and most logical development of the intuition theory that is possible at present. The objections which Mr. HERING has urged against my own writings I have endeavoured to meet in this third part of my treatise, as far as they bore on the subject itself. Those objections which were merely of personal interest I have passed over without any comment, except where I had to acknowledge that Mr. HERING was right.

On the other hand, the moment we attribute all apperception of space-relations to experience, as in the empirical theory, sensation never conflicts with experience in the illusions of the senses, but the only conflict is between one induction gained under certain restricted conditions and the other induction gained under other conditions. In that case it is a contest between forces of the same kind, and we comprehend how sometimes one of them may be defeated, sometimes the other, depending on the changed conditions; or how, when the conditions remain unaltered, both may be defeated, alternately.

I frankly admit, however, that these questions under discussion are not yet altogether ready for final decision. My own attitude to them is due partly to the simplicity of the explanations that are afforded in this way, but especially to systematic considerations also; for I think it is always advisable to explain natural processes on the *least* possible number of hypotheses and on those which are as *definitely formulated* as possible. On the other hand, however, I must also add that in the course of these researches on which I have spent a large part of my life, as I acquired better and better control over the movements of my eyes and could direct my attention where I liked, I became more and more convinced that the essential phenomena in this region could not be explained by any innate nervous mechanism.

In its main features the above presentation of the subject is the same as that which I gave in a popular lecture published in 1855. It differs in some ways from the more recent works which have been also based on an empirical theory of vision. Thus with reference to the measurement not only of the space-relations on the visual globe but also of the distance of the observed object, I have not put so much stress on the muscular feelings as WUNDT does, because, for the reasons which I have given, I think they must be regarded as quite inaccurate and variable. On the contrary, my method consisted in obtaining the main measurements on the visual globe by making different images fall on the same parts of the retina. WUNDT, in particular, has made a very exhaustive study of the relevant psychic phenomena, for which we are much indebted to him. I have called attention to some special observations of his where I differ with him.

A. NAGEL accounted for the production of binocular images by assuming that the retinal images were projected outside on two different spheres. The centre of each sphere was taken at the centre of the entrance-pupil of the eye to which it belonged (where the lines of sight all meet), and the two surfaces were supposed to intersect each other at the point of fixation. Then any point that did not happen to be on the curve of intersecton of the two spheres would properly have to be seen in double images. Now NAGEL supposed that these projections were viewed from the point midway between the centres of the two eyes, and the way they appeared then, that is, coincident, crossed or uncrossed, was the way they were supposed to look in the field of view also.

NAGEL'S theory, it must be admitted, is pretty close to the facts; but, in the first place, it is a little artificial to assume a twofold projection;

secondly, the apperception of a different distance for the two images, which is usually required by this theory, as a matter of fact does not occur; and, lastly, the positions of the images that are seen single, as given by this theory, are not always exactly in accordance with the facts. Incidentally, this is, perhaps, the only essential point of difference between NAGEL's theory and the one which I have given above.

On the other hand, the correct theory of double images and their positions was given by A. CLASSEN, although in doing so he made the mistake of denying the *de facto* correctness of the phenomena adduced by HERING which have reference to the centre of the direction-lines midway between the two eyes. Indeed, I myself am just as little disposed as Mr. CLASSEN to make this phenomenon the basis of all our localizations; I regard it simply as an approximate illusion of the senses, which is produced in different degrees in each of my own eyes, and can be overcome by increased attention. But it is an illusion that really does exist.

A difference between my method and CLASSEN's, which is really more important, is that he considers the sense of locality of the retina and the projection on the visual globe as being innate and not acquired. But if the positions of the individual retinal points with respect to each other are given by an intuitive sensation, the identity of corresponding points is intuitive also, because the fact of their being equally situated with respect to the point of fixation must likewise be included originally in the sensation in this case. However, this difference does not affect those chapters in which CLASSEN discusses vision expressly, especially the theory of the muscular sense and of binocular vision; and here a great many interesting illustrations from pathological observation are adduced in support of his physiological theories.

The views of other exponents of the empirical theory, such as H. MEYER, DONDERS, VOLKMANN, and A. FICK, with reference to various parts of the theory, have each been discussed in its proper place.

EXPOSITIONS OF THE EMPIRICAL THEORY

1855. HELMHOLTZ, *Über das Sehen des Menschen*. A popular scientific lecture delivered at Königsberg i. Pr., in honour of KANT. Leipzig, L. Voss.

1861. A. NAGEL, *Das Sehen mit zwei Augen und die Lehre von den identischen Netzhautstellen*. Leipzig and Heidelberg.

1862. W. WUNDT, *Beiträge zur Theorie der Sinneswahrnehmung*. Leipzig and Heidelberg.

1863. A. CLASSEN, *Das Schlussverfahren des Sehaktes*. Rostock.

1864. A. FICK, *Lehrbuch der Anatomie und Physiologie der Sinnesorgane*. Lahr. Heft 2.

— W. WUNDT, *Vorlesungen über Menschen- und Tierseele*. Leipzig, L. Voss. (In two volumes.)

Appendix to Volume III

by

J. v. Kries

I.

Concerning the Spatial Configuration in Vision; with Special Reference to the Questions of Innate Dispositions and Experience.

1. As to the Nature of the Idea of Space

Numerous and interesting as are the additions which have been made to our actual knowledge of the perceptions of vision in comparatively recent years, still, as was stated in the preface of this new edition of HELMHOLTZ's treatise, none of them is of such paramount importance as to shake, much less to upset, our fundamental theoretical convictions. HELMHOLTZ's classical work, published more than forty years ago, was based partly on philosophical considerations, partly on comparatively simple results of direct self-observation, and partly, too, it should be added, on a vast amount of empirical observation in the ordinary sense. But even in this latter respect, in spite of many new facts that have been gleaned and some corrections that have to be made here and there, the material contained in the first edition of the Physiological Optics may still be said to be essentially correct and pertinent. Even at the present time, therefore, HELMHOLTZ's method of treatment of the perceptions of vision appears to be still thoroughly possible and permissible. Perhaps, with some slight modifications, it could be adopted today by a writer with a certain temperament and type of mind and general philosophical convictions.

Consequently, it might seem that nothing more would be necessary except to collect everything that had actually been discovered in the meantime and insert it in its proper place in HELMHOLTZ's systematic treatise; as indeed has been done to a great extent in the Notes which have been added at the ends of the various chapters. But there were several reasons, perhaps, why it did not seem advisable to limit the revision in this way. In the first place, it was incumbent to bring out in detail the important bearing of various recently discovered facts on theoretical questions (facts concerning anatomical relations, strabismus

etc.), particularly since, in my opinion, a wrong significance has frequently been attached to them by regarding them in another way. Besides, while I believe that HELMHOLTZ's whole attitude toward the question of the perceptions of vision is one which might be adopted even at present, still, partly on account of certain particular facts and partly owing to certain changes in our general conceptions, one cannot fail to admit that some modifications of one sort or another in HELMHOLTZ's theory might possibly be made without changing it in its real essentials. The truth is, we may as well say at once, there were certain peculiarities in HELMHOLTZ's way of thinking that had nothing whatever to do with physiological optics itself, but yet aroused widespread opposition in spite of gaining also many adherents; and this antagonism has stood very much in the way of a proper appreciation of his theory in its applications to physiological optics. This is another reason therefore why it might be desirable to distinguish between the theory itself and those other matters of controversy.

Accordingly, it seemed necessary to supplement HELMHOLTZ's work by a new treatment of these very fundamental questions, and here the only thing for the editor to do was to be thoroughly independent and guided by his own convictions. So far as preserving the unity of the work was concerned, it is needless to say that the person best fitted for this task would have been one whose scientific opinions on this subject were most closely in accord with HELMHOLTZ's own views. I must confess at once that this does not altogether apply to me. On the contrary, almost as soon as I began to study these questions, I reached conclusions at variance with HELMHOLTZ's theory in some fundamental respects, particularly as to the psychological nature of the idea of space. This made it much harder for me to undertake the task of revising the theory of the perceptions of vision in this new edition; and yet I was induced to do so partly for the simple reason that it was doubtful whether any editor could have been found who would not have had to contend with just as great difficulties of the same or of a different kind. But there was another motive that influenced me also; for after all the importance of the fundamental differences to which I have referred is perhaps mainly a matter of psychology and logic, whereas, so far as the physiological questions are concerned that are uppermost here, these differences are of less weight.

However, a brief preliminary discussion of these fundamental questions is unavoidable, as it will make it clear at once that, while the conclusions I have reached are necessarily at the basis of my whole treatment of the subject, nevertheless, so far from prejudicing the questions that relate to physiological optics itself, they afford scope for the widest divergence of opinions in regard to them.

The first point to be considered is with reference to the general psychological nature of the idea of space. My opinion is that any discussion of these questions must start, first of all, with the fact that, *the idea of space as such forms a unitary and invariable part of our consciousness.* This is shown fundamentally by the simple fact that in any perception of the senses which is coördinated as to space a distinction is made between what is perceived at any place and the place itself. We can imagine something else (or nothing at all) as being in the same place, without any change being involved in the space as such. This is just what KANT had in mind when he said:

"We cannot conceive of the absence of space, although it is perfectly easy to imagine that no objects are encountered in it."

Of course, this fact does not deny, for instance, that we never do have any optical sensations that are not in spatial arrangement, and that there is no such thing as non-spatial vision, or that we do not know a visual sensation that does not involve space-apperception. All it says is, that *space represents something that can be lifted out of the optical perception as a special thing and as something that remains constant throughout all changes of the thing perceived.*[1]

However, it would not be correct to assert that we cannot form an idea of space without some material for sensation by the senses. It may, indeed, be true that we are not able to form an idea of definite geometrical forms, figures, bodies, or, as we can say more generally still, individual *places*, without thinking of them as delineated and marked off from their environment by some modality of sensation (however shadowy the idea may be); and that, therefore, when we are asked to think of a triangle, a sphere, or a point directly in front of us, we always imagine something that is seen, something connected with optical qualities. It is idle to dispute about this, when that mysterious "rapping" ("*Anklingen*") of the optical sensation itself is something hard to understand physiologically and is not at all clear. But if it is granted that the optical sensations do participate in some way in every idea of space-forms, the fact ought to be expressly emphasized all the more that it is only the emergence of individual places, but not

[1] In reference to these relations, here at the very beginning of the subject, I have my doubts as to whether the name *sensation* should be applied to the absolutely peculiar way in which space and time are given in our consciousness. I avoid the term space-apperception (*Raumanschauung*) used by KANT, because not only HELMHOLTZ, but many recent writers also have employed it in a sense wholly different from KANT's usage, that is, to connote the totality of a definite spatially arranged perception. Consequently, to convey the meaning that is intended here (namely, that which is consistently exhibited in all our perceptions), it might be better to use the expression *space-idea* (*Raumvorstellung*), which does not pretend, of course, to state anything special as to the (thoroughly specific) mode by which the idea of space is given in our consciousness.

the idea of space as such, that is thus connected with the optical sensation. This is brought out in the most positive way by the fact that even in ordinary optical perception particular parts of space, as being the places of the things seen, do, indeed, exhibit that connection with what is optically sensible (*mit dem Optisch-Sinnlichen*), whereas everything else is lacking in this sensory determination, above all the intervals in between those places. When we see a green object lying at a certain distance away, we can properly say, at least with regard to the distant point where the object is perceived, that there is a connection between green sensation and space-determination. But we also have an idea of the whole interval comprised between us and the point in question, and yet there is nothing about it that is optically sensible. The same thing is true with respect to the interval between two points lying side by side or one above the other and viewed against a distant background. Thus the variable element in space is not simply the thing that is seen at the particular place, but also the parts of it that are brought into relief by some sensible token, whereas space as a whole remains the same.

Another thing suggested by the matter of which we have just been speaking is the *oneness* of the idea of space, which is something that requires special attention also. If the significance of the sensory material always consists in its outlining some given portion of space as distinguished from all the rest, it implies also that the idea of an individual place all by itself is something absolutely inconceivable, and that the place can never be imagined except in combination with space as a whole. It is of the utmost importance to have the correct conception of this matter also. The fundamental difference between spatiality and all varieties of sensation is that in the latter the individual thing is something that can be imagined by itself as capable of existence. This is true of all (intensive or qualitative) series of sensations, no matter how accustomed we are to picture as a whole the totality embraced by them. The tone which we happen to hear at the moment may seem to be one of the familiar notes of the musical scale; and yet it must be acknowledged that this way of connecting the individual sensation with the ones that differ from it is not involved in it as something absolutely indispensable in the same manner as the idea of locality is dependent on the idea of an environment within which it is supposed to lie, that is, is dependent on the idea of space as such.

Accordingly, while the idea of space appears always as something unitary, this statement requires to be understood with certain limitations. Certainly nobody would maintain that we always imagined

the totality of space in all its infinitude in every direction, at any rate not with equal distinctness. On the contrary, another characteristic peculiarity of the idea of space is the fact that the idea of each individual part as not being cut off from the rest reveals the possibility of an endless continuation, and therefore includes within it the idea of infinite space *in nuce* in a thoroughly peculiar way, which can only be explained by referring to the perfectly similar relations in the ideas of time and number.

Consequently, I am convinced that a careful and unprejudiced consideration of the psychological relations that can be observed at any time will enable us to see that the idea of space in KANT's sense is a unitary and invariable element in our consciousness. In a certain respect this conclusion is contrary to HELMHOLTZ's views; but, as already intimated, the significance of this difference is mainly in the field of psychology, especially of logic. On the other hand, the attitude taken here does not affect those questions which I believe can be treated separately because they are peculiarly physiological.[1]

A single word can easily be used to express the particular thing we have in mind here. If the idea of space is regarded also as something unchangeable and unitary with which we are endowed once for all, and if, admittedly, our visual sensations especially come to consciousness always in spatial arrangement, obviously a perfectly general question would be to ask, At *what* places in space and in *what* spatial arrangement does the individual object of perception appear to us to be? that is, the question, not as to the origin of the idea of space as such, but as to the special conditions of the *localization*.

Needless to say, the complete answer to this question would involve a very thorough and extensive experimental, physiological investigation; and it could be answered in the most diverse ways without being inconsistent with the fundamental nature of the idea of space as presented here. For instance, we might adopt the empirical view of the problem, as advocated by HELMHOLTZ, or the opposite view of the intuition theory; for evidently there does not seem to be any *a priori* reason why we could not just as well assume that these relations were established by innate dispositions as that they were a result of experience, learning and practice, especially as neither assumption is incompatible with the nature of the idea of space insisted on here.

Another point where we must part company with HELMHOLTZ's theory at the very outset, so to speak, is in regard to exactly what is meant by this learning by experience or practice.

[1] Accordingly, we may, and in fact we must, omit any discussion of the numerous essays and theories that begin by trying to analyze the idea of space as such and seeking to discover its origin.

As is well-known, these processes were regarded by HELMHOLTZ as psychic. He speaks of them specifically in this way, sometimes contrasting this assumption directly with a physiological interpretation. If we take the position that *all* phenomena of consciousness are correlated to material processes of some kind in the brain and have their substrata in them—a position which, in spite of many differences of opinion as to the special nature of that parallelism, is shared perhaps by most natural philosophers at present—it might seem at first sight as if HELMHOLTZ's view of the origin of the laws of localization would be ruled out on fundamental principles or at least would not have any leg to stand on. However, on studying the matter more closely, we find that this is not the case at all; for what we mean by learning is primarily a process with which we are very familiar from a great variety of experiences and which is indeed characterized at first on its psychological side. The fact that we are led to imagine that some kind of material substratum is at the bottom of it, does not alter the meaning that is attached to it. Nor will any materialistic view of this process of learning ever alter in the least the fact that we are concerned here with certain modes of the accomplished result which are characterized as something special both by their psychic manifestation and by their material substrata. The contrast will always exist between them and the developments of the organism as determined by the general laws of growth, and the most that any materialistic conception of learning could possibly do would be to enable us to imagine the disappearance of the line of demarcation between learning and development by growth.

Thus the question as to whether localization is acquired is completely independent of any special theory of learning and retains its importance so far as the main fact is concerned, no matter whether we regard learning as a psychic process, or whether we postulate a material substratum for everything psychic and think of learning as being a development of nervous connections, formations, etc. A physiological view of learning is nowadays generally taken for granted in science, and in my opinion is indispensable considering the whole state of our knowledge at present; and so I consider that this is the second modification that requires to be made in HELMHOLTZ's theory. So far as the particular subject is concerned in which we are interested, this modification is of even less importance than the other one mentioned first. If learning is considered as being a physiological process, we are at liberty still to give either an affirmative answer or a negative one to the question as to whether the relations of localization are acquired. Accordingly, from this standpoint also, it is just as possible to hold an

opinion that is the same as HELMHOLTZ's in the main as to entertain the opposite view.

While the relations of localization as they exist normally in the adult human being may be considered as having been explained and established by facts that have long been known, the *nature* and *origin of these laws of localization* have continued to be a subject of much controversy. The empirical theory developed by HELMHOLTZ in his day has especially met with much opposition. These are the questions that will be discussed in the following pages.

The facts involved that will have to be considered fall naturally into several groups, which will be the basis of the division of the subject. The laws of localization, as we find them realized and observed by grown persons under normal conditions, will be considered first. Although the facts concerning them are the easiest both to observe and to verify, they have only an indirect bearing on our particular questions at present, and are the most difficult to estimate for our purpose. While we shall not endeavour to keep in mind here any development-relations whatever, possibly the special nature of the relationship existing in this case may give us some hints as to their mode of origin and enable us to make some conjectures. The *second* group of facts will comprise those relating to the *change* produced in those normal relationships under special conditions of any sort (including especially pathological conditions). Obviously, these facts will be of much interest on account of their direct bearing on our subject; for if it is found that under changed conditions relationships of any kind can be destroyed or modified, we shall be justified in assuming that the maintenance of these relations in ordinary circumstances is dependent on normal functioning and bound up with it; and this will mean that it is at least likely that the functioning has something to do with their development. We know, for instance, that this was the reason why HELMHOLTZ devoted special attention to the conditions in the case of cross-eyed vision and considered them of particular theoretical importance.

Lastly, a third division should comprise all those facts, which, if they could be ascertained, would at once settle the question under discussion; namely, the facts concerning the vision of newborn babes. Indeed, if we could be sure what a baby sees, and how it sees it, and if at the same time we could watch how vision is perfected and developed, perhaps, we could positively tell the significance of practice or learning in this case. Needless to say, the difficulties about ascertaining these facts are well-nigh insuperable. A substitute is afforded to a certain extent by making observations on persons who, being blind from birth, have been operated on to enable them to see; but it is a very imperfect substitute at best, and the observations have

to be interpreted with caution. Helmholtz has gone into this question at considerable length in the text and at least a brief discussion of some of the more recent observations should be included here.

A rather more thorough discussion will be required in the case of certain general relations which we discover as soon as we begin to think of the processes of learning and training from the standpoint of cerebral formations. An entirely general consideration of these relations is important for enabling us to judge properly the questions of localization. And, finally, on the strength of this foundation, an effort will be made to form some idea of the full significance, so far as vision is concerned, that should be attached to innate dispositions on the one hand, and to development by experience on the other hand.

2. Concerning the Relations of Normal Localization

Without referring at first to the more precise quantitative relations, we may say that the laws of normal optical localization have now been so surely established and are so consistent in every way that there are scarcely any new facts to be noted. But perhaps it will be appropriate to say something here in regard to their generally admitted characteristics. The main point that ought to be emphasized is the fact that the impression of the *direction* in which the observed object appears to be is determined by certain physiological factors, whereas the impression of the *distance* of the object is dependent on entirely different factors. Thus, in the first place, a distinction can be made between localization of direction and localization of distance. In connection with the relations of perception of direction, another matter that needs to be emphasized is that perception in a definite direction implies localization on a straight line proceeding from the observer's body; and, therefore, in particular, there is no distinction between the impressions in the two eyes in the sense that we can say that a space-determination made by one eye is referred to that particular eye as distinguished from the other eye. The fact is rather that the perceived direction is something perfectly unitary and no distinction between the two eyes of any sort is involved in such a space-determination. I venture to call this characteristic a *synchysis* [or fusion] of the impressions in the two eyes. The best way of understanding the peculiar significance in this mode of localization is to think of some other kind of localization that differs from it, such as that, for example, obtained by touching something with both hands; for it is possible to conceive of something of this sort in case of the two eyes. The thing that would be characteristic in the latter case is that the place-determination would depend on two elements, namely,

on the position of the observed object relative to each eye, in a manner which would correspond, say, to the space-relations that actually do exist. In a localization of this sort, therefore, the impressions in the two eyes would be differentiated from each other and would produce their effects according to the position of the observed object with respect to each eye separately and the mutual position of the two eyes with respect to each other.

The fundamental characteristic, therefore, of the localization that actually is realized, this synchysis, as we have termed it, is that the localization is *not* such as that just mentioned in which a difference is made between the impressions in the two eyes. It comprises rather (in the perceived directions) a determination belonging absolutely identically to the impressions in the right eye only and to those in the left eye only; and so in ordinary binocular vision this determination is not the result of a mechanism involving the two eyes in some unequal manner, but it is given immediately by the determinations belonging to the monocular impressions also and is identical with them.

The simplest way of seeing the consequences of the synchytic combination is to assume that all directions are referred to one and the same point, lying about midway between the two eyes (the so-called centre of visual directions); and to assume, moreover, that the observed directions depend on the places on the retina where the images are, according to the well-known laws, and that the relations of correspondence (equality of direction) between the two eyes are also determined in the usual way. On these assumptions, which at any rate are very nearly realized, the result is the rule of the cyclopean eye. Then we shall also be able to explain the familiar discrepancies between the arrangements in space as perceived and those that actually do exist, especially the phenomena of diplopia, which are usually, and rightly, regarded as affording the proof of the other kind of localization. However, perhaps we ought to add that in this case other conditions are involved which may or may not be always strictly and absolutely realized (we never can tell) and which therefore ought to be kept separate from the fact of synchysis at least so far as our discussion is concerned. Thus the centre of visual directions does not have to be placed exactly midway between the two eyes. It might very well be nearer one eye or the other, and its position could also be variable more or less; in fact, its position could alter a little for the various parts of the field of view. Without detriment, therefore, to the synchytic nature of the localization, the way the visual impressions are referred to the observer's body might easily fluctuate a little in a number of special details. Besides, it is well to note that this synchysis does not imply at all any hard-and-fast relation of correspondence.

All it means is that the impressions of direction in the two eyes are in harmony with each other. Whether a given point on the retina of one eye is always co-directional (*richtungsleich*) with a precise point on that of the other eye, may be left undecided for the present.[1]

Although HELMHOLTZ himself made the unitary centre of the visual directions the basis of the laws of normal localization, doubtless, it was the suggestion of these possibilities of variation that made him reiterate again and again that he was not disposed to make this relation the cardinal point of the theory of localization. The same consideration also may have had something to do with the fact that HERING, who laid special emphasis on the fundamental importance of that relation, in opposition to the utterly different views of such authorities especially as A. NAGEL and DONDERS, always connected it with the assumption of absolutely fixed innate relations of identity.

However, it should be added, this did not involve any difference of opinion as to normal vision under ordinary conditions. In fact, this kind of vision was represented by HELMHOLTZ himself in terms of the well-known rule for the cyclopean eye, in a form which is in complete agreement with HERING's views. It has been generally agreed for a long time that it fits the facts, and the synchytic union or fusion is made perfectly clear in it.

The various questions involved here are hard to keep separate, owing to the fact that the term *projection theory* (which was not a very happy choice to begin with) is used in so many various ways that the meaning is not always plain.

It implied originally that the place where an object was seen was determined by the intersection of a pair of straight lines drawn from the retinal image of each eye through the corresponding nodal point. In fact, the retinal images in this case may be said to be projected outward. However, the main question with us at present, which is the special point of the theory that was disputed and found to be unsatisfactory, is not this outward projection. The question is not so much as to the projection itself (although that in itself was an inappropriate expression in this particular connection), but rather as to the direction being referred independently to each eye separately. It should therefore be called a theory of *bicentric* projection; in which case the view so strenuously insisted on by HERING, and, by the way, just as strongly advocated by HELMHOLTZ, might be called, in contrast with it, a theory of *unicentric* projection. But it is utterly misleading to draw a distinction between the projection of retinal images and the nativistic assumption of innate place-values. For the term projection never has been used anyhow except to denote the directions in which the adult actually does see. It is

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[1] In this way of looking at the matter the only strictly valid rule that can be given for the inaccuracies of perception is that, if A_1, A_2 designate two retinal points of the right and left eye, respectively, which (under the conditions given at the time) produce the same impressions of direction, all the points lying on the two lines of sight drawn through A_1 and A_2 (or immediately adjacent to these lines) will be seen on a single straight line (or very near it).

evident also that there is no possible way of describing these (subjective) visual directions except by saying that they are in agreement with some line whose position is objectively defined with reference to the observer's body. Thus not even the advocates of innate space-values have ever disputed the fact that the vision of the adult occurs in definite directions drawn with respect to his body; and while they may speak of altitude-values and azimuth-values, they do not imply thereby any definite distance from the ground plane or from the median plane, but simply a definite direction. This is shown at once by the fact that points may be at unequal distances, and yet agree as to the aforesaid values. Accordingly, when these authors in speaking of adult vision use the terms altitude-value and azimuth-value, they are referring to a *direction*. HERING, for instance, constantly speaks of visual directions, a centre of visual directions, etc.

Therefore, there cannot be any question as to its being perfectly correct to call a theory a projection theory which maintains that vision is an act that takes place in directions referred to a centre and that these visual directions contribute to determine the place of vision. HELMHOLTZ and HERING alike recognized this fact in their different ways of regarding normal vision. But the term projection certainly cannot be used to denote a theory as to how these visual directions originated or have been developed. That would be sheer nonsense, something certainly never asserted by anybody. It is absurd to contrast it with the theory of innate space-values.

Lastly, it is utterly misleading to take for granted that the projection theory is a theory of a bicentric projection, and then contrast it with the theory of innate place-values, in order to prove that the former is right because the latter is wrong. This means confusing together two things that are absolutely different from each other, and leaving out of account entirely the very view that HELMHOLTZ adopted.

If we proceed to inquire how the directions are determined in which the various objects are perceived, there is no doubt as to how this happens in the main, as has been already intimated. On the one hand, these directions are determined by the place where the given luminous stimulus acts on the retina, and, on the other hand, by the position (or adjustment) of the eyes at the time. For instance, when our eyes are turned up or down or to the right or the left, we see whatever we are looking at approximately, at least, in its correct place, namely, up or down or to the right or the left, respectively. It is desirable to have some precise name for this factor that contributes to detemine the visual direction of each retinal place; and therefore I shall call it the *adjustment-factor* (*Stellungsfaktor*) of the visual directions.[1] As to the conditions by which it is determined, HELMHOLTZ has shown in the text that it is not so much a question of the positions actually assumed by the eyes, that is, of the central impulse of innervation. But it is worth noting that the main thing that determines this adjustment-factor is not the state of the motor-mechanism alone, but that ouf optical perceptions themselves, and especially the coöperation or

[1] ¶See HOFFELER, Über den Stellungsfaktor der Sehrichtungen. *Zft. f. Psychol.*, 66 (1913), 249-262. (J. P. C. S.)

the parts of our own body, contribute to determine it. This is proved especially by E. Fick's interesting observations.[1]

An important thing to be stressed therefore is the fact that normally this adjustment-factor represents a *unitary* factor for both eyes. Thus, exactly as we do not distinguish between the directions of the impressions perceived by the two eyes by referring them to two different origins, so likewise we are not conscious in our vision of the position or innervation of one eye alone. It is always simply *one* adjustment-factor that helps to determine all the visual directions, as is shown in the simplest way by the fact that the visual directions of corresponding places on the two retinas are always the same, no matter what the nature of the adjustment-factor may be.

Simple as these facts are, they involve something that deserves particular attention for the questions which we have under consideration at present; and that is the characteristic way in which these absolutely heterogeneous things, namely, first, the objective retinal place where the stimulus acts, and, second, the adjustment-factor of the eyes, combine to produce a result that comes to consciousness as a unitary and finished affair. For instance, the following observation will make clear what is meant by this. Consider an object O_1 directly in front of you about on the level of the eyes; after gazing steadily at it, raise your eyes so as to look at a higher object O_2, in which case O_1 will be seen indirectly; but you do not get the impression that the position of O_1 has been changed; on the contrary, it will appear to be exactly in the same place both before and after raising the eyes. An untrained observer will be certain to describe the result by saying that the place seen or the space-determination of the observed object has remained precisely as it was at first. The perceived direction (or, if we prefer to say so, the altitude and azimuth of the observed object) is determined by the two factors above mentioned acting together in such manner indeed that we get the same direction-value for one retinal place and one adjustment of the eyes as for another retinal place and another adjustment.

It might be suggested that possibly the place on the retina responds at once and produces its effect, because the distance from the point of fixation corresponds to it as correlative in the sensation; but careful self-observation will show that this is not the fact. Gaze steadfastly at a point F, giving heed at the same time to a point E seen excentrically. Now let your eyes turn to look at some other point F', and although you may indeed try to find the place that has the same position with respect to F' as E has with reference to F, undoubtedly

[1] E. Fick, Über die Projektion der Netzhautbilder nach aussen. *Zeitschrift fur Psychologie.* 29. 1905. p. 122.

you will not get the impression of something that is in agreement between the first impression and the second in the same way that you did when the place stayed the same. It takes conscious reflection to enable us to derive the idea of equality of position with respect to the point of fixation from the idea that is obtained immediately.

If, therefore, we wish to designate a space-determination of our optical sensations which comes to consciousness as such, and which can be recognized as something that is common to our various optical impressions, unquestionably the only thing that can answer this description is this visual direction referred to the observer's body that is determined by the retinal place and the adjustment-factor.

Granted, therefore, that in the unitariness and immediacy with which the place of the thing seen enters the consciousness it does seem to be perfectly on a par with the qualities of sensation; at the same time the more special mode by which this place is determined would perhaps seem to indicate that its being dependent on two entirely dissimilar factors, which coöperate to produce this impression, is the result of a physiological mechanism of a wholly different sort from anything of the kind with which we are familiar and which we ordinarily mean when we speak of determination by means of the sensations in the narrow sense.

Statements very similar to the above would result from studying the subject of depth-localization. Simply taking for granted the generally known and accepted facts in regard to it, we can say that the especially remarkable thing in this case is the fact that the determination of the distance of an observed object is the result of an unusually complicated interaction of a whole series of factors of the most various kinds. The apparent size of objects of known absolute dimensions, the way the contours run, the relations of light and shade, aerial perspective, and, finally, all the conditions of binocular perception of distance every single one of these factors may be, and sometimes actually is, involved in the determination of the distance of the thing seen. Moreover, on the whole there is pretty general agreement as to the particular way in which the observed distances are determined by these factors. HERING, especially, ascribed definite depth-values to the various places on the retina, but we must remember that they were not supposed to signify the impression of distance that accompanies the stimulation of a definite place on the retina under all circumstances. The principal meaning of these values was that they become effective in case of binocular perception of objects. Then the depth-values of the two retinal places where the images were formed of the same object were supposed to be added together so as to produce

an idea of distance which was at any rate qualitatively in conformity with what was actually observed and with what the distance was taken to be by all other observers also. At the same time it was recognized that the "empirical factors" in monocular vision may outweigh these depth-values and suppress them. The only case, therefore, in which there can possibly be any differences of opinion as to the facts of perception that are capable of direct verification is the single case of monocular perception where the empirical factors are excluded entirely; and this case will have to be considered presently more in detail.

The relations of binocular perception of depth need to be discussed first. As has just been stated, there is no divergence of opinion as to the actual facts themselves; and hence these relations may be described by saying that when nearly or absolutely concordant images in the two eyes are united in the idea of a unitary object, the latter will be seen closer or farther than the position of the point of fixation according to the sign of the "cross-disparity" [see page 381]. The production of a definite impression of distance will depend, therefore, on whether the impressions made on two definite places in the right and left eyes fuse into the idea of the same object. But now what is it that determines *whether* this happens? Every attempt to define these conditions more precisely merely shows how unusually complicated they are. The only thing that stands out as being decisive in this respect is the similarity of procedure of the contours. And yet it is plain that this is only a partially satisfactory way of accounting for the conditions of binocular union perhaps in the simplest cases. If there is a black line passing through the point of fixation in the field of view of each eye that deviates a little from the vertical, the deviation being clockwise in the left eye and counter-clockwise in the right eye, we may say, perhaps, that these two images will inevitably give the impression of an object with its upper end inclined toward the observer. But all we have to do is to suppose that the angle between the two images is gradually increased until presently we notice that beyond a certain limit we cease to have that impression any longer, and instead of it we see two objects crossing each other. There is scarcely any observer who would say that it was possible to assign a perfectly definite value for the limiting angle in this case. The truth is rather that there is undoubtedly a region within which the union may take place but does not have to do so; that is, its occurrence is not determined simply by the way the lines pass through the point of fixation, but depends on some other conditions also. Moreover, instead of a single line passing through the point of fixation of each eye, we can imagine several such lines in various directions. Then under proper conditions

each impression in one eye will be united with a definite impression in the other eye; and we might want to know what is the reason for the particular choice of combination. As a general rule, when the two eyes are simply exposed to star-like groups of radiating lines, this combination will be irregular and unstable. But if, exactly as is the case with real material objects, certain pairs of lines are interrupted or bent, etc., at some little distance from the point of fixation, they will be regarded in a perfectly fixed and definite way. It appears, therefore, that the fusion of a pair of points in the two eyes does not depend only on the images at the points themselves or at the immediately adjacent places, but the impressions of the entire field of view contribute to produce this result in some exceedingly complicated fashion. If we wish to define this relationship more precisely, all we can say is that if a union is possible that will give the impression of a unitary material object of a kind to which we are used and with which we are familiar, this combination will generally occur; whereas other combinations, which would likewise be possible so far as the local contours were concerned, but which would yield a number of more or less incoherent forms, will not occur. There is also another point that has to be considered here. For instance, it is a well-known fact that, in looking at a complicated stereoscopic picture, we are not apt to get the correct impression of solid material form simply by binocular fixation of corresponding points in the two views. This impression is rather the result of long-continued inspection. Some little time is required for this condition of stereoscopic fusion to become adjusted. We can define it only in a psychological way at present by saying that the observer must *know* what the object is; and frequently, when the stereoscopic pictures are complicated (or, for that matter, if he happens to be looking at a complicated material object), it takes quite an appreciable time for him to do this.

Just here it will be well to say something again about the matter of the equivalence between psychological terminology and physiological terminology. The conditions which we have been led to formulate here have been expressed at first in terms of a psychological conception. However, this does not prevent us from thinking of them as definite physiological modes of behaviour; although whether we regard this as something conceivable or probable or even self-evident, will depend on the general psycho-physical views we happen to entertain. But the attempt to define these relations would involve our entering the perilous region of purely hypothetical assumptions; as, for instance, if we should venture to speak of the establishment of conducting links between the central effective localities of definite retinal places in the two eyes. Therefore, it is simply because these conditions are known to some extent merely by their psychological significance that we are obliged to connote and describe them in terms of psychological concepts.

In order, therefore, to form a proper estimate of the factors involved in the binocular perception of distance, in the first place (by way of summarizing what has been stated above) let us note that it depends on exceedingly complicated conditions, so combined that the impressions in both eyes are fused in the idea of a unitary object. At present these conditions can be expressed only in psychological terms, although there cannot be any doubt about their having a definite physiological significance.

Aside from the conditions of binocular fusion, the relations of localization of distance require to be discussed more in detail in some other respects.

The first question to be considered in this connection is the relation of binocular perception of depth to the numerous other so-called empirical factors that determine the impressions of distance. In exact analogy with what has been already said concerning the determination of the impression of direction by the retinal place and the adjustment of the eyes, here also the first thing that may be noted is that the impression of the definite distance of an observed object enters our consciousness as something unitary and finished, without our noticing how the various circumstances are concerned in its production; and (connected with this fact) that, although the impressions of distance are obtained in wholly different ways, they are perfectly homogeneous as such. If a near object is fixated first, and the eye directed then to an object farther away in a slightly different direction, the modality of the perception of distance for the near object will be altered. It was seen at first without cross-disparity, and consequently the impression of distance had to be determined by considerations that were essentially empirical. But when the distant object was focused, the distance at which the near object was seen then depended partly on the distance-value of the *fixated* point and partly on the cross-disparity that belongs now to the nearer one of the two points.

And yet in passing from one of these perceptions to the other, we do not get at all the impression of change of any kind. It would be truer to say that what we see is that the object apparently stays where it was, its space-determinations being exactly the same as before.

Moreover, if in observing complicated objects at various distances we close one eye so as to cut off entirely binocular perception of depth, generally there will not be any impression then of any manifest or distinct change. Thus even monocular vision, which is confined to empirical factors, may be capable of producing exactly the same impressions of distance as binocular vision, although perhaps not so independently. At any rate it is capable of maintaining them, once they have been developed.

Lastly, we come across phenomena worth considering when we begin to examine the quantitative relations of depth-localization.

If we want to specify a definite physiological factor for the binocular perception of depth, connected with it by a fixed quantitative relation, we must utilize for this purpose the lack of exact correspondence between two retinal images of the same object that contributes to a perception of depth; that is, the *cross-disparity*, as HERING calls it. The impression of depth with reference to the point of fixation will be positive or negative according as the sign of the cross-disparity is positive or negative; and it will be greater in value or less according as the value of the cross-disparity is greater or less, respectively. Accordingly, if we seek to determine the more precise quantitative relationship between cross-disparity and depth of distance (measured from the point of fixation), we are confronted with the fact already mentioned, namely, that there is no such relationship, or at least not in the sense that a given cross-disparity, say, always produces the impression of a definite depth-distance which will be invariably the same. We saw that a relationship of this kind would lead to the grossest illusions and certainly does not exist.

On the contrary, the result of investigation seems to indicate that it is generally impossible to connect a definite value of the perceived distance with a given cross-disparity as a result that occurs regularly under all circumstances. A given cross-disparity may produce the impression of great difference of depth when the point of fixation is very far away or the impression of slight difference of depth when the point of fixation is near by, irrespective of the conditions on which the localization of the depth of the point of fixation itself depends. As soon, therefore, as the quantitative relations are taken into account, we are compelled to admit that the cross-disparity by itself does not afford the impression of a definite difference of depth, but that along with it there are always involved the manifold details that go to determine the distance of the point of fixation.

Lastly, as we have seen in certain cases at any rate, not only is it very probable that the empirical factors have some influence on the binocular perception of distance, so far as the perception of the point of fixation is determined by them, but it is also very likely that, for a given perception of the point of fixation, distant objects will be seen binocularly and therefore simultaneously in a manner which is more precisely determined quantitatively by these empirical factors (see page 393).

Hence, surveying this whole question of depth-localization, we are led to repeat what has been previously stated in regard to the localization of direction; only, it applies here with much greater force.

The great variety of the conditions on which the impression of depth depends, the complex and variable manner in which these conditions coöperate, indicate a (physiological or psychological) mechanism which at all events must be wholly different from the mechanism for qualities of sensation (in the narrow sense), and which undoubtedly is formed empirically to some extent. However, here also the result, namely, a definite impression of depth, enters the consciousness with all the immediacy of a sensation as something that is given by itself and complete. Accordingly, the examination of the laws of localization in normal ordinary vision leads us likewise to relations of a very peculiar kind, which, owing to these peculiarities, have an important bearing in my judgment on the theoretical questions under discussion. They will have to be considered again from this point of view.

3. On Changes of Localization for Anomalous Adjustments of the Eyes

As was intimated above, it would be natural to expect that we might obtain some especially valuable information concerning the physiological relations of localization if we could succeed in ascertaining whether associations existing under normal conditions are capable of any variations; and if so, how and under what circumstances these variations occur. By far the most important case in which such variations might be expected and in which their occurrence seems to be confirmed by experience, is afforded by the *anomalies of adjustment of the eyes.* We may recall, to begin with, that these anomalies generally involve serious disturbance of vision; and it will be expedient to say something in advance concerning these ailments. In the last analysis they are due to conditions which cause certain irregularities even in the case of normal vision; namely, to the fact that the directions in which we see the external objects are not referred to the place of this eye or that, but to a common centre. If we see the objects arranged in directions with reference to this centre that correspond very closely with those given objectively with reference to the right eye and the left eye, and are therefore different from those that are referred to this centre, the result will be a certain inaccuracy in the arrangement of direction. An illustration of this is normal binocular diplopia, which is especially common and familiar.

However, under normal conditions, these irregularities are checked in a very important manner by the circumstance that, as the result of the law of binocular fixation, the images of a given external object are always formed on the two places of most distinct vision. The special consequence of this is that the phenomenon of binocular diplopia, or the perception of an object at two different places in the field of view, is confined to objects that are seen indirectly.

But now as soon as that law is violated, and one point in the surroundings is focused by one eye and another point by the other eye, the consequence of the general rules of the localization of direction will be that the point focused by one eye will be seen by the two eyes in two different places. Thus there is the possibility of diplopia even for points that are focused in the fovea of one eye.

Moreover, when the two eyes focus different points, especially if they are far apart, then, according to the laws of correspondence, these really widely separated points must be seen in coincidence and ultimately in close proximity; a perception which is erroneous with respect to the mutual position of the two objects and which generally involves also seeing at least one of them in the wrong absolute position (that is, in the wrong position with respect to the observer's body). A short name for these phenomena will be convenient, and I propose, therefore, to refer to this kind of fusion as a "*confusion*" (*Konfundierung*). Then we can say that the general principles by which the localization of direction takes place normally involve the possibility of diplopias and "confusions," and that, in the case of anomalies of adjustment, in which there is no longer any binocular fixation (supposing the laws of localization are not changed), these diplopias and "confusions," being extended to include the fixated objects and their immediate surroundings, must necessarily be far more pronounced and far more annoying than they would be normally.

It is a known fact that phenomena of this kind do indeed occur when anomalies of adjustment are artificially produced in normal eyes (by a slight pressure on one bulbus, for instance) or when, as the result of some pathological condition, such as paralysis of one or more of the muscles of the eye, these anomalies are manifested temporarily, and that in this case they annoy and afflict the patient very much. On the other hand, it is equally well-known that these disturbances are not permanent. If the anomalies are due to paralysis, the disturbances gradually disappear; and in the case of those maladjustments dating back to childhood, whether congenital or gradually developed, that is, in the case of what is known strictly as strabismus, the annoyances are not noticed. Thus it happens that the eyes of a cross-eyed person are permanently in a more or less abnormal adjustment, and that particularly when his attention is concentrated on a point, the eyes are in such an adjustment that, while the image of this point will be in the fovea of one eye, it will not be in the fovea of the other eye. And yet he may suffer no more annoyance from diplopia and "confusion" than he would do under normal conditions. In this case the simplest and most natural assumption would perhaps seem to be that, as the result of the anomalies of adjustment, the normal relations of

correspondence existing between the two eyes have undergone some change and been shifted to some extent, the consequence being, for example, that the impression of direction belonging to the fovea of one eye does not agree with that of the fovea of the other eye but with that of some excentric part of the retina of this eye. If the relations of correspondence were all shifted throughout in the same way, evidently there would be compensation to some extent for the disadvantage of the anomaly of adjustment. I propose to call a newly developed relation of this kind a *secondary correspondence*, in contradistinction to the normal or *primary correspondence*. And so hereafter I shall speak of two retinal places as corresponding primarily or secondarily, as the case may be.

On the basis of the observations up to that time, HELMHOLTZ regarded it as likely that the relations of correspondence were modified in the case of strabismus.[1] It is evident at once that, if this is really the case, it would be a strong argument for attributing normal correspondence also to training and learning. Consequently, in recent investigations also the central point of interest and the matter which has frequently led to the investigations themselves, has usually been the question as to whether the conditions of vision are such that we may speak of the formation of new relations of correspondence.

Now while it may be obvious that the annoyances connected with an anomaly of adjustment can be avoided by modifying the relations of correspondence, still it is clear that we have no right to infer at once from the absence of such disturbances that new relations of this kind have been formed, because it is possible at least to imagine a whole series of other modifications that might compensate for those disturbances in similar fashion. Naturally, it would be hardly feasible to consider thoroughly all these possibilities here. On the other hand, in examining cross-eyed persons, there is always very great danger of being misled not only by their inattention and imperfect observation, but also by not paying proper heed to what they themselves report. It is a very hard matter, therefore, to find out the precise individual mode of vision of a cross-eyed person. Under the circumstances, it is fortunate that there are *certain modifications* of normal vision which can be demonstrated beyond any doubt. I believe that for the discussion of all the relevant facts, the best way to start, in order to get some clue to them, is to bring out clearly these established processes.

[1] Incidentally, we may add that when HELMHOLTZ speaks of the formation of new relations of correspondence, it is doubtful if he had in mind such a thoroughly new formation as that of which we shall speak presently. Certainly what he was thinking of chiefly was just the point which was mentioned here first, namely, the change in the relations of direction.

One modification of normal vision, which is certain and easy to observe, may be mentioned here first. Its significance with respect to the subject under discussion has long been recognized. Ophthalmologists are especially familiar with it. It consists in a certain variation of the *relations of rivalry* between the two eyes. We are enabled to observe and appreciate it chiefly because some signs of it, at least, are apt to occur with persons whose vision is not usually classified as being anomalous. Any teacher of physiological optics knows that there are many persons who seem to have a peculiar difficulty about perceiving the ordinary phenomena of diplopia. The situation is something like this. The subject is told to gaze steadily at the vertical cross-bar of a distant window and to hold his finger in front of him about in the median plane. Then he ought to see the latter in double crossed images alongside the window-bar on either side of it; but he only sees it in one of these places. In order to help him to see both images, he is advised to close (or cover) first one eye and then the other; and then with his right eye he will see his finger on the left of the bar, and with his left eye he will see it on the right of it. But at the instant when the second eye is being opened (or uncovered), the half-image in the other eye regularly disappears, or that in the eye that is just beginning to see does not become visible; and so he never does see both images at the same time, but only one at a time.

Whether a definite field of view always predominates in these cases, or whether one prevails sometimes and then the other, is a question which I am not able to decide without further special investigations on this point. The former will certainly often be the case, and the whole mode of behaviour may be attributed to a dominance of one eye as the result of better visual acuity or refraction. And yet it is worth noting that the phenomenon can also be explained without this assumption; and from this point of view it reveals a peculiar form of the rivalry-relations and (it may be added) one that undoubtedly plays a great rôle in case of cross-eyed persons. If, as is the case here, the two half-images cannot both be seen at the same time, this must mean that whenever the left image (say) is visible and therefore the impression in the left eye prevails at a given place, then also at the other place where the half-image is in the right eye this latter image is suppressed every time, and the impression in the left eye gains the victory. Thus, while normally the rivalry-relations are locally independent of each other in such wise that the impression in one eye may get the upper hand at one place and that in the other eye, at another place, the relationship we have here is such that the same impression comes to perception at both places (whether it be now in one eye or now in

the other). I shall call this relation a *regional relationship* of the rivalry between the two eyes.

Let us say again that this does not imply at all a regular preponderance of a definite field of view and is not to be confounded therefore with what the ophthalmologists call an habitual suppression. The only question here is that for all parts of a considerable region the rivalry-relations are so connected that everywhere and generally the *same* eye predominates. Whether this is associated with a dominance of one eye so that that particular eye is always or most often the victorious one, is very questionable in the first place; and so what is meant by the term employed above is a behaviour that has nothing at all to do with this matter.

If we say that the difference between a regional rivalry and the normal rivalry is that the latter is *independent of locality (örtlichunabhängigen)*, it should be added that the question here is not in regard to a fundamental opposition between these two things, but simply as to a gradual difference. For even in case of the effects that conspicuous contours, etc., are known to have on the rivalry between the two retinas, anybody can notice that the triumph of the impression at a place in one eye helps the impressions of that same eye in the immediate vicinity of this place. However, this conducive influence falls off rapidly as the distance increases, and even at moderate distances the effect of it is so trifling that it can easily be overbalanced by other circumstances. If we think of it as being so magnified that it has a decisive influence at still greater distances, its behaviour then would represent what was meant above by a regional relationship. Thus even the normal rivalry-relations cannot be said to be independent of locality except in a limited sense. There are no absolutely independent rivalry-relations, and the regional relationship simply implies an enhancement of a process that exists even under normal conditions.

Perhaps, a somewhat stronger regional relationship is the basis of what is usually regarded as a certain impairment or obstruction of binocular vision, such as occurs, for instance, in those cases where there is a difficulty about fusing stereoscopic images. Incidentally, in cases such as those mentioned above, the regional relationship is not absolutely fixed. My experience is that the double images can always be made visible by using the method with the cord which I have described elsewhere [page 490].

Now it is obvious immediately that the disturbances which would naturally be expected from anomalies of adjustment may be counteracted and eliminated in some measure by a regional formation of the rivalry-relations. Thus, in a specific instance, if there were some relationship of this kind for the foveal regions, it would not be possible for objects to be "confused" that happen to be seen a little away from the centre of the field.

The other modification of normal vision that has to be mentioned here is one which I chanced to discover in studying my own mode of

vision. I reported this matter a long time ago,[1] and I may remark at the outset that the phenomena described then can still be observed at present absolutely unchanged. I shall simply mention here the salient features, referring the reader to my original article for the details. My eyes are in normal adjustment for the mode of vision which I use principally, and so far as the functions of binocular vision are concerned, my vision is practically perfectly normal. However, under certain circumstances, my eyes undergo a divergence which may amount to about 14°. It is accompanied, as a rule, by a certain suppression (*Exklusion*), particularly if this divergence is unintentional on my part and I am not thinking about the way I am seeing. Thus when I happen to be looking at nearer objects, the field of view of my left eye prevails, whereas for more distant objects the other field is in the ascendant.[2]

However, this suppression is by no means complete and insistent. The fact is rather that the moment any prominent objects happen to be in the two foveal regions, and my attention is turned to them, the phenomena of "confusion" that would be likely to be anticipated can be very easily observed. But then a phenomenon of a peculiar kind occurs which I believe has an important bearing on the vision of cross-eyed persons. If my attention is directed, alternately, first to the object seen by one eye and then to the object seen by the other eye, it appears to be in a *different* direction, and in each case approximately in the correct direction objectively. The "confusion" then (that is, the impression of immediate proximity of the two objects to each other) extends further in a perfectly irresistible way. Thus the whole foveal region can be seen in two different directions.

A rivalry-relation takes place between these two visual directions which is very analogous to the rivalry between the two visual globes; and so in the article referred to above, I spoke of the whole phenomenon as a *rivalry between the directions of vision*. It is connected, too, to a certain extent with the rivalry between the visual globes. If the objects seen by the right eye predominate, then generally the right-hand visual direction, that is, the direction corresponding to the actual adjustment of the right eye, will prevail also. Still this relationship is no rigid and general one, as shown by the "confusions" themselves. A fairly conspicuous object seen by the right eye can very well be

[1] *Archiv für Ophthalmologie*. XXIV. 4. 1878. p. 117.

[2] For instance, for many years I have been in the habit of letting my right eye diverge when I am reading. For long distance vision, especially when I was younger, I used to have the habit of letting my left eye diverge, because, by not trying to produce the convergence required for binocular fixation, I could more completely relax the accommodation, and the tendency of this was to improve the vision.

perceived in the left-hand visual direction also, the consequence being that it appears to be inserted in the wrong place in the perceptions of the left eye.

If we want to give a perfectly proper description of the relation between this particular mode of vision and normal vision and of the origin of it, it would not be right to say that a change has occurred in the original relations of correspondence. The modification that has taken place here consists primarly rather in what might be properly called a *duplication* (*Verdoppelung*) *of. the adjustment-factor*, in consequence whereof one and the same retinal place can give rise to two entirely different impressions of direction.

As to how this really happens, the first thing to note is that, although in the main the impressions in the right eye are determined by one factor and those in the left eye by the other factor (so that on the whole the impressions in the two eyes are localized approximately correctly), still this relation is not rigourously carried out. Hence, to a certain extent and under suitable conditions, there is a powerful impression of place-relations such as would be in accordance with normal correspondence (where there is a unitary adjustment-factor). This explains the effect which occurs when two objects are fixated that are far apart and in which the real fact comes out in the most striking manner. The instant the attention is fastened on either object, it will be seen approximately where it ought to be, and so the two objects will be seen in entirely different places; and yet at the same time there is the powerful impression of the two objects being close together in accordance with the normal correspondence.

In connection with this, another thing to be noted is that the difference between the two directions of vision is not constant in value. For instance, by not letting my eyes diverge as much as they might but holding them at a less divergence than this, I can reduce this difference if I wish to do so. But the controlling consideration then certainly is not the adjustment of the eyes nor the innervation; rather, in accordance with what has been stated already as to the great uncertainty of the adjustment-factor and its being determined by empirical considerations, we find here also that the difference in the visual directions is determined by the observed objects themselves. The nature of the objects, indeed, generally gives an immediate impression as to how far apart the objects are that are focused by one eye and the other eye and as to their positions with respect to the observer. This empirical determination of the adjustment-factor is evidently at the bottom of its duplication (*Verdoppelung*); and hence (at least in cases of this kind) the empirical circumstances are the real factors that determine the difference between the impressions of direction in the two eyes.

This is connected with the fact that when I produce after-images very nearly in the centres of my two eyes (in the way TSCHERMAK describes), they will invariably be seen very close together (that is, with the same adjustment factor) when the eye is entirely in the dark. Under these circumstances there is no duplication or doubling whatever. Similarly, when I am in the dark or if my eyes are shut, I can never be sure whether my eyes are parallel or divergent nor am I able to adjust them with certainty one way or the other, just as I please. Nothing but actual vision and the check it affords enables me to control divergence arbitrarily.

The above relations are important in many ways. They are important, in the first place, because they give some sort of idea of the way by which a modification of normal vision may be developed. This is true of the rivalry connecting certain regions, but perhaps in a very much more important way it is true of the modifications of the relations of direction. As a matter of fact, the latter can be understood when we note that even under normal conditions the impression of direction is invariably a result of different factors, that the adjustment-factor is concerned in its determination as well as the retinal place, and that by a duplication of the adjustment-factor this part of the whole relationship affords something that might be a basis of a modification.[1]

And so the views that have been developed here enable us to find our way to some extent amid the perplexing and apparently conflicting reports obtained from the examination of patients. It is true indeed that we are subject to various mutually contradictory and geometrically inconsistent impressions of direction, not all occurring at once, but competing with each other in some fashion. This is something that could not be calculated in advance in the case of persons with normal vision. A patient who happened to have my mode of vision and who, without being trained physiologically, was a thoroughly good and careful observer, would unquestionably report that he saw two objects in entirely different directions, and yet very close together; or, again, that he saw an object both to the right and to the left of another object, and yet not double, etc.

However, I am disposed to think that the chief significance of these facts is that they afford certain vantage points for developing further the elementary relationships that have been indicated above, thereby enabling us to decide questions that are of interest mainly on account of the special investigations that can be made with cross-eyed persons.

[1] In HERING's discussion of the case which he investigated, he reached a perfectly similar conclusion with respect to the development of the anomalous relation of visual direction.

We shall proceed at once to study these relations a little more in detail from this point of view.

To begin with, we can imagine a further development of these modifications of vision of such nature that, in case the anomaly of adjustment is a permanent one, the duplication of the adjustment-factor (which with me is simply temporary) will be permanent also. Moreover, in order to be entirely rid of all wrong localizations, all impressions in one eye would have to be regularly localized by one adjustment-factor and all impressions in the other eye by the other factor; in other words, it would be necessary to observe strictly a certain behaviour, which, as I have said, is given in my own case by suggestion. Again, a nearer approximation to normal conditions of vision would be obtained, if the difference between the two adjustment-factors were practically constant in value. If every impression in one eye were seen with *one* adjustment-factor, and every impression in the other eye with another adjustment-factor differing from the former by a constant amount, then each point in one eye would be, so to speak, *equal-as-to-direction (richtungsgleich)*, not with the point in the other eye corresponding to it primarily, but with a point that differs from this point by a certain amount. The effect would be to modify the correspondence, at least so far as the directions of vision are concerned. At any rate, there would no longer be any difference as to visual directions between this state of affairs and that of a modified correspondence, in which there was again a unitary adjustment-factor just as in normal vision.

Tschermak speaks of a condition of this sort as an *anomalous association of visual directions (anomale Sehrichtungsgemeinschaft)*. Thus the first question that arises, therefore, is as to whether the modification of the relations of direction between impressions in the two eyes, which we know at first as a facultative modification varying in value, can be developed into an exclusive modification and into one that is approximately fixed in amount.

Besides this, another set of questions arises as we probe into these facts. Thus it is obvious at once that, in case of such a modification of the relations of direction as that which we have just been considering, a series of disturbances and disorders would be bound to occur if the rivalry between the places in primary correspondence went on in the same old way and continued to be independent locally. For instance, if the nature of the thing seen happened to be such that a certain place in the right eye and the place in the left eye which was equal to it in direction (by secondary correspondence) were both suppressed, then nothing whatever would be visible at the given place in the newly arranged field of view. Likewise, we might have a double perception

at this same place. The observations mentioned above have already indicated *one* way by which disturbances of this kind could be avoided, and that was by the regional formation of the rivalry. Supposing this had been accomplished to a great extent, ultimately we should find that, while each eye would see in the right direction, there would be a more or less general exclusion between the two eyes, so that in the main they would function alternately—a mode of vision which we may speak of briefly as an *alternating* one. In this way disturbances of the kind referred to would be eliminated, but it is obvious, of course, that such a mode of vision would be quite different from normal. Consequently, when we consider whether modifications of some other kind may not take place whereby this changed mode of vision would more nearly correspond to normal vision, another set of questions arises. What these modifications would have to be, can be stated at once. New relations of correspondence between the eyes would have to be formed, modified not only with respect to the directions of vision but in all other respects also, if the old relations are to be abolished completely. In addition to the matter of equality of direction, this connection of correspondence consists in a number of well-known functional relations: that is, it means that if the impressions at places that correspond exactly do not accord and therefore fuse with each other, they must either exclude one another in the form of rivalry or combine in the form of binocular colour mixing; and it means also that places that do not correspond exactly must coöperate together to give impressions of depth. Thus the further question would come up here as to whether new modified relations between the two eyes are formed in all these respects also, parallel with the modification of the relations of direction, and as to whether cases occur where we can speak, therefore, of the formation of a new correspondence in an absolutely thorough way. This question has added importance because, as compared with the modification which may be considered as having been established by the facts above cited, the modification which we have in mind now would not only imply a gradual further development, but something new and, as we might say, a decidedly more radical modification of the normal relations.

Suppose we consider a condition of this kind for a moment. In the first place, it may be noted that, if we can imagine such a state of affairs as being developed by gradual stages from what we have found out about the duplication of the adjustment-factor, still the other circumstance alluded to above, namely, the regional relationship of the rivalry, would no longer be involved here at all. The fact is rather that, supposing it had been, say, temporarily present, it would have had to be completely eliminated again so as to give place to

another factor that was locally independent in the ordinary way except that it would apply to new pairs of points.

The next thing to be pointed out here is that a secondary correspondence would necessarily always lag behind the primary one in its operations, just because at least one of the two places in secondary correspondence would be situated more or less far from the centre of the retina where the visual acuity is relatively low. The advantage of the primary correspondence consists in the coöperation of the two places where the visual acuity is highest, and obviously this advantage could never be obtained in the present case.

Lastly, we must also remember that perhaps we should have to suppose that a definite behaviour of the ocular movements was involved in a secondary correspondence, whereby the image of the object which it was the intention to fixate would be focused always in the fovea of one eye and on the place in secondary correspondence with it in the other eye. We may speak of this briefly as a *modified binocular fixation*. Then the lines of fixation would converge or diverge as the distance of the observed object varied, exactly as they would do normally; and the anomaly of adjustment would be a constant modification to be added algebraically to all adjustments of the eyes.

Since, from what has been said, it is sufficient for our purposes to consider the very extensive mass of observational material from these special points of view, I shall simply refer in a footnote to the most important works on this subject,[1] and proceed at once to discuss the separate questions, alluding to the individual works when necessary.

In the first place, with respect to the *directions of vision*, the observations show that anomalous relations of direction occur to the greatest extent between impressions in the two eyes. They can be demonstrated, in the first place, whenever objects that are focused in the two foveal regions are perceived in different directions; and, in the second place, whenever objects focused in the fovea of one eye and on an excentric part of the other eye are perceived practically in the same direction. Aside from the unions in the form of binocular colour mixing to be discussed presently, this latter case may be noticed also

[1] SACHS, Über das Sehen der Schielenden. *Archiv f. Ophth.* XLIII. p. 597.—Idem, Über das Alternieren der Schielenden. Ibid., XLVIII. p. 443.—BIELSCHOWSKI, Untersuchungen über das Sehen der Schielenden. Ibid., L. p. 406.—TSCHERMAK, Über anomale Sehrichtungsgemeinschaft der Netzhäute bei einem Schielenden. Ibid., XLVII. p. 508.—SCHLODTMANN, Studien über anomale Sehrichtungsgemeinschaft bei Schielenden. Ibid., LI, p. 256.—HERING, Über die anomale Lokalisation der Netzhautbilder bei Strabismus alternans. *Deutsches Archiv f. klin. Medizin.* LXIV. p. 15.—See also the compilation given by HOFFMANN, Die neueren Untersuchungen über das Sehen der Schielenden, in *Ergebnisse der Physiologie*. I. 1902.

in the simple kind of binocular "confusion" when the two images are combined.[1]

Nor can there be any doubt that in many instances the anomalous visual relation is a comparatively fixed one, strongly prevailing over the original, if the latter is still present at all. This conclusion is supported especially by the possibility of demonstrating it by TSCHERMAK's method of after-images. If, as in this case, the after-images developed in the two foveas and observed in the dark field of view are perceived in very diverse places, it is an absolutely sure sign of the difference between the visual directions of the two places of most distinct vision, which is independent of any empirical conditions.[2]

On the other hand, serious difficulties are encountered when we begin to inquire whether the modified relation of visual directions can become so firmly established that the original relation *ceases* to exist altogether by the side of the new one; in other words, whether the original relation is suppressed completely and abolished. Doubtless, this is not the case as a rule. According to the authorities above cited, traces of the original relations of vision are to be found in nearly all cases. They consist mainly of the "confusions" between the two foveal regions that can still be noticed so easily in my own case; that is, they are manifested by the fact that objects which are really in very different directions give the impression of being adjacent when their images are focused approximately in the two foveas. Although there are numerous instances when nothing of this kind can be observed, one can scarcely help suspecting that under extremely favourable conditions perhaps a very skillful observer might be able to detect some such phenomena. It should be noted also that the regional rivalry that exists in a great many cases may prevent the occurrence of a "confusion." If the regional rivalry is very much in evidence, its effect anyhow will be to destroy the conditions which would presumably be conducive for a recurrence of the original relation of direction, and then there would be no point in trying to find out whether it still existed. Accordingly, the question will be left open as to whether the original visual relations are abolished completely, and we shall

[1] ¶The original reads: *Der einfachen Art binokularer Konfundierung zu "Sammelbildern."* (J.P.C.S.)

[2] Doubtless, therefore, the method of after-images is very satisfactory for testing whether the anomalous relations are wholly in the ascendant, and for telling if this is the case in a way that establishes the fact beyond peradventure. On the other hand, this method is not at all suitable for showing up a purely temperamental anomaly of this kind that may happen to be present. For instance, in my own case, as I have said, after-images that are close to the two foveas always appear directly adjacent to each other in the dark field (which means that I see them with the same adjustment-factor). And so an anomaly of this kind could not be demonstrated by the method of after-images.

have to be content merely to state that the modified relations have gained the upper hand and are certainly in firm control in many instances.

The next question is as to whether the modification of the visual relations can become constant in amount, so that a definite place on the retina of one eye will be conjugate to a certain place on the retina of the other eye in the sense of being co-directional with it. The best way of testing this is to examine in detail the combination of the impressions of the retinal places that are in secondary correspondence. We ought to find out whether the same impression of direction is obtained when the retinal images are at definite places (that is, whether fusion takes place when the images correspond); and if the impressions are different, whether the inequality of direction can be noticed in the form of binocular double vision. In reference to this, the observers all report that the secondary correspondence is never one that is very stable, but, on the contrary, wavers to a very considerable extent. In general, the establishment of a double vision like normal vision is not successful. For instance, when the image of an object is focused in the fovea of one eye, its image in the other eye can be varied considerably without destroying the unitary perception. This is so even in cases where binocular mixing of colours renders it certain that both impressions are registered. This result is in harmony with the fact that the movements of the eyes are never adjusted for the distance of the fixated object with anything like the same precision as in normal vision, and the consequence is that even a modified binocular fixation is not realized as a rule except in a very rough way. It is true, there are also certain difficulties about the interpretation of these data, as can hardly fail to be noticed; for, as was intimated above, a degree of precision in a new relation of correspondence that is even approximately equal to the normal precision is precluded at the outset by the simple fact that at least one of the retinal areas that is affected each time is excentric and consequently has only a relatively low visual acuity. Whether the amplitudes of variation alluded to above exceed the value that is conditioned and explained by this circumstance, is a matter that cannot be positively settled without quantitative data. I am disposed to think that they probably do, since from the nature of the methods employed here the displacements that were tested must generally have been quite large.

Proceeding now to the relations of *binocular coöperation*, we find that the observations show the greatest diversity of conditions in these respects; and the result is that we have begun to separate strabismic persons into various groups. In certain cases the development obviously proceeds in such a way that there is a tendency for a regional

relationship of the rivalry to become more and more firmly established. Especially when for some particular reason (such as monocular amblyopia) one eye acquires a permanent supremacy, the vision that is developed will be essentially monocular; and that is a case which is of no particular interest to us. In other instances the mode of vision evidently becomes an *alternating* one; and here also it is impossible to say positively whether the suppression ever does become absolutely compulsory. In most of the cases, as already stated, "confusions" can still be observed, which, taken in conjunction with a persistence of the original relation of visual direction, indicate also that the regional rivalry has not become absolutely established. However, there are other cases besides these in which an alternation of this kind is not developed, but both eyes are used simultaneously. As far as we are concerned, these latter cases are the most interesting ones. Aside from the modified relation of visual direction in cases of this type, the question arises as to whether those other functions connected with correspondence (rivalry, binocular colour mixing, perception of depth) may not also be developed anew in a modified relation between pairs of points that are not the same as those that were in primary correspondence. With respect to these relations, some phenomena of *binocular colour mixing* have been reported from time to time,[1] and there is scarcely any doubt as to the actual occurrence of this effect (for retinal places in secondary correspondence). When the image of a bright object is formed in the fovea of one eye and outside the fovea of the other eye, it appears single only, and its colour changes when a piece of coloured glass is inserted in front of the strabismic eye.

Of recent years quite a special interest has been taken in the other function of binocular vision that has to be mentioned here, namely the *perception of depth*. In testing this function the so-called empirical factors (especially, the movements of the eyes) must be excluded. However, this can be done without much difficulty, for example, by using HERING's so-called "drop test."[2] Numerous tests of this kind have been made, and the investigations show that, while binocular perception of depth was found to be lacking in the great majority of cases, it was undoubtedly present in some few instances. (See, for example, especially one of the cases described by BIELSCHOWSKI, *loc. cit.*, page 447. Binocular localization of depth was demonstrated also in the case given by HERING.)

[1] SACHS, *Archiv f. Ophthalm.* XLVIII. p. 444.—BIELSCHOWSKI, ibid., L. p. 445.

[2] ¶For testing stereoscopic vision, by dropping beans or marbles so that they fall vertically across the field of view, and requiring the observer to tell whether they descend on one side or the other of a fixed horizontal thread. (J.P.C.S.)

Finally, concerning the last question to be alluded to here, namely, as to whether the abolition of the original *rivalry-relation* between parts of the two retinas that were in primary correspondence has made them completely independent of each other, it is especially difficult to obtain a thoroughly satisfactory answer. It is true that there are numerous observations showing that objects are perceived simultaneously in different directions when they are fixated by both eyes. But, at least in regard to some of these observations, we cannot be perfectly sure that the two objects seen at the same time were both fixated exactly, or, to express it more generally, were seen by absolutely corresponding points. It is a question whether these difficulties can be eliminated by requiring perfectly exact fixation or by using extended objects where no gaps can be perceived. Besides, it is not easy to be sure from the statements of untrained observers, who are not familiar with these physiological relations, whether the two perceptions really do occur simultaneously or alternately in the form of rivalry. This is all the more apt to be the case, because, certainly if the observers do not happen to be trained, even when no relations of rivalry are involved, there are always some difficulties about focusing the attention simultaneously on two objects situated in different parts of the field of view and being sure that they are both perceived at the same time.

An observation made by Schlodtmann might possibly be considered as being good enough proof of the simultaneous perception of two regions in primary correspondence with each other. He succeeded in finding out how the visual acuity of a normal region varies according as the intention to fixate was dependent on the eye in question or on the other eye. It would be absolutely impossible (so it would seem at least) to make an observation of this kind in a case where the vision was alternating. And yet there is a question here as to whether the point fixated by one eye really did correspond exactly with the place in the other eye where the visual acuity was being examined, or whether it was simply very near this place.

In conclusion, some brief allusion must be made to those phenomena which are observed after an operation has been performed to correct cross eyes. If the anomalous relations are as firmly established as, according to the above, they certainly must be in a number of cases, it is natural to expect that, when the adjustment of the eyes has been corrected by an operation, there will be disturbances of vision at first from diplopia and "confusion," exactly similar to the case of a muscular paralysis. It has already been stated by Helmholtz that such is indeed the case; and this has been confirmed to a great extent by subsequent researches. But here too, as soon as we try to keep the details carefully in mind, the relations become complicated; for it is generally agreed that these disturbances disappear comparatively quickly and that

a normal binocular vision is established with astonishing rapidity when there are no other obstacles to prevent it.

On reviewing all the facts which have been adduced, the following conclusions may be summarized as being established. In the case of permanent anomalies of adjustment, the relation between the impressions of directions due to the images in the two eyes is very frequently altered, and a modified relation of direction is developed. In this respect subsequent observations have fully confirmed HELMHOLTZ's conclusion, based as it was on the meagre observational data available at that time, to which he rightly attached much importance on account of its theoretical bearing. The duplication of the adjustment-factor and the rivalry between the visual directions connected with it, as I have described them, afford also a clear idea of the mode of development of a condition of this kind. Perhaps in a limited sense we can speak of such a condition as a new organization of the relations of correspondence, even if the modified relation of visual direction is always differentiated from the normal one by having a considerable latitude of variation, and although the question still remains as to whether the latter ever is completely eliminated. The relations of binocular coöperation are generally not developed in the same way for the places in secondary correspondence as we know them to be for the places in primary correspondence; and yet we cannot deny that they may be developed in this way in some rare instances, both as to binocular colour mixing and binocular perception of depth, albeit comparatively imperfectly, especially in the latter case. Whether the rivalry between the places in primary correspondence can be abolished completely is something that cannot be positively established.

On the whole, therefore, correspondence would appear to be a functional relation which in some kind of way is specially *promoted* for the pairs of points in primary correspondence, without, however, being so absolutely firmly established that it cannot be developed for other pairs of points also, although this is accomplished with some difficulties and not so perfectly. While such a modification is apparently formed with comparative ease so far as the *impressions of direction* are concerned, it seems to be more difficult with respect to the other relations of binocular coöperation.[1]

[1] ¶The following is a list of some more recent literature concerning strabismus:

W. HAUSMANN, *Stereoskopen-Bilder zur Prüfung auf binokulares Sehen und zu Übungen für Schielende.* 3. Aufl. Leipzig, 1913.—C. DELOGÉ, The nature and treatment of strabismus *Amer. J. of Ophthalm.*, Series 3, 4 (1921), 407-418. —C. WORTH, *Squint, its causes, pathology, and treatment.* 5th ed. Philadelphia, 1921.—A. BIELSCHOWSKY, Die Genese abnormer Konvergenzstellungen der Augen. *Arch. f. Psychiat. u. Nervenkr.*, 65 (1922), 127-138.—Idem, Convergent strabismus in myopia. *Deutsch. opt. Gesell. in*

4. On Learning to See, and on Forgetting

The next subject we have to consider is how persons who are born blind can learn to see after having undergone an operation. We can dispose of it rather briefly, because, while quite a number of new cases have been reported, there is not much more to be learned from them than was contained in the earlier accounts that have been fully discussed by HELMHOLTZ in the text. References are given in the subjoined footnote to some of the more recent literature.[1] The bibliography prior to 1891 may be found in UHTHOFF's article (which is the one mentioned first in the footnote). All the observations tend to show that an optical recognition of definite *objects* that are otherwise familiar is impossible at first and is acquired only very gradually in the course of weeks. (Perhaps this is the main reason why vision is so extremely imperfect just after the operation has been performed.) This is a fact which doubtless is principally of psychological importance, inasmuch as it indicates that here, as everywhere, the retention in the memory of compound impressions comprising a lot of details is a matter of repeated perception and of an acquisition depending thereon. With respect to what may be considered as primitive space-determinations undeveloped by any experience, the fact is not without interest that even quite simple forms are not recognized at first. (See, for instance, UHTHOFF, *Zft. f. Psychol.*, XIV. p. 209.)[2]

The most interesting fact of all from the optical point of view is the great uncertainty about perception of distance. (The boy examined by UHTHOFF two months after the operation tried to reach out his hands and take hold of an object 1.5 metres away.[3]) The other thing (undoubtedly connected with the first) is the corresponding uncertainty about judgment of size. These facts bring out very clearly the great rôle played by experience in regard to these matters. It is especially remarkable that the wholly peculiar relations between size and arrange-

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Jena. 1922, 245-248.—R. I. LLOYD, Measuring the deviation of a strabismic eye on the stereoscopic campimeter. *Amer. J. of Ophthalm.*, Ser. 3, 6 (1923), 839-841 —E. HOLM, Central and excentrical fixation. *Acta Ophth* , 1 (1923), 49-54.—E. LANDOLT, A study on strabismus.*Amer. J. of Ophthalm.* Ser. 3, 6 (1923), 93-102. (J. P. C. S.)

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[1] UHTHOFF, Untersuchungen über das Sehenlernen eines siebenjährigen blindgeborenen und mit Erfolg operierten Knaben. *Beiträge zur Psychologie*, etc. HELMHOLTZ-*Festschrift* 1891.—Idem, *Zeitschrift f. Psychologie*. XIV. p. 197.—FRANCKE, Das Sehenlernen eines 26jährigen intelligenten Blindgeborenen. *Beiträge zur Augenheilkunde*. XVI, 1894.—SCHLODTMANN, Optische Lokalisation bei Blindgeborenen. *Archiv. f. Ophth.* LIV p 256. 1903.—SCHANZ, *Zeitschrift f. Augenheilkunde*. XII, p 753—LATTA, Notes on a case of successful operation for congenital cataract in an adult. *Brit. Journal of Psychology*. I. 1905. p. 135.

[2] ¶See J. H. FISHER, Vision learning after successful operation at the age of six *Ophth Rev.*, 33 (1914), 161–165. (J. P. C. S.)

[3] HELMHOLTZ-*Festschrift*. p. 156.

ment, the way in which they depend on the distances being unequal, the occlusion of a (farther) larger object by a smaller near one—all have to be learned over again.—A brief reference should be included here to another class of phenomena with which we have become acquainted in recent years, and which in many ways are similar to the cases of which we have just been speaking -namely, the phenomena of *forgetting how to see* resulting from long disuse of vision. For instance, if a child's eyes have not been functioning for years as the result of what is known as blepharospasm, vision has to be acquired anew to a certain extent after that condition has been cured, and the phenomena observed in such circumstances are very analogous to those which are noticed in the case of a patient who, being blind from birth, has undergone an operation to enable him to see.[1]

5. On the Physiological Foundations of Judgment and Learning

I shall proceed now to discuss certain general considerations relating to the origin of the phenomena of consciousness and its dependency on physiological processes. As I have already said, these relations have an important bearing on our ideas of the laws of localization. In connection with the argument about to be given it will be well to keep in mind certain objections which have been urged to a great extent against the empirical theory. Suppose, for instance, that when a point is seen with a certain cross-disparity, we have the impression that it is farther off than the point of fixation: if this impression is the result of training, apparently it proves to be a *judgment* based on a general experience. The argument against this that is often used is that the determinations of the places of things seen are given in our consciousness exactly in the same way as their optical qualities are (such as colour and brightness), and that consequently there is no justification for making a distinction between these determinations and those qualities as if they were something radically different, but that, on the contrary, the former have just as much right to be regarded as *sensations* as the latter. And so this is a question about which it is necessary to take a stand, and the significance of which needs to be made clear; that is, the question as to whether the space-determinations of our visual impressions are to be termed judgments or sensations.

[1] Uhthoff, Beitrag zur vorübergehenden Amaurose nach Blepharospasmus. *Sitzungsberichte der Marburger Gesellschaft zur Bef. d. ges. Naturw.* pp. 1–68. 1891.—Silex, Eigenartige Sehstorungen nach Blepharospasmus. *Archiv f. Psychiatrie.* XXX. p. 270. 1898.—Lobanow, Verlernen des Sehens durch Katarakt-Erblindung. *Klin. Monatsblätter für Augenheilkunde.* XXXVIII.

It might be supposed at first that this was simply a question of terminology that did not admit of a positive answer one way or the other, and that it was more or less a matter of arbitrary convention whichever way we decided. The real psychic fact of seeing an object directly in front of us, and of seeing one object farther away than another—surely, that is different both from the pure typical sensation produced, for example, by the action of a perfume on the olfactory organ, and from the typical judgment expressed by saying that it is going to rain soon. Whether we prefer to extend one of these categories or the other so as to include the optical perceptions, seems to be more or less arbitrary and at best simply a matter of expediency. Yet on looking into the question more carefully, we shall find that its decision one way or the other has been based usually on very definite general conceptions, the logical consequences of which prove to have a practical importance and to be of value.

When the arguments on this subject are reviewed, we find, to begin with, that the matter can be, and indeed has been, considered from two entirely different standpoints: namely, (1) with reference to the psychological qualification of the space-determinations, that is, as to what these determinations are and what they signify, and (2) with reference to the way in which they originate, especially as to how they come to consciousness.

If, taking the first point of view and disregarding entirely the mode of origin, we fix our attention on the nature of the given phenomenon itself, especially on its compositeness or oneness, on what can be differentiated about it or in it, etc., there can be no question as to the fact that seeing an object at a particular place, even according to our notions of space in general, does contain an element of objectivity which distinguishes between the object and the subject, and that it does represent a conviction or an impression (if you prefer to use that term) of an actual attitude toward, not the subject himself, but toward the external things, the *non-ego*; and that, therefore, in its own peculiar content, it is completely equivalent to what we are otherwise in the habit of terming a judgment.

If we start with the other point of view, and consider the mode of origin as the paramount question, we shall reach a different conclusion. Then the original assumption generally is that the judgments are dependent on previous processes that can be shown in our consciousness. They appear as the result of reflection, consideration, etc., wherein the tendencies both to affirm and to deny can generally be detected to some extent; and the judgment finally consists in the victory and triumph of one tendency or the other. On the contrary, the sensations seem to enter the consciousness directly as the result

of physiological conditions. The thing that stands out then as the decisive criterion is the immediacy and irresistibility of the intrusion; and we call everything a sensation that behaves in the same way as the typical sensations in this respect. This principle has been accepted by most modern writers on physiology. Thus, for example, it is adopted by EXNER when he speaks of a sensation of the motion of observed objects in certain cases.

Now it seems to me that we are dealing here with a principle that could only be carried out on a perfectly definite assumption, an assumption which is identical with a conception that was doubtless once actually entertained. Possibly even now, without being expressed and without being perfectly clear, it is this conception that is to a great extent at the bottom of the distinction about which we have just been speaking. Briefly stated, the characteristic thing about this conception is that the action of the body on the mind consists in the production of sensations, which therefore would constitute for the mental life that which was given to it immediately; whereas all higher and more complex psychic forms, especially the judgments, would be produced and determined by the utterly different and independent laws of the mental life itself. If this were so, then, for any class of phenomena defined by its psychic nature, and for it only, the mode of origin would also be one that was rigidly determined. The two ways of characterizing the phenomenon here spoken of, namely, as to its psychic nature and as to its origin, would amount to the same thing in this case. Now after reviewing all the known facts, there is no doubt in my mind that this is *not* the case (which I regard as a fact of fundamental importance). Not only is it a fact, in my opinion, that a certain class of psychologically unitary phenomena of consciousness are connected with purely physiological conditions (not characterized by correlatives of consciousness) in the same way as the sensations themselves are supposed to be, but exactly the same thing is true likewise to the greatest extent in regard to higher and more complicated psychic phenomena, and especially in regard to quite typical judgments.

A case of this sort is the peremptory impression of an *object in motion* that is produced when a definite optical feature of some kind traverses the field of view; as, for instance, when two systems of mutually intersecting lines are shifted with reference to each other. The same sort of thing is manifested in a perfectly unequivocal way by those phenomena conventionally known in psychology as *judgments of recognition*. The connection of the instantaneous sensory impression with a conception developed by experience to which it is subordinated usually follows just as promptly and immediately as the sensation

itself. A sweet taste is usually classified at once under the proper empirical conception. In many cases (especially with colours, for instance) the conceptions of which we are speaking at present do not signify sensitive states of the subject but properties of the objects themselves. We see something white or red, bright or dark, etc., and the sensation becomes involved with the corresponding conception in such an insistent way that it is difficult to imagine the sensation apart from this association.

Exactly the same thing that is true about recognizing simple qualities of this kind is likewise true of the recognition of entire objects that are known by experience. The impression that we see a cat running or a horse standing still, or that we hear a certain person speaking, is produced just as immediately as the impression that something in front of us is moving or that somewhere there is a black or red object, etc.

The possible limits of this region and how they are produced are questions that we do not need to discuss at present. Enough has been said already to show that this principle of calling everything a sensation that has a direct physiological basis cannot be carried out without coming in conflict with our ordinary mode of speaking in a way that would be absolutely disastrous and intolerable. We shall have to be content rather simply to say that *judgments, including those involving conceptions that are undoubtedly acquired by experience, are also determined directly by physiological agencies in characteristic fashion, and may come to consciousness as something bestowed immediately, complete and obligatory.*

This formulation is important because these processes obey characteristic laws, and hence the basis of any proper comprehension of the subject lies in recognizing that we are here in the domain of a special kind of physiological action. It will suffice to recall some of these characteristics.

One of the first that may be mentioned is the *abrupt variation* to which these relationships are liable. The distinct impression that we ourselves are in motion can be converted, as is well-known, into the other impression that the observed objects are moving, while we are stationary. A similar reversal of the impressions of distance occurs in looking at SCHROEDER'S "staircase" diagram [see Fig. 49], especially if it is turned around. The reader may be reminded also of the phenomena noticed in looking at "puzzle pictures." After long contemplation the "hidden" object usually is suddenly perceived; but, having once been discovered, it continues to be seen, and it is difficult to recover the previous mode of vision. All these phenomena enable us to realize how an impression, coming to consciousness immediately and showing,

therefore, that it has a direct physiological basis, may be linked with some sort of special physiological mechanism which is revealed by this characteristic of its function.

The second point to be considered is that judgments evoked immediately in this manner may be in conflict with our better knowledge. We may *know* that an object stays where it belongs and at the same time have an irresistible impression of its being in motion. We may know that a person is absent and still have the impression of hearing him talking. We see, therefore, these two kinds of judgment standing in a relation to each other such as would be impossible in the case of the conscious judgments to which we are accustomed chiefly. A certain independence is manifested between these two kinds of judgment, probably indicating a duality in their physiological substrata; and while these relations are unquestionably of much significance, it would be premature to use them as the starting point for new hypotheses.

Lastly, we must call attention again to a matter that has been mentioned already and carefully considered (see pages 271 and 389); that is the fact that the impressions that are produced immediately by physiological agency are not unfrequently in conflict *with each other*. For instance (as we saw above and emphasized as being particularly important for the general significance of relations of this kind), we may be under the powerful impression that an object is in motion, although after a certain interval of time it appears to be just where it was at first. Phenomena of this kind were included by FLEISCHL in his proposition, that for immediate sensations the laws of logic are not valid. Clever and acute as this formulation was, it is not free from objection. On the basis of the preceding discussion, we shall state that for the physiological processes under consideration at present an inner relationship, such as we are aware of in our conscious modes of thought, either does not exist at all or perhaps does not exist in exactly the same way. In my opinion this is a satisfactory statement in regard to this class of facts. Besides, when the phenomena are considered in this light, they cease to appear as puzzling and paradoxical as they did at first; for the truth is, there is no reason whatever for us to expect the same (or analogous) rules to be applicable to these physiological processes as to the very different processes which are presumably the substrata of conscious mode of thought. For example, if we suppose that the impression of a movement may be initiated directly by the gliding of the image over the retina, it is not strange to find that this impression is more or less independent of the local values given at the beginning and end of an interval of time and may occasionally be in conflict with them.

It might be worth while to illustrate this theory, which has been presented here at first merely in a general way, by showing how it applies in some special cases. A subject in which these questions are of much importance in my opinion is the case of phenomena of contrast of both luminosity and colour. In interpreting these effects, as I have shown elsewhere,[1] we are absolutely bound to take account of the wide fluctuation of judgments of recognition which is so characteristic of this very region. Unless we consider the peculiar physiological conditions in such cases, we cannot really have a clear notion of what HELMHOLTZ intended when he speaks of them as "illusions of judgment." At present, it is true, we are still unable to say with any certainty how far the significance of these relations extends, but undoubtedly we have no right to argue that, simply because we have a positive impression of seeing first a white object and then a grey one at the same place, this amounts therefore to a satisfactory proof of a change in the sensation; although this is the argument that has often been employed against HELMHOLTZ's theory of contrast. This would be to underestimate completely the complexity that is unquestionably characteristic of the physiological conditions of judgments of this kind. The tacit assumption in that case would be that these conditions were perfectly simple and that these judgments of recognition were always correct.

In order to obviate any misunderstanding here, and to clarify the situation in regard to the literature on the subject, it ought to be stated that HERING, who was the chief opponent of HELMHOLTZ's theory of contrast, afterwards pointed out these relations himself and laid special emphasis on them. In particular, he has described[2] the peculiar conversion that occurs when an (objectively) dark place surrounded by a brighter area, which at first is seen as a spot, is afterwards seen as a shadow falling on the surface, due perhaps to some shifting; so that it gives at first the impression of a grey of the same luminosity as the surroundings, and then that of a (shaded) white. Apparently, HERING did not notice, or else failed to realize, that the very things which he properly emphasizes here as noteworthy are precisely the facts which HELMHOLTZ used as the basis of his theory of contrast, and that by acknowledging them, we are bound to admit at the same time that this theory is at any rate a possible explanation to a great extent; and that the effect of these facts was to knock the props from under HERING's previous opposition, which had culminated in his assertion that HELMHOLTZ's theory turned black into white. The very facts

[1] NAGELS *Handbuch der Physiologie*. III. p. 240.

[2] HERMANNS *Handbuch der Physiologie*. III. p. 574. Grundzüge der Lehre vom Lichtsinn, in GRAFE-SAMISCHS *Handbuch der Augenheilkunde*. Kap. XII. p. 8.

brought out by HERING himself prove that it is possible for the positive impression of grey to be converted into that of white without any change in the sensation.

The fact that HERING does also speak of a change in the sensation in the case of this conversion,[1] makes it difficult in a certain way to understand these relations. But it is just here, in my opinion, that this expression appears to be misleading and not very appropriate; for there is no doubt about the fact (which can be proved unequivocally by personal observation) that when the impression of a spot changes into that of a shadow, there is something on it or in it that *persists unchanged*. This is appropriately expressed by saying that, while the sensation proper has remained the same, the empirical conceptions associated with it have been modified in some peculiar way. (This association of the sensation with empirical conceptions can very well be considered as a physiological process and as something different from a cognition in the ordinary intellectual sense.) But how shall we denote this element which remains constant, if the association with the empirical conceptions white and grey, spot and shadow, is itself termed a sensation, and is therefore spoken of as a change in the sensation when the conversion takes place? When we use this terminology, we have no name left to describe a behaviour that is perhaps undoubtedly extremely significant. Indeed there is danger of losing sight of it altogether and of confusing it with something entirely different.[2]

It must be unequivocally admitted, therefore, that there is bound to be a distinction between the sensation (in the strict sense) and its connection with empirical conceptions. Whereas the former depends in a comparatively simple and practically invariable way on stimulus and the state of the organ of sense, the latter is subject to far more complicated and entirely different laws, although in the last analysis they are certainly physiological also.

HERING has raised the question as to whether we have the right, if I understand him correctly, to look at the matter in this way. He insists that in such cases as the above all we can do is to establish the fact of a variation in the sensation-complex, and that we know nothing about a "pure sensation." However, in reply to that, the

[1] See, e.g., HERMANNS *Handbuch der Physiologie*. III. p. 568.—Grundzüge der Lehre vom Lichtsinn, in GRAFE-SÄMISCH'S *Handbuch der Augenheilkunde*. Kap. XII. pp. 8ff.

[2] Thus, for example, we are liable to make the mistake of supposing that HERING had assumed that the sensation was subject to the same kind of influences in the case of contrast phenomena also, whereas the influences which he did assume in this case were of a wholly different nature. On the contrary, to assume that there was a modification such as occurs in the case of the conversion of the impression of spot and shadow, would be equivalent to HELMHOLTZ'S explanation and would differ from it only in terminology.

point is that the immediate and positive impression of the constancy of the real sensation in this case amounts to an experience that we have concerning it, and that therefore the sensation is by no means wholly beyond the range of our observation. Moreover, it should be recalled that, according to HERING's own theory of the sensations of vision, when a conversion of this kind does take place, *no* change is to be assumed in the relation between the so-called D and A processes,[1] and therefore the physiological basis of that constant element of consciousness may also be taken for granted. We must, therefore, most positively insist that the question as to how the sensation itself is formed must be kept separate from its association with empirical conceptions that are retained in the memory; and that this distinction is not just merely a theoretical postulate, but actually does apply to our observations to a certain extent.

The above relations are also fundamentally involved in *comparisons of size*, and so this is the proper place to refer to that subject again in order to consider it from these new angles. The impressions we have of absolute size evidently belong also to the judgments that are immediately connected with physiological processes in this same way; and they are subject to the peculiar conditions that exist for these processes. This is shown by the fact that often, when there is no question of the angular size or even of the distance for that matter, the observed objects give a very direct impression of a definite absolute size. It is this that enters the consciousness immediately and is retained in the memory, when we are unable either to say what the angular size is or to recall it.[2] In view of this fact, we can see why our immediate impressions of the absolute dimensions of observed objects are sometimes related to each other in ways that are mathematically impossible (which is another fact to be stressed here as having much significance). This is the case chiefly with reference to the relations between distance, angular size and the impression of absolute size. In connection with these matters, attention has already been called to the fact that, in case of the exertion of accommodation, for instance, the apparent size of an object may diminish without any corresponding decrease of its angular size, and that, so far from the object seeming to be nearer under these circumstances, the apparent distance increases. Consequently, we cannot avail ourselves of the mathematical law between angular size and the impression of distance, which is objectively valid, in

[1] ¶See Vol. II, p. 435. (J.P.C.S.)

[2] See some remarks on this subject in a paper on Beiträge zur Lehre vom Augenmass, contributed by me to the HELMHOLTZ-*Festschrift*. 1891.

order to deduce the impression of absolute size. As a corollary from Fleischl's formulation referred to above, it might be argued that, while the laws ofmathematics are doubtless true in regard to imaginary or geometrical magnitudes, even they are not valid for magnitudes perceived immediately by the senses.

It seems to me sufficient importance has not been attached to these considerations in connection with the vexed question as to the apparent dimensions of the heavenly bodies. In my opinion, any attempt to explain these phenomena should start, first of all, with the fact that in gazing at the heavenly bodies we get a very definite impression of a certain *absolute* size. This may not be true of everybody, but it is certainly the case with many persons. This apparent absolute size is, indeed, plainly out of all proportion to the angular diameter and the distance at which these objects are seen. For example, in my own case (which I believe is likewise true of many other persons), I get a very positive impression from the disc of the full moon that can readily be expressed in terms of an absolute magnitude, notwithstanding the fact that I may be perfectly aware of the absurdity of any such estimate. When the moon is high in the sky, I can fancy that it is about 20 cm in diameter, whereas, when it comes up above the horizon, I may estimate the diameter to be between 30 and 35 cm. An object that was really this size would have to be about 25 metres away in order to subtend the same visual angle as the moon. But the actual impression of distance does not correspond with this at all. If the moon happens to be so situated that it is obvious at once that it has practically the same angular size as some object on the earth, for instance, if its upper edge is just over the top of a chimney, my previous impression may waver; but the moment it is impossible to make any direct comparison, the impression of size comes back irresistibly. Doubtless, it is these impressions of absolute size that are the real basis of the illusion about which there has been so much discussion. Nor do I believe we can ever be sure about the explanation of this illusion until it is clear in our own minds how we really do get an impression of absolute size, especially in a case like this where there is such a discrepancy between it and the impression of distance.

The ideas thus obtained likewise tend to conform the position we took in regard to the theories of the geometric-optical illusions see [page 234] and especially as to certain general questions which were raised by Witasek in this connection (*Zft. f. Physiologie*, etc., XIX. 1899. p. 81); to which I shall revert briefly once more. Witasek starts out by asking whether those illusions can be explained as modifications of the sensation or as illusions of judgment, and so he makes a distinction between what he calls sensation-hypothesis and judgment-hypothesis. In following the argument, it should be noted that what is meant here by sensations are the space-determinations of the thing seen or the localizations, as we might say.

From our point of view also certain considerations can be formulated which are similar to those of Witasek. Indeed, it would be quite possible to suppose that definite space-forms (more or less different from the real forms of the observed objects) could be given for every kind of optical impression, such that all our judgments as to the mutual relations between the parts (dimensions, directions, curvatures, etc.) would be consistent; in other words, that our entire judgments would fit into a definite structure, which, while it would certainly be different from the real one, would not be contradictory *per se*. Were this the case, we could endeavour to explain all the relevant phenomena simply by the relations of *localization*; whereas, in regard to the origin of all the judgments concerning the inner relations of a space-form, no special problem would be involved, inasmuch as these judgments would be correct throughout in the sense that they would correspond to the relations of a definite space form of this kind.

Now the one fact that should be stressed here as the most important and the most certain thing about these phenomena in my opinion, is that this at any rate is *not* the true state of the case. In speaking above of the relations between impressions of absolute size and distance, we saw that the comparisons made by the judgment may be subject themselves to very complex conditions, and consequently may sometimes be mathematically impossible and contradictory. It can hardly be doubted that the figures of two dimensions, to which the so-called geometric optical illusions relate, probably behave in the same way also.

The result is, therefore, that at any rate the phenomena cannot be interpreted on the "sensation-hypothesis" *alone* (to use Witasek's phraseology here), but that the assumptions that are characteristic of the "judgment-hypothesis" are also appropriate to some extent. But now if this is so, then, exactly as was found to be the case with contrast phenomena in connection with the sensations of light and colour, it will be extremely difficult to obtain a perfectly positive proof of a change in the "sensation" (as this term is used by Witasek), that is, a proof of a change in the localization. In fact, the evidence of a change of localization submitted by Witasek is open to the same objections as the arguments that are often advanced concerning those other phenomena of contrast. Moreover, the assumption made to start with, which in my opinion is by no means free from objection, is that the conditions on which the judgment depended would have to be given exclusively by the determinations that could be psychically demonstrated.[1]

When we take into consideration the complicated conditions of those judgments that are the direct result of physiological processes, the evidence for the change of localization does not appear to be conclusive. This does not mean, of course, that there may not be some influence on the localizations such as Witasek has assumed; but it will be very hard to obtain a satisfactory proof of it, and still harder to be sure as to what share those other relations have in the optical illusions of size.

These general conceptions that have been developed here are important with reference to a still wider range of questions. If the impression of direction produced by a retinal image, whether it is in the fovea or not, depends on the temporary adjustment of the eyes (apart from the retinal place itself), it is natural to ask how this effect is produced and what is the actual factor in this adjustment that does

[1] See, for instance, *loc. cit.*, p. 155, where the effect of this assumption comes out very clearly.

influence the impression of direction. The apparent movements of the eyes that can be noticed when the ocular muscles are paralyzed show that it cannot be a question in this case of sensations of some kind emanating from the muscles themselves and caused by their states of tension or contraction. If, therefore, we are disposed to regard the fact of innervation itself as being the controlling circumstance and to consider that a "feeling of innervation" is at the bottom of the adjustment-factor, the difficulty about this is that it is very doubtful, to say the least, whether any such feeling exists. At any rate, it cannot be brought clearly to consciousness in any satisfactory way. The same thing is true as to the influence of accommodation and convergence on the impression of distance. Here also, in my opinion, it is necessary to bear in mind that we have no right to insist that that factor which is manifested in the space-determinations (that is, in our impressions of distance and direction) shall be a definite psychic element that can be demonstrated in our consciousness; and that being so, we are not obliged to look for such an element. In my judgment, the physiological process of the given innervation itself will always have to be considered as the controlling affair, which, by virtue of a relationship of some kind, is interlinked with the processes that determine the impression of locality.

When matters are viewed in this light, apparently it does not make much difference so far as our questions are concerned, whether these innervations are accompanied or not by any feeling of which we can be conscious. At any rate this dispute need not involve us in any difficulties.

Similarly, we may suppose that the relations of accommodation (or convergence) also have a direct influence on the impressions of absolute size; and on this assumption we can see, too, how these relationships do not always need to be initiated by a corresponding formation of the impression of distance (as previously explained, page 388).

These relations are also not without importance in regard to the problems of localization. They show that even complex psychological images (*Gebilde*), undoubtedly empirical in origin, may be subject to laws concerning their occurrence such as were formerly supposed to apply to the sensations alone. In other words, the conditions under which they originate cannot at any rate be conclusively proved to be phenomena of consciousness at all, and hence they must be conceived in a form that is essentially physiological. Consequently, there is nothing at all exceptional in what is assumed by an empirical theory of the space-determinations, and we see that something quite similar to it does take place on a large scale. It follows at the same time that the "immediacy and inevitability," usually regarded as the

criterion of a direct sensation, can actually be developed to a great extent; and hence this criterion does not tell us anything whatever as to the original nature and genesis. The fact that a special relation between the impressions in the two eyes does give the prompt and peremptory impression of a definite configuration of distance, as we know, and the fact that the place-value obtained by our visual sensations as something given immediately does signify a definite relation with respect to our own body, although doubtless the empirical conception of this relation is implicitly involved in these determinations at the same time—these facts will not seem strange to us, when we remember that the impression of a movement or of a definite absolute size or generally of any manifold situation denoted by empirical conceptions may be produced just as directly and energetically in a perfectly similar manner, even though sometimes it is in conflict with our higher intellectual knowledge.

The more general relationship in which the development of localization appears to be presented by this view of it is one of the points that should be noted. Another one that is no less important is the fact that here also we have been led to think of the nature of learning as a physiological development. The result, as we shall see later, is that there is room for a wider range of possibilities than there would be if this development were connected simply with the psychic phenomena and had reference only to them. It is true that as yet we do not know in detail the modalities of such a development; still we can say that there is a certain support for them in the plasticity of the cerebral dispositions, as it is usually called nowadays, which is a property that is being rated higher and higher in the light of our modern discoveries. (We need only instance here the re-learning in the case of reversed healing of motor nerves.)

Now after these expositions, if we recur to the original question of terminology with which we started, I think we can see very clearly that it would be very inadvisable, to say the least, to consider space-determinations as sensations. They are so fundamentally different from what is connoted by sensations in the strict meaning of the word, that is, from those phenomena which are comprised within a fairly well-defined territory and which are characterized both by the immediacy of their psychological quality and by the conditions on which they depend, that it certainly does not seem to be appropriate to call the two by the same name. Yet, on the other hand, it must be admitted that no matter how much these space-determinations are entitled to be called judgments in a certain sense, they are different from what we usually mean by judgments. Therefore, the word *perceptions*

(*Wahrnehmungen*), introduced and established by HELMHOLTZ as a special name for the phenomena of this whole territory, appears to be a convenient and appropriate designation.

For this reason, although I am fully aware of the accuracy of EXNER'S observations in this field, and appreciate their great importance, I am compelled to differ with him when he speaks of "sensing" (*Empfinden*) a motion. The impression that a body is or has been in motion is certainly to be called a judgment so far as its psychic contents is concerned. This is even more obvious at once than it is in the case of simple localization. If this impression is on a par with localization as to the inevitability and immediacy of its occurrence, logically the name perception should be applied to it also. At any rate, we should not make the mistake of supposing that this term is used simply to denote something that is perfectly unitary, for it is also used to denote a variety of things that are quite different from each other. It is a question whether this evil could be avoided by extending or specializing the terminology still further. It certainly could scarcely be carried out in any entirely general way, and the special conditions in each particular province would have to be taken into consideration. As to perceptions of motion, I should think it might be sufficient at first to distinguish two modalities by speaking of direct and indirect perception of motion. Matters would probably be much more complicated with reference to perception of distance. Certain differences in the nature of the impression itself are undoubtedly connected with the big differences between those circumstances by which the impression of distance is generally determined. The differences given by a painting are not the same as those given in a stereoscope nor the same as those that occur in nature, for example, with respect to the more distant parts of the landscape; although the latter depend on the same details as those in the painting (perspective, apparent size, aerial perspective, etc.). Here, too, there may be some doubt as to how far the term perception should be extended, and of late years it has frequently been found necessary to make a distinction between the perceived (or "sensed") distance and one that was only imagined. However, I am afraid that such a distinction might be liable to serious difficulties. The impressions of distance that depend on the so-called empirical factors as they are *imagined* and those that are determined binocularly as *perceived* are so inextricably intertwined that it would seem hopeless to try to differentiate between them. The apparent distance of a point which is seen with a cross-disparity of some kind is perhaps dependent in a complicated way not only on the value of the cross-disparity but on all the aggregate of empirical factors involved in determining the apparent distance of the point of fixation

itself. That is why it seemed to me more correct to use the perfectly general expression "impression of distance" wherever the word "perception" might be open to criticism.

Thus, while the nomenclature introduced by HELMHOLTZ appears to be both appropriate and felicitous in this respect, objections to it are more likely to be made because the expressions that have to be used to describe certain processes that may be regarded as physiological were borrowed originally from psychology. This is particularly true with respect to the matter of unconscious conclusions [see page 6], which has been a great subject of controversy. Later we shall have to allude to it again, and see what HELMHOLTZ meant by this expression and his justification for using it [see page 645].

6. Empiricism and Nativism[1]

In the following pages I shall try to coördinate and crystallize the results of the preceding discussion, but I ought to say in the beginning that my aim is not to develop a definite theory of the characteristics of spatial perception which could be recommended as the most probable and acceptable hypothesis at present. My intention is more modest than that, for I shall simply endeavour to systematize the facts that to a certain extent may be said to be positively known, at the same time specifying those matters about which we are not yet able to come to any definite decision. After all this is the real problem at present, and, I may add, one that is more suited to my predilections and scientific principles.

Empiricism and *nativism* are the two slogans that indicate what has been the main issue here for a long time; and, indeed, this famous controversy is well calculated to bring out the fundamental points that ought to be considered. However, in order to make it perfectly clear what is our peculiar concern with this question at present, a preliminary statement ought to be made especially in regard to the meaning of nativism. If this term is taken literally as implying the congenital existence of dispositions of some sort, modes of functioning, etc., that is, their actual presence at the moment of birth, we cannot help feeling that there is something more or less arbitrary about choosing this particular instant of time. Of course, nobody can deny that we may conceive of an origin or formation of dispositions of some sort, taking place perhaps during the first year of life, and yet not attributable to any process of learning or practice, which would have to be con-

[1] ¶The title of this section in the original is *Empirismus und Nativismus*, of which, therefore, the above is the literal translation. The writer enters here on the discussion of the two theories, which we have called the empirical theory (*empiristische Theorie*) and the intuition theory (*nativistische Theorie*). (J. P. C. S.)

sidered therefore in the sense of intuition (*im nativistischen Sinne*). Consequently, it is not a question as to the *moment of time* but as to the *ways and means* of origins and developments of this kind. Accordingly, the antithesis here indicated is based on a perfectly definite general notion and proceeds from the fact that we have to make a distinction between two modalities in the origin and development of the human organism. The nature of the organism as to the particular species to which it belongs and as to its specific hereditary characteristics is to be regarded as being fixed by one of these modalities, although we have practically no insight into it as yet. We may perhaps speak of the principles governing these entire processes as the *determinative* (*innate*) *laws*.[1] A thing is said to be acquired (*erworben*) as distinguished from that which is determined innately (*dem bildungsgesetzlich Bestimmten*); but of all these acquired characteristics the only ones that need to be considered here are those that are perfected by *practice* and *learning*, with the essential features of which we are familiar. These developments are evidently dependent on certain characteristics of the central nervous system in a way that is quite exceptional, and are thus made possible.—Starting out with this general point of view, let us proceed now to inquire how these relations founded on intuition (*bildungsgesetzlich begrundete Verhältnisse*), on the one hand, and processes of learning, on the other hand, are both involved in the development of localization and of many other matters also.

Supposing we could obtain the answers to a question of this kind—one of which was what we might call the "empirical" solution and the other the "nativist" solution the distinction implied by these terms would be nothing like so simple as the distinction, for instance, between the emission theory and the undulatory theory of light. It would be an utter mistake to suppose that empiricism and nativism were two diametrically opposed hypotheses one of which was bound to be right and the other, therefore, wrong. We must assume, rather, that certain innate dispositions subject to the laws of genesis (*Bildungsgesetze*) may very well be present, but that they are modified and developed by practice and learning, thus constituting the foundation and the

[1] ¶The term used in the original is *Bildungsgesetze*. The writer constantly employs this word and its derivatives throughout the whole of the subsequent discussion. *Bildungsgesetzlich* is used sometimes almost synonymously with *angeboren* ("innate," "intuitive") or *primitiv* ("primitive"). Thus *bildungsgesetzliche Grundlage* seems to mean "innate basis," "basis of intuition," "innate substructure," "predispositions," etc. Some other combinations are *bildungsgesetzliche Einrichtungen*, *bildungsgesetzlicher Basis*, *bildungsgesetzliches Verhaltnis*, *bildungsgesetzliche Unterstutzung*, *bildungsgesetzlich bestimmt* (*festgelegt*, *fixierte*, *gegebene*, *vorbereitete*, etc., all of which refer in some way to this innate (anatomical or physiological) mechanism.)(R.P.A.)

starting point for the process of acquirement. Under such circumstances modes of behaviour corresponding to a nativist theory would be combined with those corresponding to an empiricial theory almost in any kind of way. Thus the task before us might be described in a general way by saying that *it consists in bringing out the true significance, first, of congenitally fixed predispositions and then of the processes ensuing from the general laws of training, with a view to obtaining some insight into the manner in which they coöperate and are interlinked with each other.* Moreover, it should be noted here at the outset that there are various matters that require to be kept separate from one another, simply because they are different owing to the mere fact that the congenital predispositions involved in them are not of the same nature and significance; and therefore they can be, and ought to be, examined independently in this respect. There are, in fact, three cases having to do with the relations of localizations that may be conveniently distinguished: namely, (1) *the arrangement of the directions* of objects seen in the field of view, (2) the relationships between the two eyes known as *fusion (Synchyse) and correspondence*, and, lastly, (3) the *determinations of depth or distance.* A fourth case which might be added is the obedience of the *ocular movements* to regular laws, for while this certainly is a different matter from the others, still it is related in many respects and may be conveniently included along with them.

As the subject has to be treated with some care, it will be advisable to outline a *modus procedendi*, which I trust will not weary the reader too much. Thus, first, we shall inquire, only in the most general way, whether innate predispositions *(bildungsgesetzliche Grundlage)* can be assumed at all in the questions under consideration; and then, second, we shall see whether more precise ideas can be obtained as to their nature, and if so, what these ideas will be.

In the first place, with reference to the *arrangement of the directions* of the things seen in the field of view of one eye, we know that it corresponds approximately to the configuration of the images on the retina. This relationship existing in normal adult vision might be said to be the result of experience only without any support from intuition, provided that from the very beginning, for example, a certain discriminating quality (or local sign in LOTZE's way of using that term) were characteristic of the impressions at each place on the retina whereby one place could be distinguished from another. These local signs, however, should enable us to discriminate merely between the individual impressions, at the same time without promoting or preparing the way at all for a definite configuration corresponding to that of the objects themselves. Now this would be the case if the local

signs of the various places on the retina had absolutely irregular and unrelated characteristics, especially if these characteristics did not vary continuously from one retinal point to the next. Nobody, I fancy, would consider such a state of affairs to be likely. For, ignoring the fact that there would be no object whatever in an unsystematic intermingling of the stimulations of the various retinal places, either with reference to some psychic peculiarity or in any other sort of relation and that, therefore, it would have to be regarded as highly improbable, I should say that it was beyond dispute that the human faculty of learning would be utterly inadequate for the task of trying to systematize empirically such an enormous number of absolutely disconnected tokens. Thus there cannot be any doubt as to the coöperation here of some intuitive (*bildungsgesetzlichen*) mechanism; and a characteristic thing about this mechanism in the most general form it could possibly have would be that the effect of stimulating the various parts of the retina, even in the central regions, would cause a continuous variation from one place to the next, which somehow is at the bottom of the subjective space-configuration, producing a configuration of the individual impressions in conformity with the places on the retina. This may occur at once as a matter of necessity, but at any rate it is greatly promoted and facilitated in preference to any other mode of configuration.

The conception thus reached must be immediately limited in one way that is not unimportant. The arrangement of the perceptions in the same order as that of the retinal points to which they belong may certainly be regarded as a matter that is settled by intuition (*bildungsgesetzlich festgelegt*); therefore, whatever is imaged on the two consecutive points a_1 and a_2 will be seen in directions r_1 and r_2 that at all events do not differ very much. If a retinal point c happens to be within a closed curve p, this curve will determine a corresponding closed aggregate of directions (forming the surface of a cone) within which the direction corresponding to the point c will be comprised. We may infer therefore that the *positional* arrangement of the things seen is something settled by intuition. But whether this is also the case in regard to the quantitative relations of the differences in direction between the various points, is another question entirely. There is nothing whatever to make us assume this. On the contrary, from all that we know at present, there is much more reason to suppose that the more precise quantitative determination of these relations is a matter of experience.[1]

[1] Accordingly, if we take account only of those matters that may be regarded as fixed by intuition, the configuration in the field of view would still be capable of being varied much in the same way as a picture made on a sheet of rubber could be deformed by stretching the rubber differently at different places.

The special conditions of vision are responsible for minute variations of the space-determinations from the regular and simple anatomical relations, and it is these little discrepancies that seem to indicate that an exact quantitative determination is probably the result of experience. By proceeding on the assumption that these quantitative determinations are developed by experience, it is possible to find a reasonable explanation not only for the deviation of the apparently vertical meridian and for the so-called HERING-HILLEBRAND horopter deviation, but also for a series of other idiosyncrasies of the eyesight.[1] It seems scarcely credible that the visual relations could be determined in such detail merely as the result of phylogenetic development and inheritance. Besides, binocular perception of depth shows in a very convincing manner how space-determination that is dependent on definite physiological relations can be affected *quantitatively* by all sorts of circumstances. However, this question is one that cannot be answered unequivocally at present; and if anybody chooses to argue that all the aforesaid relations are fixed by intuition, we are obliged to grant that he cannot be flatly contradicted.

Suppose we consider now the *relation between the two eyes*; then there will be two things which must be kept distinct. It is a very important fact, as we have already stated, that there is no difference between the impressions in the two eyes that can in any way be connected with their actual positions of adjustment. Unquestionably, we are justified in assuming some basis of intuition for this fusion *(Synchyse)* of the impressions in the two eyes. Here we ought to keep in mind certain well-known anatomical relations first of all. Some of the fibres of the two optic nerves cross each other in the chiasma, the result being that the right halves of the two retinas are connected with one half of the brain, and the left halves with the other half. The consequence is that the central connections between the two eyes are placed thus in peculiar relations to each other such as do not exist in the case of any other organ of sense—at any rate they are relations of such nature that the rigid distinction between right and left to which we are accustomed elsewhere does not exist here.

There is another simple consideration that may be mentioned in this connection. Suppose that from the very start there was a striking and perceptible difference between the impressions in the two eyes; suppose, also, that at the same time the movements of the two eyes were independent to begin with, and especially that, when light was allowed to fall on one eye and not on the other, the effect would be to produce movement in the stimulated eye without causing the other eye to move: then it would be hard to see how it would ever be possible to obtain the kind of localization that is actually realized by us. We might

[1] It is assumed then that the relations of correspondence between the two eyes are not absolutely fixed exactly, at least in case of the excentric parts of the retina, but are capable of being modified to a certain extent by experience. This assumption is also rendered likely by the facts adduced here, which is a matter that we shall mention again immediately

rather expect that a localization would be developed of the same sort, for instance, that is peculiar to the impressions we get by touching things with both hands; in other words, a localization in which the impressions in the two eyes, instead of being *fused* (*synchytische*), would be appreciated differently. When all the facts are taken in consideration, it is beyond question, it seems to me, that the fusional (*synchytische*) nature of human vision has an intuitional basis, at least to the extent that there are absolutely *no* contrivances in the eyes themselves that are designed to enable us to discriminate between the two of them or that are intended to connect with the ideas formed by the sense of touch as to the places where the two eyes are. Unquestionably, the perception of space is everywhere pre-arranged (*vorbereitet*) in a certain way by a definite central representation of the peripheral sensory surfaces and perhaps also by reflex relationships besides. Consequently, when the known anatomical facts are taken into account, there cannot also be any doubt as to the fact that the peculiar differences between the senses of sight and touch in regard to localization (the fusion of the optical perceptions) are manifested also by a difference in those preliminary preparations for it—in other words, that this specific mode of functioning is pre-arranged (or provided for) by an innate mechanism.

As we have said already, this question must be kept distinct from the other one, as to whether the relations of correspondence are also established by intuition in any specific and detailed fashion. In favour of such an assumption, we might, perhaps, instance the tenacity with which the primary relations of correspondence [see page 579] persist when the adjustments of the eyes are abnormal, together with the fact that this tendency can be shown to exist in cases in which, presumably, vision has never occurred with the eyes in normal adjustments. At the same time we might allude to the very probable, if not absolutely proved, inferiority of the modified as compared with the normal correspondence; and, lastly, to the comparatively quick recovery of normal correspondence after an operation has been performed to correct the maladjustment of the eyes (see page 591). And yet, on the other hand (as has been repeatedly said) a special circumstance must be kept in mind that tends to promote normal correspondence, the fact, namely, that this connection exists between the two retinal places where the visual acuity is keenest. Besides, we might easily conceive of innate predispositions such that, instead of a given cone in one retina being placed immediately in touch with a definite cone in the other retina, a certain freedom was allowed in this connection. Accordingly, if we may venture to suppose that a certain

preparation is also made for the relations of correspondence (which I may add is an hypothesis which I myself have always been inclined to favour), still it must be admitted that it will certainly be difficult to say how far its significance extends; that is, to specify the degree of precision with which the retinal points of the two eyes are placed in communication with each other.

It seems to me that there is more justification for assuming that there is some such freedom or latitude when we remember that it exists to a certain extent in mature vision. This would be the case if the relation of equality of direction could be modified within certain limits by the nature of the observed objects. In fact, on the supposition that the point a on the retina of one eye is co-directional with the point a on the retina of the other eye, it is likely that for certain peculiarities in the images the impression at a may be fused (*verschmelzen*) with, that is, may have the same direction as, that at the adjacent point a'. In this case, then, even points ordinarily having the same direction will necessarily give impressions different from each other (that is, different in direction), the result being that the whole relationship of equality of direction will apparently be shifted a little. I know that it is a moot-point whether this is actually true or not; still in view of the very convincing arguments on this subject presented by HELMHOLTZ (page 450), I believe there is very little doubt about it. Besides, by not considering the correspondence as being absolutely fixed in the very beginning, we are enabled to account for some of the special matters alluded to above (such as the deviation of the apparently vertical meridian and the so-called HERING-HILLEBRAND horopter deviation) as being due to an empirical development, in accordance with the explanation given in the text.

The third point to be considered has reference to the *perceptions of distance*; and here the situation is quite different.

Those things that we mean ordinarily when we speak of empirical factors may be eliminated here at once, and so it is only with reference to the binocular perception of depth that any innate basis can be conceived. The conditions of binocular perception, as we have seen in the preceding pages, are also exceedingly complex, and, moreover, a whole series of circumstances undoubtedly of an empirical nature are involved and have a very important part to play. It follows at once that if we were to assume an innate basis of comparatively simple form for the perception of distance, as might perhaps seem plausible in view of analogous relations, we should be led to attribute innate determinations to the perceptions of distance that would be utterly misleading and incorrect—that are fictitious and without any real existence. An instance of this kind is HERING's attempt to attribute positive depth-values to the points on the nasal halves of the two retinas and negative depth-values to the points on the temporal halves. We can test monocular vision and prove conclusively that there are no such depth-values. The impression of depth-combinations of this sort can only be really produced by a perfectly definite coöperation

between impressions in the two eyes depending on complicated conditions.

It would, indeed, be possible to suppose that it is some innate predisposition that determines whether, in case there happens to be a definite combination of impressions in the two eyes, an impression of distance will arise in some absolutely definite way. But when we consider that the result (namely, the impression of distance) depends on a peculiar combination of stimulations of two localities, and, moreover, that it is determined by a series of factors of an entirely different sort that are certainly empirical (those factors, namely, that determine the apparent distance of the point of fixation), it must be admitted that such a relationship as this is utterly beyond any intelligent idea that can be formed of innate proclivities by analogy with any known facts.

Thus the assumption of a definite intuitional basis for perception of depth breaks down, because, without either coming squarely in conflict with the observed facts and hypothecating purely fictitious depth-values that have never been proved to exist, or making assumptions, which have no foundation or analogy anywhere else, as to what ought to be determined by intuition, we are utterly unable to state what sort of basis it might be and what it could determine or simply prefigure (*vorbereiten*).

Granting then the exceeding improbability of the existence of any special innate basis for the localization of distance, we can now go a step farther and say that we can think of this phase of the perception entirely from the empirical point of view without having to attribute anything incredible or inconceivable to the faculty of learning. Perhaps it is scarcely necessary even to say this in speaking of the empirical factors strictly so-called, as it is obvious from the significance actually attached to them. But the remark applies especially to the relations of binocular perception of distance; for in their case we must remember that a certain basis would be found in those same dispositions which we were led to assume with respect to the localization of direction and the relationship between the two eyes. It would merely have to be learned how certain relations occurring in *these* respects were connected with the ideas of distance; in other words, it would be necessary to acquire a certain modality, a principle for appreciating these ideas. We need scarcely wonder at the actual accomplishment of this task, considering the certainty with which our impressions of distance are ultimately determined by other circumstances that are unmistakably empirical.

Taking all the facts into account and weighing them without bias, I believe that as a result of the previous argument we are able to come to a definite conclusion in regard to the matter under discussion, which

is very probably correct, although of course we cannot be absolutely certain. *It may be surmised that there is some intuitional basis for the distribution of the visual impressions side by side in the field of view in the same collocation as that of the retinal points, and doubtless also for the definite combination of the impressions in the two eyes known as synchysis and correspondence. On the other hand, it must be considered extremely unlikely that there is any such basis for perception of distance.*

When we endeavour now to form a more precise conception of these innate dispositions whose existence, according to the previous argument, would appear to be likely, the inherent difficulties that are encountered are much greater; and closely related to this same matter and fraught with similar difficulties is also the question as to how a process of learning is connected with these dispositions. It involves especially trying to picture the psychic nature of a vision that has not yet been developed, such as may be conjectured to exist in the case of a new-born babe. Needless to say, in making any assumptions whatever as to these relations that are absolutely incapable of direct observation, we shall be treading on very unsafe ground.

We may begin by considering LOTZE's theory of *local signs*, which, on account of its schematic construction and consequent simplicity, deserves to be put first. As a starting point for the spatial configuration of that which is perceived, let us imagine any kind of tokens, non-spatial at first, attached to the sensations whereby a differentiation is produced between the sensations mediated at different places on the retina. The local sign belonging to each retinal point would have to be conceived as being innately determined, and, according to the previous argument, there would have to be some characteristic of the local signs that varied continuously from one place on the retina to the next; moreover, we should have to suppose that some kind of distinguishing relation exists between the local signs of corresponding points. On some such basis as this we are bound to acknowledge that it is apparently conceivable to some extent how the perception of space might be acquired. For instance, suppose in the first place that all visual sensations are provided with local signs varying by continuous gradation; and, secondly, suppose that the idea of space is given to begin with (as must certainly be assumed): then we may imagine that we gradually learn to associate the sensation having a given local sign with an object lying at a definite place in space, and this process might be completely correlated with the methods of learning with which we are acquainted. Still, it must be admitted, there is a certain vagueness about this conception, due to the fact that we are dealing with matters that we are unable to render concrete. The tokens here spoken

of as local signs are not manifested as conscious phenomena at any rate. The difference between an object seen above and one seen below consists simply in the fact that we do see one of them above the other. If there is any *other* distinction enabling us to connect and associate this difference of spatial appearance that could be considered as being at the bottom of it, we are not able to bring it to consciousness at any rate. Considered as a psychic quality, the local sign is something intangible, purely fictitious. It could very well be argued too that, where associative connections actually do occur, as in this case, even if we do happen to be mainly interested in the final phases of the chain of association, still, by a little practice and concentration, we can generally succeed in bringing the initial phases to consciousness also.

It would seem, therefore, extremely dubious whether the local sign can be supposed to be a psychic quality. That being the case, the question might be asked, whether there might not be some possible form of learning in which, by the general laws of association, certain physiological processes, not represented in the consciousness, were associated with psychic phenomena or with other physiological phenomena having correlates in consciousness. On the assumption that such is the case, the fact of a given physiological process repeatedly concurring with the apprehension (*Wissen*) of an object located at a particular spot would be sufficient to cause that special process to arouse at once the impression of a body's being at the place. Indeed, I consider it very likely that a form of learning or development of this sort may occur.[1] Regarding the matter in this light, we should not have to insist that the local signs must be tokens manifested in consciousness. All that would be required of them would be to signify some intrinsic difference between the stimulations of different places on the retina—a difference which might naturally be supposed to be primarily anatomical.

By enabling us to think of the local signs as being physiological, instead of psychic, tokens, this physiological conception of the processes of learning involves, it seems to me, a very fundamental modification of the theory, which, while it does indeed help us to get rid of some of the objectionable features, does not eliminate all of them; for new difficulties crop up also in this modified form of the theory of the local signs, some of which, at least, are very significant. The one that is of less importance, perhaps, but most obvious and most frequently insisted on, is the fact that we are not able to think of a visual sensation except

[1] The phenomena that can be observed in the case of learning of movements are especially calculated, I think, to cause us to attribute general properties to the central nervous system involving just such a mode of development as that indicated here. However, I cannot attempt to discuss these matters at present.

with reference to some place in space. That psychic attitude (*Verhalten*) which was assumed to be the starting point of learning, namely, an optical "sensing" freed from spatial attributes—it too is something that cannot be demonstrated. This is indeed pertinent, no doubt, but how much weight to give it is difficult to say; for one can hardly venture to assert that it would be impossible for a connection of this kind, even if it were one developed by learning, to acquire gradually a degree of fixity such as apparently does exist here. But it seems to me that there is another point of more importance.

The evidence for such a process of learning as that which has just been outlined would be perfectly clear, provided there were numerous instances in which it could be shown that a definite physiological process or state was associated with our apprehending in some other way the fact of a body's being situated in a certain place. Accordingly, this particular mode of learning presupposes that we have some knowledge of the positional relations of objects derived from other sources. The latter might be traced to another organ of sense, such as the sense of touch, or even to impressions associated with muscular activity; and yet it might justly be argued that there is no special reason to suppose that these other organs are any more directly in touch with the ideas of space than the sense of sight. If therefore, for the sake of logical consistency, we should raise the question as to whether we obtained directly a space-notion from any of the senses, including the sense of sight, the only alternative as an intellectual starting-point, so to speak, would be the conception of objects capable of moving in space. But on such a basis as this we can scarcely see how the faculty of localization could be acquired. Even if the local signs are supposed to be tokens of sensation manifested in the consciousness, it is not clear what would make us connect them with space-relations or to associate a change occurring with respect to them with the idea of a motion.

Now according to any physiological conception of the local signs the difficulty here spoken of appears to be absolutely insuperable; for in order to acquire something in this particular sense of the word, that is, in order that physiological states *not* represented in the consciousness shall be associated with psychic phenomena, necessarily the latter would have to be regularly present at least. Thus, as stated above in formulating the nature of this sort of acquisition, an absolutely indispensable requirement would be a knowledge of the space-relations of observed objects derived in some other way.

Unquestionably, therefore, in view of these difficulties, we are forced to take in consideration the possibility of there being a direct

connection between our optical sensations and ideas of space depending on some innate basis.

This brings us into the domain of nativist ideas, and so we shall proceed to inquire how we are to conceive of space-determinations that are given by intuition, so-called *primitive* space-determinations. Owing to a matter which, while it is not capable of exact proof, cannot be seriously questioned, and as to which the most extreme views of the intuition theory are in harmony with those of the empirical theory, the assumptions relating to this question may be formulated more precisely. This circumstance is the fact that not only the knowledge we may happen to have of any special objects whatever but the knowledge of our own bodies also must certainly be regarded as a mental possession acquired by experience, and not innate. This being so, it follows at once (as has been shown by HERING especially) that the primitive determinations of space at first must amount simply to an arrangement of the visible objects with reference to each other and without any relation to the observer himself. Starting with this premise, we ought, however, to point out at the same time the fundamental difference between such space-determinations and those with which we are actually acquainted as mature human beings. Anything in regard to our optical sensations that is susceptible of proof, and especially anything of this sort that stands out in the form of a determination arrived at immediately and necessarily, turns out invariably to have some relation to our own body, and so for this reason it can never under any circumstances be identified with those primitive place-values. The mere fact that the impression of direction in developed vision is not determined simply by the retinal place itself, but that the so-called adjustment-factor (page 570) is always involved in it also shows that this is true. As was pointed out (*loc. cit.*), the thing that is seen is determined as a unitary consequence of the coöperation of these two factors. Thus the place occupied by the observed object is already partly determined in this respect by relations that certainly are empirical; and the only immediate way of qualifying it is by describing it as being a relation with respect to our own body. As has been observed, we are unable to form any idea of a non-spatial optical impression, and it may be said just as truly that it is impossible to think of perceiving a place that is not determined by its relation in space to our own body. Try as we will to conceive of a space-determination or place-value lacking in this relationship, and we shall merely exert our power of imagination in vain.

HERING, it is true, has endeavoured to represent this transformation (*Umwandlung*) in a comparatively simply way, by supposing that the idea of our own body was subsequently fitted into the innately

determined configuration of the observed objects. But this conception is found to be unsatisfactory and inadequate in very various ways. In the first place it fails to do justice to the psychological significance of the fact that the place-value (as determined by the combination of retinal place and adjustment-factor) is referred to our body. If, on moving the eyes, the retinal place varies along with the adjustment-factor in a definite way, and if then an object is seen to stay in a fixed place, the permanence of this position will make a powerful impression at once; and it is place in this sense that is the immediate and finished result of the spatial determination of the act of vision (see page 571). Therefore, it would be an utterly inapt description of the phenomena to say that while the place-values of the visual impressions generally varied, yet at the same time, owing to the altered adjustment of the eye, our body is fitted in the visual space in an altered manner, and that thus the idea is obtained of something stationary with respect to the body. If we wish merely to describe what is given in our consciousness, the only thing we can say is that the place of the individual thing that is seen remains unchanged as a final and unitary datum.

Moreover, it is impossible to identify the place-values of developed vision with these conjectural primitive values, because, as we have seen [page 583], owing to certain special conditions, resulting in a duplication of the adjustment-factor, two different place-values may belong to the same retinal place. If the idea of our body were subsequently fitted into the fixed visual space, such conditions as those just mentioned might even result in doubling this idea of our body. However, this is not what happens; but what it amounts to is that there are two entirely different place-values for the same retinal place or for two retinal places whose primitive place-values would have to be supposed to be equal, the difference between them being exactly of the same sort as that between the place-values of two different retinal places. The effectual part that the adjustment-factor has in producing the place-values that are characteristic of developed vision is manifest here also.

Finally, by carefully considering the significance of the place-value of mature vision from another angle still, the difference between it and a possible primitive place-value can be shown in the most positive manner. The space-determination that stands out in our visual impressions as a fixed element signifies a *direction* referred to a point in our body. This is the only way we have of describing the relation whereby the impressions on two immediately contiguous retinal places are continuously adjacent to each other under all circumstances. Thus a very characteristic feature of the space-perception

of the sense of sight is that its fundamental determinations cannot be regarded as being attributes of the "things seen" at all, but they represent relations with respect to a point which is not itself an object of optical perception. Thus once more the fact above indicated is brought out here with special clearness, namely, that the idea of a point that is not only not seen but is invisible enters as a factor in the space-determinations of our developed vision, from which by their very nature these determinations cannot be dissociated.

Accordingly, if the optical sensations are supposed to be endowed originally with primitive space-determinations, these latter must undoubtedly represent something thoroughly different from those with which we ourselves are familiar and which are expressed in our consciousness; and the only way the latter can be developed from the former will be through a process of complete remodelling, wherein the relative arrangement of all elements will doubtless be preserved, although the significance to be attached to each of them individually must be transformed completely.

The argument in regard to impressions of distance is perfectly analogous to the discussion of the impressions of direction. We have already shown that no congenital depth-values could be attributed to the retinal points, because it would be quite impossible to specify what they would have to be. But, aside from this point, just as was the case above when we were speaking of the arrangements of direction, here also the fact must be emphasized, that, no matter what the primitive depth-values themselves do signify, they are bound to be something utterly different from the determinations that are characteristic of mature vision. Determination of depth, as we know it in the vision of the adult,—the thing that varies when the point on the retina is kept fixed,—can only be described by designating the place on a straight line proceeding from a point in our body as being the distance from a certain given point in this line. The same idea is also expressed by speaking of a visual direction, meaning thereby the thing that does not change with change of depth. Thus the kind of perception of distance with which we are acquainted is likewise something that cannot be dissociated from the idea of a point which is the origin of the visual directions and also the origin from which the distances are reckoned. So in our depth-impressions also the idea is always implied of a point which is itself unseen. Hence, if we are to attribute a definite configuration in depth to the primitive visual sensations, we shall be obliged to conceive of these depth-values or distance-values as being something entirely different from anything in our ordinary vision.

If, therefore, we think of the primitive space-determinations in the way Hering did, and suppose (1) that the altitude-values and azimuth-values belonging to each point on the retina signify the configuration on a given (plane or curved) surface, whereas (2) the primitive depth-values relate to a dimension normal to this surface, obviously these determinations will be fundamentally different from those with which we are acquainted, and which are represented by directions all radiating from one point and owing their significance to that fact.

Accordingly, if we assume that there are innate (primitive) space-determinations, it is obvious at all events that they cannot be anything permanent or fixed. Undoubtedly, this is an important fact in regard to the special question under consideration, as to the innate dispositions that form the basis of our perception of space. This substructure can scarcely be considered as something which, having supplied the starting point for the process of learning, has then been completely transformed and ceased to exist. At any rate it is far more likely that we are concerned with dispositions that are permanently maintained and that continue to play their part in the function of developed vision also. Supposing this to be so, we might expect to find that the main evidence for the existence of such predispositions was, not so much the fact that certain primitive space-determinations were originally attached to the visual sensations, as that there were certain permanent dispositions which were just as determinative for primitive as for developed vision. We should have to think of such dispositions as being given by anatomical or physiological mechanisms of some kind, to which no fixed psychic correlate was attached, but which, rather, might be connected with psychic phenomena in various and varying ways. For instance, one can easily see how a disposition of this sort might be so contrived that the sensations connected with the individual retinal points would be arranged in every case in harmony with the configuration of the retinal points themselves.

The necessity of searching for these native predispositions in certain anatomical or physiological relations, and not in any definitely specific psychic qualities, is more clearly manifest when the other point is considered which remains to be mentioned and which likewise indicates the likelihood of there being some innate basis; and that is the connection between the two eyes. There certainly appears to be a probability that even in primitive vision the impressions made by the luminous stimulation of two corresponding points would agree as to their place-values to some extent. Yet when we speak of points on the two retinas as being related to each other in this fashion, the connection which we are thus led to assume is not very clearly defined. The phenomena of rivalry already tend to indicate that there must be a certain closer connection between the stimulations in a single eye, whereas a

general difference must exist between the stimulations of the two opposite eyes. This is brought out still more clearly in the regional formation of the rivalry (see page 580), as developed under certain conditions. We cannot avoid assuming that there is some such difference as this in order to explain also the relations of perception of depth (irreversibility of relief). Moreover, flicker-phenomena should be kept in mind here, in which SHERRINGTON has shown (see page 530) that in the disappearance of flicker a similar close connection is manifested between the stimuli affecting the same eye. And, lastly, it should be recalled that under special conditions it is even possible to discriminate directly between impressions in the two eyes (see page 491). Ordinarily, we are not able to do so or to recognize any distinction between sensations mediated by one eye or the other; and thus again we are compelled to think of the difference undoubtedly existing between such stimulations as being determined by anatomical or physiological relations, which in their remoter effects may be psychically appreciable, but which cannot be noticeable all the time. It is just in connection with the association between the two eyes that to a certain extent we are forced to regard these innate predispositions of which we have been speaking as anatomical or physiological affairs, which cannot be described, except partly at least, in terms of psychic correlates. Moreover, by looking at the matter in this way, we shall be more apt to see (what we had already been led to suspect) that the relations of correspondence (equality of direction) are not any absolutely fixed relations.

Perhaps it will not be amiss to mention that, in my opinion, it is just in connection with this matter of the association between the two eyes that the difficulties as to the special nature of these supposedly innate relations, and especially as to their anatomical significance, are extremely great. The close connection between corresponding points, on the one hand, coupled with the thoroughgoing difference between the impressions in the two eyes, on the other hand, constitutes an exceedingly remarkable situation, to which there is no analogy in the case of any of the other senses. To endeavour to clarify such obscurities as these would be idle until we know more about the processes of the central nervous system, which we are far from comprehending at present.

Thus after carefully analyzing the space-determinations that might possibly be attributed to a primitive (innate) vision, we learn that their significance is very limited: first, because the space-determinations of developed vision would have to be quite different from them and could only be derived from them by a complicated process of remodelling; and, second, because the actual basis for such a development would have to be found not so much in those primitive place-values themselves but rather in certain material relations, either

anatomical or physiological. Nor are we any better able to form a distinct idea of the place-values or space-determinations that might possibly be attributed to primitive vision. Perhaps we might conjecture that the general idea of space, in which we know no variation, is at the bottom of them also. But while we may speak of them, as in case of the sense of touch, as being superficial, indefinite as to depth, etc., the endeavour to form any kind of conception of them will lack solid support and is simply an unnecessary strain on the imagination, in my opinion.

We may now bring the discussion to a close. While it may have raised some questions impossible to answer at present, I venture to think we can come to some conclusions that may be considered to a certain extent a satisfactory outcome of the task we set out to perform. First of all, there cannot be any doubt as to the fact that both innate substructures and a process of development based on experience each contributes to establish the laws of localization as they are known to exist in the vision of adults. Moreover, the significance to be attached to each of these elements can be defined somewhat more precisely with considerable probability at least.

In particular, the correspondence between the configuration of the things in the visual field and that of the retinal points may be traced to congenital (innately fixed) relations. The characteristic feature of this perception, namely, the absence of any difference between the impressions in one eye and those in the other, or rather the fact that the space-determination of all such impressions is unitary, may also be just as well attributed to the same origin. Moreover, we may suppose that the normal relation of correspondence has been selected through the medium of a definite primitive mechanism (*bildungsgesetzliche Verhältnis*) in preference to any other that is very different from it. But it is extremely unlikely that this is an absolutely exact relationship; on the contrary, there is reason to suspect that the innate dispositions are such as to allow a little scope for empirical development, although we are unable to estimate the range and significance of it in any quantitative way.

While it is possible that in these respects primitive or congenital dispositions contribute to a certain extent to the formation of the laws of localization, those space-determinations actually obtaining in developed vision are undoubtedly acquired, as shown by the fact that they are all relations with respect to the observer's body, and cannot be separated from the idea of the body itself.

Moreover, it is extremely unlikely that there is any definite innate basis for anything that has to do with perception of depth (or distance),

because the conditions in this case are so complex and so various that we can scarcely assume any fixed basis for it whatever. At all events, there is nothing in this relation that requires an actual substructure of any kind.

Again, in regard to the conditions of localization of direction and with respect to the binocular relations, we must consider that these primitive substrata are only of limited significance: first, because the origin of the more exact quantitative development is more likely empirical than innate; second, because the relation of correspondence is presumably no perfectly rigidly fixed affair, but admits of a certain degree of freedom, although, indeed, the extent of it cannot be more precisely defined at present; and, lastly, because the relations of correspondence are also not unalterably fixed, but, on the contrary, different relations with respect to the impressions of direction can be developed with comparative facility. It is true, similar variations seem to be possible in other respects (perception of depth), but we may recall (see page 592) that the difficulties encountered then are greater.

While, therefore, it may be inferred that the spatial optical sensations of undeveloped vision are already endowed with space-determinations, these latter cannot be identified with those space-determinations that are peculiar to developed vision. We are obliged, rather, to think of the space-determinations of mature vision as the result of a process of acquisition, the origin and foundation of which is to be found in dispositions in which those primitive space-determinations are inherent also. A comprehensive way of expressing it would be to say that developed spatial vision is the result of a process of learning, for which, however, certain preparations are made by innate dispositions, or which is initiated by them, as we might say.

The learning itself must undoubtedly be regarded as a process of physiological development, as is shown by the fact that the very dispositions that initiate the process are partly non-psychic in their manifestation and are probably to be considered as anatomical and physiological mechanisms. Apparently, there is nothing unusual or exceptional at all about this whole process, especially as to the immediacy and positiveness of its results. There are numerous analogies to it in other regions by which we can recognize the significance of similar physiological relationships acquired by development.

Aside from the matters of which we have just been speaking and about which there is some question as to the significance of the innate substructures, we are not able at present to reach any positive conclusion (1) as to the psychological nature of those space-determinations that may possibly be attributed to primitive vision; nor (2) as to the special nature of the anatomical or physiological mechanisms of

which those innate bases must be supposed to consist; nor (3) as to the kind of physiological substratum that has to be assumed for learning itself. Doubtless some of these problems are beyond the range of investigation altogether, and others would no longer be within the province of physiological optics, but would belong to other fields of inquiry, especially to the physiology of the central nervous system.

7. On the Origin of the Laws of the Ocular Movements

According to our plan as outlined in the beginning, we have yet to discuss the laws of the ocular movements from the same points of view as the laws of localization. In an entirely general way, here also we have to inquire as to how and to what extent the way has been prepared in advance by innate dispositions for the orderly processes that can be observed in the movements of the eyes, and as to how far these processes are dependent on individual experience or training. It should be observed, to begin with, that there are two things here which it will be well to distinguish. The regular movements of the eyes with which we are familiar are first of all *coördinated* with each other; that is, the ocular muscles cannot act except in perfectly definite combinations, all other combinations being impossible. In the second place, these orderly processes are related to the manner in which these movements are released; that is, to the *intentional* factors, as they may be called briefly. We know that normally each movement can be evoked only in a specific way, which also involves a certain limitation of the range of phenomena to which our motor intentions may be directed.—A few general preliminary remarks will be useful, first, as to how we can conceive of the possibility of our learning or acquiring a movement and developing a law of movement wholly without the aid of any innate predisposition; and, second, as to what assumptions we are justified in making in regard to such predispositions and their occurrence and significance, on the basis of facts that are known about other things.

So far as the first point is concerned, it is obvious that a movement cannot be acquired unless it is possible to bring the necessary muscles into play. Under such circumstances, supposing there really were no specially prepared formations of any kind, this would be like "beating about the bush," so to speak, without any plan or system. The disconnected movements of little children at play would be something like this mode of movement; or perhaps it might be compared also in some ways with the tentative efforts of grown persons in trying to acquire an entirely new movement such as the pronunciation of a foreign word. In order to acquire a definite movement in this way, evidently some-

thing else is necessary, something that can be most easily illustrated in the case just mentioned of trying to utter a word. If, by a mode of innervation that is more or less the result of accident at first, the desired word is once pronounced, evidently it may be possible to retain this accomplishment. In the first place, by permanently retaining it or by continually recovering it, a definite innervation relationship will be developed into a fixed coördination; and, in the second place, this movement will be so associated with the idea of its accompanying consequence that the intention of obtaining this result can release or evoke the requisite mode of innervation. Just how this occurs, and what properties of the central nervous system are involved in it, are questions which we cannot unravel at present, and which at all events do not need to be discussed here. The only thing that is certain is that the impression of having accomplished the desired result at the given moment, of having executed the movement correctly—or, to express it still more generally, any sort of significance that distinguishes between the movement itself and its result—will be a controlling factor in releasing the movement. Thus the general basis for the development of voluntary coördinated movements may be said to be the fact that, when the effect of a movement possesses a certain *outstanding significance*, the movement concerned is retained and associated with the impression of this effect; that is, it is established both coördinately and intentionally.

While certain possible ways are thus afforded for acquiring a movement without any innate preparatory groundwork whatever, on the other hand, there cannot be any doubt also as to the general biological possibility of special predispositions or as to their actual existence. The earliness with which new-born quadrupeds learn to stand and walk is often cited as a striking proof of this fact. Countless other illustrations might be given that are perhaps even more conclusive (it will suffice to mention only one of them, namely, the singing of birds). Thus there are plenty of facts to show beyond any doubt that the execution of quite definite movements may be intuitively prearranged and prescribed under certain circumstances with a precision extending to the most minute details. Moreover, these innate dispositions are obvious to us to some extent at least, especially inasmuch as the coöperation or simultaneous activity of two muscles appears to have its anatomical substratum in the close juxtaposition of the origins of the appropriate nerves, although the basis of a given temporal sequence is at present obscure.

Another fact in this connection is that a quite specific release of such movements may be established intuitively in the wide range of

phenomena known as *reflexes*. It is easy to understand that this would be the simplest method of preparing in advance for the deliberate or intentional association of the movements involved. If the movement is due to a reflex and the circumstances in which it occurs are such that the effect of it possesses that outstanding significance alluded to above, the conversion of the reflex relationship into an intentional one will follow at once. Thus, for instance, if the appearance of a bright object on the right-hand side of the visual field is the occasion of a reflex movement at first which causes the eye to turn to the right, this is a very simple way of preparing for a development that will result in the eyes executing the same kind of movement when the attention is diverted to an object over there with the deliberate intention of fixating it.

Let us consider now the laws of the ocular movements, first with reference to the question of *coördination*. It will be convenient to adopt the usual classification of the subjects that are to be discussed, namely, the *laws of binocular fixation* and *constant orientation* and LISTING's *law*. First, as to the law of binocular fixation, it might be natural to think that this was the one that was most apt to be acquired by training; for that outstanding significance referred to above as being the decisive factor in the acquisition of movements is undoubtedly characteristic of accurate fixation. The fact is that any other adjustment of the eyes results in binocular double vision; and it is natural to suppose that this is generally, if not always, felt to be an annoyance. According to what was said above, it might be assumed therefore that the innervations varied without following a definite rule of any kind until the two eyes were both directed toward the same point, but that as soon as that happened, the requisite innervation would then be established. Undoubtedly, in my opinion, it might be theoretically possible for binocular fixation to be developed in this way, but I must say that it seems to me extremely unlikely that this is the sole basis of its origin. If the possibility of innate preparation for specific movements exists at all (as we have found to be the case), and if situations of this sort are frequently met with, it would be hard to understand why a foundation of this kind would not have been developed for such a typical and comparatively simple relationship as the one we have here. Moreover, special facts can be adduced that tend to support this assumption in a remarkable way, even if they do not absolutely prove it to be true. In the first place, there are the observations that have been made of the ocular movements of babies soon after they are born. It is true, there is much discrepancy as to details in the reports of these observations. Still it seems to be established that a predilection for associated movements of

the eyes can be noticed at a very early period, and that the so-called atypical cases are of only rare occurrence.[1] Moreover, sometimes one eye becomes blind, and then the associated movement of the two eyes gradually deteriorates until at last it almost ceases entirely, but the process is very slow and usually goes on for a number of years. Thus it must be admitted that there is a certain discrepancy between the rapidity with which the normal relationships between the two eyes is developed and the difficulty of losing it after it has once been established, which makes it very likely that there is an innate basis for it. Finally, anatomical investigation reveals a quite definite and peculiar arrangement of the nuclei of the motor nerves,[2] the significance of which, while not perfectly clear in every respect, must undoubtedly be that this arrangement is favourable for specific combinations of innervations.

The theory developed above may easily be put in a still more definite form, although there will be less certainty about it in many respects. If we start with the fact that normally the impulse to execute an ocular movement of any kind is generally the result of the intention of fixating a point which is seen excentrically at first, it may be assumed that impulses to move the eyes will be guided by the spatial characteristics of the object to which our attention is directed at the time and which it is our intention to fixate. This being the case, it may be further conjectured that that duality which is peculiar to the spatial determinations of vision will be particularly in evidence in the impulses releasing the ocular movements, and that they will be determined partly by the *direction* and partly by the *distance* of the point that is to be fixated. In accordance with this, an innate mechanism may be primarily assumed to underlie the simultaneous activity of the muscles by which the two eyes are raised or lowered and turned to the right or left; and in this sense we might speak of a binocular mechanism of raising, lowering, and lateral turning, whereby all associated (or parallel) movements of the two eyes would be produced then by calling on these prearranged mechanisms in some kind of combination or other.

[1] In regard to this subject, see Hering, *Die Lehre vom binokularem Sehen*, p. 6.—Donders, Pflugers *Archiv*, XIII, p. 383.—Idem, *Archiv f. Ophth.*, XVII (2), 1871, p. 34.—Genzmer, Untersuchungen über die Seelentätigkeiten des neugeborenen Menschen. *Diss.* Halle, 1873.—Raehlmann und Witkowski, *Archivf. Physiologie*, V, 1877, p. 454.—Incidentally, the atypical ocular movements observed by Raehlmann and Witowski even in the case of older children (particularly when they were asleep) bring up the question as to whether the entire function of these motor mechanisms, irrespective of whether they are innate or acquired by practice, is not invariably connected with actual *vision*; whereas when vision is not taking place (for instance, when the eyes are closed), these mechanisms are out of action entirely, and then movements of the eye may occur in some other fashion, perhaps not involving these mechanisms at all.

[2] Concerning this, see Zoth in Nagels *Handbuch der Physiologie*, III, pp. 327 ff.

In addition to this contrivance, another one might be imagined which would pave the way in a similar manner for the release of symmetrical movements of convergence.—If, owing to a general tendency, we are disposed to assume innate substrata in the most thorough-going fashion, we shall be led to form some such conceptions as these. Whether these substrata really are of this nature or whether they prepare the way for the normal ocular movements in a less definite and detailed manner, cannot be decided at present with any degree of certainty.

But no matter how we think of them, or whether we attach much or little importance to them, at all events there cannot be any doubt that they are not equal by themselves to maintaining normal movements of the eyes (even in the particular case under discussion at present), but that in addition to them the general processes that are involved in all training are operative and indispensable for this purpose. There does not seem to be anything surprising about the fact that when the eyes are turned upward, the elevating muscles of the two eyes act in unison as the result of an innate mechanism. But it is hard to believe that it is owing solely to such a mechanism that they are able to maintain exactly those impulses of innervation that are required for fixating binocularly an object in an elevated position; and it would be still harder to believe that such a relationship should be, so to speak, self-sustaining during all the vicissitudes of growth and nutrition. We know too that corrective modifications can actually be produced, as is shown in a very simple fashion by the so-called *fusion movements*. By producing exceptional conditions (for example, by placing a prism in front of one eye with its edge horizontal so as to cause a difference of level between the two eyes), we see that the relationships of innervations will be altered at once, to such an extent indeed, that in a short space of time deviations of several degrees can be compensated. Undoubtedly, deviations arising without any external appliances will be abolished also in a similar manner; or, to state it more correctly, the *absence* of deviations of any kind, in spite of the manifold vicissitudes of growth and nutrition, is undoubtedly to be attributed to a persistent activity of the same relations that are found in the case of the fusions also. The fact that the right and left superior rectus muscles are innervated simultaneously and approximately to the same degree, may be attributed to an innate predisposition, but the exact determination of the relative amount of this innervation will, however, certainly be a matter for vision itself to control. In accordance with the general laws of training, the outstanding significance of binocular fixation might be supposed to furnish this visual control, as has been already intimated.

In regard to the above matters, another question that might be asked is, whether any deviations that are due to a special relationship can evoke just such a modification of the innervation as will suffice to correct them. It seems to me that any such assumption as this would be apt to encounter great difficulties with respect to deviations of level and torsion, and it is hardly necessary; for the deviations in question are minute, and therefore slight irregular fluctuations in innervation would be sufficient to produce the correct adjustment of the eyes, and hence it would be enough if just this innervation were maintained. In regard to convergence, obviously there cannot be any doubt of the existence of a direct tendency to fusion in the case of double images that are horizontally adjacent. We shall recur to the origin of this tendency.

In regard to the *law of constant orientation* and LISTING'S *law*, matters are far more complicated. In the first place, there are mechanisms connected with both of these laws which are obviously *useful* for the organism. Localization is greatly simplified by the law of constant orientation; the adjustment-factor (to use this term to express briefly what is meant) is reduced to a function of two variables instead of three. By virtue of LISTING'S law, the illusory movements of objects occasioned by movement of the eye are reduced to a minimum, which means that the law can be deduced from the principle of easiest orientation (see page 145). However, the fact that a given form of activity is useful to the organism does not give us in the first place an insight into the manner of its origin. Therefore, HELMHOLTZ himself properly remarks that the principle of easiest orientation must not be taken to imply how the law of rotation was actually developed. According to our general conception at present, the development of an activity that is beneficial to the organism may be considered as comprehensible if it can be transmitted by heredity. The facts mentioned might therefore be used to explain the laws of rotation, if we can assume that they are innately established in the case of the individual. The nature and extent of the assumptions that would have to be made in regard to this would again evidently depend on what individual development is capable of accomplishing with certainty. Now in regard to this it is plain that the circumstances of binocular double vision mentioned above *cannot* be taken into consideration here; for with any other laws of rotation, even if the law of constant orientation did not exist, a definite relationship might very well be established between the two eyes in which diplopia was excluded. On the other hand, it might be well to recall here the principle of least muscular exertion as advocated especially by FICK [see page 70]. Here, as everywhere else, the tendency of training will undoubtedly be to execute the movement so as to accomplish a given result in the most convenient way and with the least exertion. If this is the case, the (phylogenetic) development of a given motor pattern must take place in conjunction with the

development of that arrangement of muscles which is most suitable for that motor pattern. Conversely, the development of such a muscular arrangement will suffice also to guarantee the formation of the motor pattern in question. In regard, therefore, to training so as to make the least exertion, we can see how the laws of rotation were developed (as useful relations), even supposing that only the anatomical organization of the muscles is considered as hereditary and congenital; whereas a similar assumption in regard to the motor pattern itself appears to be unnecessary. This does not mean, of course, that the mode of motion is not also fixed or pre-determined by heredity, but simply that there is no positive reason for assuming that it is.

On the whole, therefore, the result of regarding the laws of rotation as being useful for the organism is that their development can be traced (1) to the hereditary anatomical organization of the muscular apparatus, and (in a manner that cannot be clearly outlined at present) (2) either to the inheritance of the specific motor patterns themselves or to the development of this movement as being the most convenient with the given arrangement of muscles.

The statement above made that the relations of binocular vision cannot be employed to explain the special laws of rotation, needs to be qualified to a certain extent. Thus, while the relationship between the two eyes cannot lead directly to the formation of a definite law of rotation, still it seems to me that it is undoubtedly calculated to promote the formation of some such law and to insure its maintenance. If a specific movement (such as turning the eyes upward to the right) must in any case be executed by both eyes in perfect harmony, undoubtedly it will be easier to get accustomed to a definite mode of motion than it would be if it were a question of a single entirely independent eye. More obvious still is the way in which the binocular relationship maintains and insures a law of motion that has already been developed. Slight deviations from the laws of rotation will always occur, owing to all sorts of accidental modifications in the processes of nutrition and conduction. The great majority of them will be apt to be non-symmetrical or one-sided, and will therefore be compensated by the mechanism of binocular vision. Naturally, there is little chance of coincident variations in both eyes where the above method of correction could not operate.

Thus far the ocular movements have been considered merely from the point of view of coördination, but now we must discuss their *intentional* (purposive) *features;* that is, the conditions by which they are released. The normal impulse to move the eyes is due mainly at any rate to the diversion of the attention to a point that is not in the centre of the visual field. As has been already intimated, it is very likely that this intentional "set" (*Intentionierung*) is pre-conditioned by (congenital) reflex connections. If bright or otherwise prominent (*e. g.*, moving) objects in excentrical parts of the visual field start a reflex impulse to turn the eyes at once toward that side, the intentional

coupling of the associated movement will thereby be prepared for in a simple and comprehensible way.

It seems rather more doubtful whether a similar assumption could be made for some sort of convergence mechanism, chiefly on account of the complicated conditions on which a reflex of this kind would have to be supposed to depend. In this case therefore it might appear more likely that the conditions of activity of the convergence mechanism were only gradually developed along with the whole formation of the perception of depth.

We might be inclined to base the assumption of a congenital convergence-reflex on the fusion-tendency above mentioned that exists in the case of horizontally adjacent double images. Only, it would still be necessary to account for the fact that the frequent release of voluntary movements under definite conditions leads also, as we know, to the development of a fixed motor habit that is similar to a reflex. According to the general conditions of binocular fixation, we can see perfectly therefore how it is that a comparatively fixed relationship is developed of such a kind that the movements required to get rid of double images (namely, more or less convergence) are initiated at once. This way of considering it is also supported by the fact that the fusion-tendency is not very strong and can easily be overcome by the mere intention of seeing double images.

The fact that in normal circumstances movements of the eyes can only be released by the intention of looking somewhere or other, evidently constitutes an important limitation; and the question arises as to the reason for it and the significance of it. At first there may not seem to be any point to this question, for it is plain that movements that do not permit of coördination cannot be intended. Obvious as this is, the question may still be asked as to whether movements in conformity with the laws of coördination cannot occur perhaps in some other way as well as in the usual way; that is, by other motor-intentions besides those of fixation and shifting the gaze. Now, as a matter of fact, it is quite possible for this to happen—which in my opinion is a fact of some importance. For instance, this is the case when we train ourselves to see an object singly or in double images (as we know we can easily do). With a little practice the convergence can be adjusted to the proper degree for seeing the double images. Here the intention of separating these images farther apart or of bringing them closer together so that they will ultimately blend controls the movements of the eyes just as directly as they are controlled otherwise by the intention of fixating a point; and here, as everywhere else, when the intention is set on a definite result, it releases the corresponding movement. Thus as soon as these special results of vision attract our attention for any reason, this intentionalizing of our ocular movements can lead to a very varied development. Now it seems to me that the importance of this fact is that it not only enables us to dispense with the assumption of innate predispositions of some kind by which our

motor intentions would be definitely limited in advance, but it renders such an assumption unlikely. It would seem rather to favour the simple notion that any result of motion that is feasible according to the laws of coördination may also become the object of a motor-intention. The reason why the movements of the eyes are ordinarily released simply by the intention of fixating is because those other results (such as double images) are of no significance for us and are merely felt to be annoying (unless of course we happen to be studying them in a scientific way).

Another thing in favour of this theory is the fact that under exceptional conditions erratic (*abweichende*) movements of the eyes can occasionally be voluntarily produced. By studying my own mode of vision, the diplopias, rivalry phenomena, etc., I have developed the possibility of executing typically one-sided movements. While I am gazing steadily at a point with my left eye, I can cause the right eye to deviate or I can bring it back again until I obtain exact binocular fixation. I cannot succeed in the same way when I fixate with the right eye and try to move the left eye by itself. In fact in this latter case I notice that my right eye does not stay steady; which may be explained by supposing that the one-sided movement is in reality a combination of an associated movement of the two eyes to one side together with a movement of convergence, as assumed by HERING in such cases. However, in the first instance (adduction of the right eye) this very characteristic and easily noticed behaviour was certainly not present; and therefore there is no doubt in my mind that this really is a case where a typical one-sided innervation has been acquired. Moreover, with divergence-adjustment I can voluntarily modify the relation between the levels of the two eyes by about two degrees, thereby shifting the relative heights of objects when they have been fixated, or approximately fixated, by "confusing" together the adjacent half-images in the two eyes.[1]

As against this view of the matter, it might be asked, why is it then that we do not succeed in learning how to produce differences of level between the two eyes? It has been intimated already that it is likely that differences of this sort in the vision of the two eyes are continually occurring. If that is so, the fact that we ought to be able to maintain these differences of level when once our attention has been attracted to them and concentrated on them, might seem to be one of the postulates

[1] WEINHOLD, *Klinische Monatsblätter für Augenheilkunde.* 1903.—BIELSCHOWSKY, Über die Genese einseitiger Vertikalbewegungen der Augen. *Zeitschrift f. Augenheilkunde,* XII. 1904. p. 545.—LECHNER, Abnorme willkurliche Augenbewegungen. *Arch. f. Ophthalmologie,* XLIV. 1897. pp. 58, 596.—PETERS, Über das willkurliche Schielen des einen bei Primärstellung des anderen Auges. *Klin. Monatsblatter f. Augenheilkunde,* 45. 1907. p. 46.

of the general faculty of learning which we have assumed. But it must be remembered that in order for us to maintain the result of a given movement, there must be something striking and clear-cut about it. If as the result of a coördination that has already been organized those differences or deviations are limited to such a small amount that they can scarcely be noticed, it is not surprising that we do not succeed in maintaining them or in producing them at pleasure. Whether this is also the case with respect to the coördination of the eyes of little babies, which is presumably far less rigid than that of mature vision, is a question that may indeed be very well open to doubt. Accordingly, there is some probability for supposing that the real reason for our *not learning* to execute such movements of the eyes as produce, for example, a difference of level between them is simply because there is none of that outstanding significance about these movements which has been mentioned above, and they are of no interest.

On the whole the result of this discussion is very similar to that which was reached in the case of the relations of localization; for the above considerations show, first of all, that while the regularity of the ocular movements as it is found to exist in the adult human being is at any rate partly due to innate or inherited dispositions, it is also partly due to individual training. The former must be regarded as responsible for promoting certain combinations of innervation. So far as this is concerned, it may be assumed, to begin with, that the controlling factor here also is the general principle of all motor training, according to which it is possible for us to maintain those modalities of movement whose effect possesses an outstanding significance for us. The thing that determines the continuation of this training is the fact that binocular fixation, and, generally speaking, binocular fixation only, does possess a marked significance of this kind. Possibly this may be connected with the fact that normally there are no movements of the eyes for which the intention to release them is anything more than a definite purpose of fixation. At all events, however, that circumstance must be regarded as responsible for the steadfast control that is undoubtedly needed for a permanent maintenance of the law of binocular fixation. Moreover, the tendency that can be seen everywhere to execute every movement as conveniently as possible and with the least muscular exertion probably has something to do with the development of the laws of motion. It would be obviously impossible at present to define more precisely the significance that should be attached to these two relations, namely, innate dispositions, on the one hand, and training, on the other hand. This would involve our being able to estimate quantitatively the amount of play that is permitted to the innate factors at the moment of birth, which for more than one reason is out of the question.

8. Historical and Critical Survey

In accordance with the occasion and plan of this exposition of the subject, it should not be concluded without showing how far the outcome of it is in agreement with the opinions that were advocated by HELMHOLTZ in his day, and how it differs from them and supersedes them. Such an inquiry will also be of interest for its own sake. However, these questions cannot be discussed without considering to some extent the opposite view which HELMHOLTZ disputed and refused to accept. Accordingly, without attempting to go into the whole development of the doctrine of the idea of space and of the space-apperceptions, I shall venture to give a somewhat general historical survey in which those aspects of these questions that are of most interest for us can be presented so as to limit the discussion to them.

Philosophical speculations on this subject had begun to be made, of course, long prior to the time of KANT (for instance, we need mention only LOCKE's distinction between primary and secondary qualities); but so far as our purpose is concerned, perhaps we may start here with him. KANT's contribution in this domain of philosophy is generally known as the doctrine of the *a priori character of spatial intuition.* While it would be impossible to discuss it fully here, the point that needs to be stressed is that this expression relates to two entirely different matters, which even KANT himself did not always keep as separate from each other as they should be kept: first, the evidence for, the type of validity of, certain propositions in regard to space, an *a priori* in the logical sense, and, second, certain psychological facts that are not altogether aptly denoted by the term *a priori.* The latter point is the only one that is of interest to us at present.

Concerning it KANT's doctrine maintains that the idea of space is a unitary and unchangeable element in our mental life. In my opinion, as I have already said, this proposition must be unequivocally accepted to the extent that all changes of sensory perception that can be experienced or imagined differ from one another only in the sense of our being able to perceive difference at the same place; the totality of the places themselves coalesces in a quite invariable way into just what is called space. This itself can never be conceived as diminished by the subtraction of any part of it or as increased by addition or generally as changed in any way at all. But now if space is the unalterable form of our sensory perception, given us once for all, the further question still remains as to the spatial order of the particular object of sensation; that is, as to the places in which it is represented for any given state of our organs of sense; in other words, the question concerning the special relations of *localization,* as we expressed it

previously. Since in any case localization must be governed by the momentary manner in which the external world affects our senses, that is, by the behaviour of the organs of sense and, consequently, of the brain, the problem here is evidently one that constitutes a part of the general doctrine of the psycho-physical relationship. While this theory seeks to explain in a perfectly general way how the phenomena of consciousness are either determined or affected by the physical processes of the brain, a special branch of this inquiry will consist also in trying to ascertain how the spatial relationship of what is perceived, that is, the *subjective* spatial order, is determined by the given external situation, particularly by the objectively existent spatial arrangements (of external objects and processes in the organs of sense and in the brain).

Even from the KANTian point of view, we insist that this question can and must be raised; and it is especially necessary not to be misled by the fact that we have spoken of an objectively existent spatial order, whereas, according to KANT, space is conceived to be a subjectively determined form of apperception. Even this latter conception cannot in any way vitiate the empirically fixed fact that external objects do affect our *organs of sense*, and that certain processes of the brain thus engendered produce specific spatial perceptions, just as they influence conscious phenomena in general. KANT's conception therefore does not alter in the least the distinction between a spatial arrangement which happens to be perceived by any person at the moment and that spatial arrangement which we speak of as being objectively realized. If we wanted to emphasize both the subjectivity of the idea of space (in KANT's sense) and our reality-concept (*Wirklichkeits-Denkens*) at the same time, we could do so simply by using some other expression to denote what is called here the objectively realized spatial arrangement.

How KANT happened to leave this question untouched—whether he simply overlooked it entirely, or whether he made the mistake of supposing that it was so obvious that it answered itself and that any discussion of it would be superfluous, or whether he deliberately left this problem alone as something foreign to his province at the time—we shall not attempt to explain. But at all events the more unreservedly we give our assent to KANT's theory of the unitariness and immutability of the idea of space, the more emphatically it must be pointed out that KANT not only did not answer this question but never once expressly propounded it. The two persons who, I should be inclined to say, first formulated this question in a perfectly clear fashion (absolutely on the basis of KANT's doctrine, by the way), and endeavoured to answer it were LOTZE and JOH. MÜLLER.

Starting with the assumption of the unitariness that is to be ascribed to the totality of that which is spatially perceived, conceived as the conscious content of the mind, in the first place LOTZE felt that it would

be inadmissible to regard the objectively given spatial arrangement of any nervous processes straightway as an adequate basis for the spatial relationships of what is perceived, that is, for the subjective spatial order. Rather, according to his view, the mind would have to construct this spatial order from some symbols attached to the individual sensations. Thus he conceived the theory of the *local signs* [p. 615] as peculiar tokens whereby otherwise similar sensations were differentiated from one another according to the place where they originated. While these characteristic signs were comparable to the special qualities of sensation by virtue of being peculiar to the sensations, still at least they were *not* supposed to be of *immediately spatial nature*; rather, they simply indicated to the mind the possibility of a differentiation that might serve as a basis for the development of a spatial arrangement of the individual elements of perception, that is, might develop a power of localization. There are two points in LOTZE's theory that are particularly worth noting. One of them is that, in addition to the local signs of the sensory impressions, the idea of space here is considered as something independent and given *a priori*, exactly in KANT's sense. The other point has reference to the further mechanisms of the development of the perception of space assumed in this theory. Instead of attributing to the sensations in advance a place-value proper, suppose some other kind of token was considered as attached to them so as to prompt the mind in some way to associate a place with the object of sensation; this would amount to regarding localization as a psychic activity coming under the general laws of psychic processes. In a perfectly natural way therefore, although LOTZE himself did not always have these relations steadily in mind, the theory of local signs leads us to think of localization, or the association of the local signs with place-values, as being something that gradually becomes established by learning. Accordingly, LOTZE's theory would seem to be the starting point and first step, strictly speaking, toward HELMHOLTZ's empiricism. Doubtless, HELMHOLTZ himself was fully aware of this relationship, as is shown by his use of LOTZE's expression "local signs," which may be said to be the cardinal conception of both LOTZE's theory and HELMHOLTZ's theory. However, while there is this connection between the two theories from an historical point of view, yet in order to see the facts in the right perspective, it should be stated that the real starting point of HELMHOLTZ's theory was quite different from the problem which LOTZE undertook and from the solution of it which he obtained. HELMHOLTZ had in mind a detailed investigation from the standpoint of the physiology of the senses; which is the essential thing to be remembered in order to obtain the correct historical connection.

Thus HELMHOLTZ was led to consider the great array of facts which went to show that localization was a complicated relationship acquired by experience and training, and variable in many ways. Trying to obtain an explanation and satisfactory formulation of these facts, he found that the only acceptable view prevalent at that time was the one in which the process of learning was considered as a psychological, not a physiological, operation: and so he was led to adopt LOTZE's conceptions as those which would enable him to formulate his results in the most comprehensible way and make them fit in best with the views generally entertained.

However, a further complication resulted from the fact that HELMHOLTZ believed that not only the special relations of localization but the idea of space itself had to be regarded as acquired by experience. Not to mention the difficulties in which this involved him in other parts of philosophy, so far from helping to clarify the theory of localization, it tended to obscure it to a very great extent, and indeed deprived it of an indispensable basis. For, by regarding the apperception of space as something given *a priori* in addition to the sensations and their local signs, LOTZE's theory, as we have said, supplied just the foundations that were needed for a development of localization in the empirical sense; but it is not possible to conceive how a spatially ordered perception could be developed simply from the non-spatial characteristics of the local signs. The truth is that by trying to extend empiricism to the apperception of space as such, HELMHOLTZ constructed a theory which had an hiatus in it and was left hanging in the air at the place where it absolutely needed to have a solid support. The deficiency can be seen best where, in expounding his theory of localization, HELMHOLTZ likewise was obliged to assume that the idea of space as such was somehow given along with the local signs, and then dismissed this point with the (rather unsatisfactory) suggestion that perhaps the most general relations of the idea of space could be supposed to be given by the sense of touch (see page 533).[1]

[1] For anyone who takes our attitude toward the subject, it must be a source of regret that although in the main we can accept HELMHOLTZ's empirical theory of localization, which after all is the matter in which we are chiefly interested here, he laboured under a misapprehension especially in regard to KANT's doctrine of the apriority of spatial intuition, and also in regard to the relation of the various problems to one another. The result was that he was placed in a position of antagonism to KANT, although such a position is not necessary for an empirical theory of localization. It also prevented him from grasping fully the distinction between space-determinations and qualities of the senses. Lastly, from his failure to recognize a basis that was certainly needed by his theory of localization, there were obscurities in it in some respects, which are undoubtedly responsible for the fact that it has not always been properly appreciated. This was the reason why, as I stated in the beginning, I deemed it worth while to detach the theory of localization from this relationship and to lay special emphasis particularly on the compatibility of this theory with KANT's doctrine.

Thus, while there are doubtless some inner affinities between the doctrines that are associated with the names of KANT, LOTZE, and HELMHOLTZ, as we have tried to show and as may be inferred from the above, they are not of a simple nature by any means. The situation is found to be very similar when we follow a line leading from KANT to HERING through JOH. MÜLLER.

JOH. MÜLLER has already been mentioned as one who, like LOTZE, tried to obtain an answer to the question which KANT left severely alone, as to how the specific spatial configuration of our perceptions is determined. The great difference between LOTZE and MÜLLER was that the former attacked directly the psycho-physical problem that is presented here, whereas MÜLLER really reached his conceptions by trying to develop KANT's doctrine further and to mould it into a more intelligible form. There was one thing especially in KANT's theory which MÜLLER adopted and translated in terms of physiology; this was the *subjectivity* of the qualities of sensation, the fact, namely, that the impressions due to the sensations aroused by outside objects are partly dependent on the nature of the sensitive subject or organism.

This is evidently connected in a way with MÜLLER's doctrine of specific energies, but the connection is not altogether simple; for it is obvious that MÜLLER's so-called specific energies are not by any means an indispensable condition for the possibility of the subjective determination of the qualities of sensation. On the other hand, however, it is clear that *if* our sensory nerves do possess specific energies such as were assumed by MÜLLER, the type of the sensation will, therefore, be independent of the stimulus, and the subjectivity of the qualities of sensation will be thus accounted for in a way that is particularly clear and simple. Quite similarly MÜLLER endeavoured also to find some tangible support for KANT's theory of the subjectivity of the idea of space, and he believed this could be found in the spatial relationship of the subject himself. In our optical perceptions, according to him, the retina is sensitive of its own extension in space. At least it might seem that this would afford an especially simple basis for the subjective nature also of the idea of space, particularly when we consider that the idea of space in the case of different individuals might be different according to the spatial determinations of the sensitive material. Thus here, too, MÜLLER went further than KANT, for he not only regarded space as the universal and unchangeable form of our sensory perceptions, but believed that this was to be attributed to the actual spatial arrangement of the processes underlying our mental life. In conjunction with this notion he also considered that the subjective arrangement was given even in its details

by an arrangement that was present objectively. In this way MÜLLER's theory was extended to the same subject as that of LOTZE's theory, namely, the nature of the psycho-physical relationships. It may be added also that the questions that were presented here were answered by it in exactly the opposite sense: for, whereas LOTZE's main position was that the objectively given spatial configuration was not a sufficient basis for that which is given subjectively, MÜLLER's view of the matter was that the latter is to be regarded as the immediate result of the former.

Thus while JOH. MÜLLER was dependent on KANT, he is found to have definite physiological or psycho-physical conceptions that were entirely foreign to KANT; and, similarly, when we come to consider HERING's nativism, we find that, although it has some important inner affinities with MÜLLER's doctrine, nevertheless it differs fundamentally from it, and ceases entirely to have anything at all in common with KANT's views. The real starting point of HERING's theory—the soil in which his ideas originated and the sphere in which they primarily belong—centres again in the special facts of the physiology of the senses. This is true in his case just as it was also in that of HELMHOLTZ and the almost diametrical opposition between their ideas and conclusions is also due to the identity of the problems which they both had in mind. HERING starts out with the incontrovertible fact that spatial properties are to be attributed to our optical sensation in the same urgent and immediate fashion as are its other characteristics (such as colour and brightness), and if this led him to postulate a physiological basis for these determinations, he was merely following here in the footsteps of JOH. MÜLLER who had likewise insisted that there was something in the spatial arrangement of the object of perception that was determined immediately by the physiological processes and the anatomical conformations. Meanwhile HERING had rejected altogether the essential part of MÜLLER's subjective spatial arrangements. There are traces of KANT's doctrine of the unity and unchangeableness of the idea of space to be found in MÜLLER's theory, but that is not the case with HERING's theory. KANT's idea of space compels us to recognize a fundamental distinction between the spatial (and temporal) determinations of our sensations and their qualitative or intensive determinations, but the characteristic feature of HERING's system is that it wipes out this distinction and places the spatial determinations exactly on a par with the others. Thus no agreements of any kind can be found between HERING's nativism and KANT's doctrines. Wherever HERING's theory touches on subjects that are also dealt with by KANT's theory, the two theories come very near to being in direct conflict

with each other, and HERING's view corresponds with what is usually called a sensationalist philosophical system.[1]

One of the main results therefore of a comprehensive survey of this kind is the fact that while we are right in thinking of the whole theory of the perception of space as connected with a series of philosophers whose theories should be considered as successive phases of a continuous development, it would be a thorough mistake to regard these theories as successive attempts to solve one and the same problem. We should bear in mind rather that the subjects which were uppermost and which an effort was made to explain were very different from time to time and depended on the trend of progress as well as on the individual and the age in which he flourished. These various subjects were the general psychological nature of the idea of space (including also the logical character of the propositions of geometry), the psycho-physical relationship, and, lastly, the specific characteristics of spatial perception, that is, the laws of localization. Hence, we see how erroneous it would be to regard HELMHOLTZ's views as being merely the negation of KANT's. On the contrary, so far as a certain part of HELMHOLTZ's theory is concerned, that is, in reference to the theory of localization (the very part that is of special interest to us at present), the easiest and most intelligible basis on which it can be constructed is KANT's doctrine of the unity and unchangeableness of space. Yet it would be still more absurd to try to identify the views that HELMHOLTZ opposed, that is, so-called nativism, with the apriority of KANT. On the contrary, the sensationalist feature of this theory is in the most direct opposition to KANT's apriority.

After having surveyed the subject in this way, I may now proceed to inquire how far the state of the various questions has been clarified or shifted by the discovery of new facts since the days when HELMHOLTZ developed his empirical theory. But before pursuing this inquiry, it might be briefly stated that HELMHOLTZ's view as to the general psychological nature of the idea of space, with the questions of logic that it involves (the points in HELMHOLTZ's theory which I cannot accept), has been more and more the subject of keen controversy as years have passed; and in these particulars KANT's conception especially has had more and more numerous and distinguished advocates. It will suffice to mention here the names of LIEBEMANN, WINDELBAND,

[1] This is in accordance with the fact that in his earliest paper published in 1861 HERING specifies JOH. MULLER as the philosopher with whom he was most nearly in sympathy. Then he goes on to mention a number of other investigators (HELMHOLTZ, LOTZE, VOLKMANN, PANUM, BRÜCKE, and NAGEL) as those who in one way or another have influenced him in his work; but he makes no allusion to KANT.

and Riehl. Particular attention may be called to the latter's brochure on Hermann v. Helmholtz *in seinem Verhältnis zu* Kant (Berlin, 1909).

In order to make the matter perfectly clear, it might be well to add that the fundamental questions concerning the psycho-physical relationship—the very questions in regard to which Joh. Müller's views, as we have said, were opposed to those of Lotze—must be considered as absolutely open to debate at present. It is undoubtedly true that we cannot adopt Müller's conception and suppose that the sensations are immediately correlated with retinal processes, although his assumptions might be considered as being applicable to the processes in the cerebral cortex in some analogous way. Moreover, as has been stated already more than once, it is now pretty generally admitted in scientific circles at least (in spite of the fact that there is still considerable dispute about it), that the substrata of all the phenomena of consciousness even in their most minute details are to be found in the physical processes of the brain. But as to the nature of these processes, and as to the particular notions we must form of the substrata of a spatially ordered perception, for instance—these are questions that utterly elude us when we endeavour to form a precise idea of them. At all events no satisfactory answers have as yet been given to them.

Concerning the real crux of Helmholtz's doctrine, the matter of localization, it must be granted, I think, that the facts by which this question has to be decided at present are on the whole practically the same as those which were known forty years ago and which Helmholtz made the foundation of his argument. Since then no positive facts have been brought to light that could tend to upset the main features of this theory or modify them essentially. The fact is, rather, that certain special observations, for instance new data about strabismic vision, together also with some modifications in general points of view, have tended to support the empirical theory in a remarkable way and to put it on a broader and firmer foundation. Thus, while we have been compelled to differ with Helmholtz's theory about some matters of principle in regard to the psychology of the idea of space, the trend of our discussion, as has been shown repeatedly, has been essentially in agreement with this theory as to the main question of localization. Moreover, the result of our studies of this question has been to show that experience and training were of fundamental importance in this connection. In the main it is the facts bearing on this subject which Helmholtz has presented and discussed that enable and indeed compel us to assert today that localization is a development through experience.

Those points in regard to localization where we have been obliged to differ with HELMHOLTZ (or rather—strictly speaking—where it was found necessary to develop his theory further) are only of secondary importance after all, no matter how much weight may be attached to them. What they amounted to was that, in the first place, more significance must be allowed to congenital substructures of localization (particularly by assuming that there are such bases for the associations between the two eyes); and, secondly, the processes of learning must be regarded from the point of view of a physiological development, which necessitates some modifications of the theory, partly on account of these processes themselves and partly also with reference to the substructures that are needed for them. In order to obtain a proper appreciation of these modifications, it should be borne in mind that, positively convinced as HELMHOLTZ was of the fundamental significance of learning for localization, he would certainly have been the last person to maintain that he or anybody else at that time was in a position to give a complete and thoroughly satisfactory picture of the nature of the processes of localization or of the innate predispositions that must be connected with learning. On the contrary, he was careful to state again and again that for reasons of methodology he preferred above all to employ a principle of interpretation of the facts, which was recognized to be correct and indispensable, in order to see how far he could get with it, so to speak. While this mode of treating the subject may have involved his relegating other possible explanations to the second place, there was no implication that these possibilities were denied or disputed; it simply meant that he considered that they belonged in another field of inquiry which was not thoroughly developed at the time, and that for this reason he thought it best to keep them carefully in the background. It would be, therefore, a complete misapprehension of HELMHOLTZ's views to suppose (as has been intimated sometimes) that he meant to deny altogether the participation of innate factors in the case of localization. The truth is, rather, that HELMHOLTZ was disposed to think that from his point of view it was extremely probable that there was some kind of coöperation such as we have deemed likely; that is, with respect to the relationship existing between the visual direction and the location on the retina, although he doubted whether such an assumption could be absolutely verified. The fact that modern investigations of strabismus have enabled us to develop still further assumptions of this sort need not imply that any fundamental modification has to be made in HELMHOLTZ's theory.

It is necessary to put special emphasis on this, because some of the writers mentioned above have taken the position that these new observations have upset the empirical theory or shown that it was not correct. To express

such an opinion as this amounts to placing secondary considerations on a par with fundamental ones in a more than arbitrary fashion, it seems to me. Modern observations tend to show that the secondary correspondence is not so precise or so efficient as the primary correspondence, indicating therefore the probability of an innate predisposition in favour of the latter; but this is an idea of exactly the same kind as was advocated by HELMHOLTZ himself in regard to the arrangement of direction within the field of view. Had the facts now known been current forty years ago, HELMHOLTZ doubtless would have deduced from them a closer connection between the local signs of corresponding pairs of retinal points, which was an assumption which he considered then as superfluous.

But the main thing to be remembered is that to a great extent these modern investigations have *corroborated* in a very positive manner inferences that HELMHOLTZ had already made from the scant material at his disposal at that time. The main conclusions which he reached have been shown to be absolutely probable. One instance is the *modification in the relation of visual directions* (or the anomalous association of visual directions, as TSCHERMAK expresses it). As has been already said, this one fact brings out in a particularly striking way the circumstance that what is called visual direction is not something given and fixed by the retinal place itself, but is a complicated result, which for that reason is capable of being modified, and which depends on the cooperation of the adjustment-factor. Undoubtedly, these new facts do enable us to appreciate the profound modifications of the mode of vision in strabismus, and they show especially the development of an anomalous relation of visual direction. But granting all this, I am convinced that any unbiased consideration will necessarily lead to the conclusion that, in regard to the most important and decisive matters, the facts tend to support the fundamental conceptions of an empirical theory to a remarkable degree, although perhaps not altogether to the extent that HELMHOLTZ supposed. It would be turning things upside down, it seems to me, to regard these new facts as a corroboration of the points of view of the intuition theory. They are certainly the opposite of what might be anticipated on the basis of those conceptions.

Similarly, with respect to the physiological *vs.* the psychological aspect of learning, we must remember that the entire process, which HELMHOLTZ endeavoured to explain for the first time so far as the perceptions of space were concerned, was regarded then as something known and familiar that came under the head of psychology. It was therefore absolutely unavoidable to take account of these relations and to use them as a basis even with respect to the terminology. It is true that nowhere does HELMHOLTZ ever say expressly that in the last analysis all training and learning are to be regarded as a physiological process due to the development of cerebral mechanism, but neither does he deny it.

Undoubtedly, HELMHOLTZ frequently made a contrast between psychic and physiological processes, but in so doing he merely adopted the principle of characterizing the latter in accordance with certain properties directly demonstrable and certainly attributable to them, and not hypothetical. This is something that should be always borne in mind in trying to understand exactly the sense in which HELMHOLTZ

used various special terms of a psychological character. Consider, for instance, his usage of the expression *unconscious conclusions* [page 6], which has been referred to already on several occasions; he meant chiefly to say that the relations of which he was speaking were of a similar kind to a series of other well-known relations, but it is hardly to be supposed that he had any idea of giving any final description of these processes in this way. It might be difficult to dispute that he was not using this language figuratively in regard to certain physiological processes; but that is pure conjecture without a particle of proof in its favour, and it would be idle to discuss it.[1]

We may be sure therefore that what HELMHOLTZ really had in mind here was something quite different. There were certain processes which he considered as psychological and which he mentioned specifically in terms of psychology; and he wanted to make a distinction between them and definite classes of well-known physiological processes and relationships and to indicate that the former were something of a different sort. Such a contrast is no less justified now than it was then, even from the standpoint that is generally taken today and that has been taken in this discussion; for in this particular instance, we are concerned with physiological processes of a special type, distinguished by various important and significant characteristics and above all by their variability and capacity for development.

After all, naturally disposed as I am to agree with many of my *confrères* and to regard learning as being a physiological process, I never have been able to consider this as amounting to any profound or fundamental divergence from HELMHOLTZ's views. On the contrary, the empirical theory of localization gains probability by being considered in this way, and at the same time is freed from a number of obstacles; for, as we saw above, by regarding localization in the light of a development by experience, we find that it is a process like many others, which our conceptions of the flexibility and variableness of cerebral relationships help us at least to understand better.

In this connection some brief allusion may be made to the utter inaptness of an opinion which is sometimes expressed, namely, that, since HELMHOLTZ's views were *psychological*, they had put an end to all further investigation; whereas, by putting the perceptions of space on a distinctly physiological basis, the opposite views had, so to speak, made them for the first time an

[1] Many interesting passages might be quoted to show that HELMHOLTZ never meant to question the possibility of a physiological basis of the mental functions that are involved here. Thus he says (on page 541): "If any one objects to including these processes of association and the natural flow of ideas among the psychic activities, I will not quarrel over names." See also page 500, where in arguing against the physiological nature of certain dispositions he makes the same kind of reservation (They do not depend, he says, "on some organic mechanism of the nervous system, at least on nothing more than underlies our mental activities.")

object of exact investigation. But no one can seriously suppose that the psychological processes in general or even those specific ones under consideration at present constituted a kind of chaos devoid of all law and order, and that consequently the psychological interpretation of them was tantamount to giving up trying to understand them or to define them by any regular rules. As a matter of fact the main thing that is required here consists in bringing out clearly that the relations of localization are subject to cultivation and development, and are therefore liable to modification in a great many ways. This indicates a certain region which it is necessary for us to investigate very carefully. At first we do not have to decide whether the solution of this problem will prove to be psychological or physiological; and so it is perfectly immaterial whether we use one set of terms or the other to describe the problem. An intuition theory may adopt the opposite method and consider the determinations of space to be sensations connected, like the other sensations, in some fixed way with the physiological stimulation of the organ of vision; but by doing this, that whole region referred to above will be in dispute and to a certain extent misconstrued. This, therefore, is the real way to put a stop to investigation, if that is the thing to be desired; whereas the empirical method enables us to obtain a glimpse of a certain region of facts and in this way supplies us with new problems and opens up new paths. This has been already proved, because, in fact, we owe it to the "empiricists," and not to the "nativists," that a new and fruitful line of inquiry has been started by studying strabismic vision for instance.

On the other hand, the feature of the nativist systems that aroused HELMHOLTZ's special opposition must also be pronounced unsound and untenable at present. For the nativism against which HELMHOLTZ fought culminated in attributing specific place-values to the visual impressions, that were supposed to belong to them exactly in the same way as the special qualities of sensation belonged to them, by virtue of innate predispositions.[1] In my opinion, this whole conception may be dismissed at present as being thoroughly inapplicable. Even in regard to the localization of direction, which is the point where it is most open to discussion, it does not take any account of the peculiar significance attaching to visual direction itself simply by virtue of its having reference to the observer's own body. While the primitive altitude-values and azimuth-values also are regarded as determinations of developed vision, only completed by fitting in the idea of the observer's body, the development that takes place here is regarded

[1] At all events, the distinction between the space-determinations of primitive vision and those of developed vision has been stressed more often by the "nativists." (See *e.g.*, HERING in HERMANN's *Handbuch*, III, p. 565.) Only, it must be obvious that if we assume a development of a wholly different kind for the space-determinations belonging to developed vision, the difference between this conception and a purely empirical one will be one of name only. Expressions such as that mentioned above merely show therefore that even those persons who wanted to take a nativist view of the space-perceptions have not succeeded in carrying out this principle rigourously. This does not alter the fact at all that the principle was intended to establish as close a connection as possible between the space-determinations of mature vision and innate space-values; nor also the fact that it has been applied in detail throughout a wide range.

in a way that is much too narrow and schematic, although the fact of development is not directly disputed. This is why it is impossible with this way of looking at the matter to take account of the phenomena observed when the eyes are abnormally adjusted (duplication of the adjustment-factor, formation of altered relations of visual direction). Moreover, the quantitative determination that is also assumed is not very probable, to say the least, nor is there the slightest proof of it. On the contrary, it would be far more likely that the characteristics of the eyesight had been empirically developed.

When we try to follow out the similar conception in regard to the impression of depth, it is found to be decidedly more difficult still. This is true even in the case of binocular perception of depth where it is generally possible to imagine a definite physiological basis for the impression of depth; that is, in the case where the impression of a distance-arrangement is produced by a combination of the impressions in the two eyes for which there is a definite cross-disparity. For how can we think of this impression as the result of depth-values belonging to the two eyes, when according to circumstances the same cross-disparity will produce the impression of an entirely different interval of depth, and when this interval depends on the distance at which the point of fixation is seen, which, in turn, depends on the most manifold empirical considerations? Thus even in this case the algebraic addition of the depth-values of the sensations in the two eyes would seem to be a purely schematic affair that had no relation whatever to the actual facts. This whole conception appears to be completely unsatisfactory in regard to the complicated relations involved in the combinations of the impressions in the two eyes that are responsible for the impressions of depth. Perhaps, the same thing might be said even more emphatically with reference to the phenomena in the case of monocular vision where the hypothetical depth-value is supposed not to be in evidence at all. If, therefore, depth-values are to be attributed to the sensations of vision and if they are supposed to be fixed determinations like those of the other optical qualities of sensation (only with the extra feature that in the case of binocular fusion the resultant impression of depth has to be obtained by adding the impressions algebraically), all that can be said is that as a description of what is actually observed this is very far-fetched. In the immediacy with which (as a rule) the impression of distance comes to consciousness it is certainly on a par with the other determinations of sensation; but it involves exceedingly complex conditions that are doubtless dependent on experience and liable to be modified by it to a very great degree. At present therefore we must likewise fully agree with HELMHOLTZ when he said that, inasmuch as there are no circumstances

in which these depth-values have been shown to exist, they stand for something that is purely fictitious, and that if they actually did exist, they would be in the way, because they would continually have to be corrected and superseded by experience.[1]

But we may well go a step further and assert that HELMHOLTZ was absolutely right in disputing the very principle on which the whole nativist conception was based. The starting point of the argument in favour of nativism always has been the fact that the spatial attributes of our optical impressions inhere in them in exactly the same urgent and immediate form as the other qualities of sensation (colour and brightness). Nobody disputed that; the only question was as to whether the inference could be drawn that there was a closer agreement between these two things in their origin and physiological basis. This was what HELMHOLTZ denied. He showed that certain impressions may be given so immediately and so insistently that they are completely on a par in this respect with the sensations themselves, and that yet they may depend on very complicated conditions developed by experience.

On the strength of what we know at present this is fully confirmed. Formations by which relationships of this sort are developed are undoubtedly of very frequent occurrence. Therefore the existence of this immediacy and urgency is no useful criterion of the physiological nature or of the mode of origin of psychic phenomena of any kind.

There is another difficulty that cannot be evaded as far as I can see. By supposing that the space-determinations must be on a footing of perfect equality with the qualities of sensation on account of this immediacy and urgency, nativism has unfortunately overestimated the importance of this characteristic feature. The only instance in which we are justified in putting these two things on a par with each other is when our attention is wholly absorbed in the manner in which a determination of this kind comes to consciousness. The above criterion is of no value at all as to the psychological nature and the more general conditions of the origin of the impression. By trying to carry out a parallelism between the space-determinations and the

[1] HERING himself instances the mode of appearance of transparent objects where the space is seen *filled up* to a certain extent of depth, that is, where something is seen (in one direction) not simply at a definite depth, but in a whole *interval of depth*. This seems to me just as open to objection as what we have just been discussing. For it is evident that a sensation of this sort cannot be evolved from some kind of depth-values supposed to be attached to each point of the retina. They must be assumed to occur in an entirely different way in this case. Thus the more we insist (as HERING does) on the homogeneity between a vision of this sort and the ordinary impressions of depth, the more right we shall have to assume that the latter have a complicated origin also and cannot be simply determined by fixed depth-values.

qualities of sensation in these respects also, nativism has become entangled in untenable consequences and has been led to misconstrue that whole region of facts that are peculiar to the space-determinations. We have to thank the empirical theory for enabling us to comprehend these facts and appreciate them properly.[1]

At the same time it is well to emphasize once more that nothing we have said is at all inconsistent with the fact that *innate predispositions* for spatial perceptions must be granted as an indispensable condition. Moreover, they must be presumed to exist in certain connections where HELMHOLTZ thought they were unlikely. Some of the nativist assumptions on this point have been confirmed directly, whereas others can claim to be justifiable conjectures at least. However, it should certainly be added that, although the idea of nativism was that these innate bases could be found in the place-values supposed to be rigidly attached to the various points of the retina, it has never succeeded in giving any really relevant description of them, certainly at least none that is at all thorough or satisfactory. Even in the case of a single eye we are scarcely justified, as we have seen, in assigning a given direction-value (altitude and azimuth) to each point on the retina. The relation between the two eyes will be still less adequately expressed by attributing the same direction-value and opposite depth-values to definite pairs of corresponding points. Not to mention the fact that the first relation is not perfectly fixed, and the second is after all purely fictitious, it must be said that the peculiar circumstances by which a difference is expressed between the impressions in the two eyes and an inner connection between those pertaining to the same eye are not brought out by these values; such, for instance,

[1] As we are interested here only in the question of localization, I have omitted to mention what seems to me to be the most serious and fundamental mistake of all in the nativism under discussion at present. As was intimated above, this mistake is in its sensationalist nature, that is, in its regarding the idea of space as an aggregate of a limited number of separate sensations of locality associated with the sensations of vision.

Without going into details, it may be briefly indicated in this connection that there is a general tendency inherent in nativism all through to consider the phenomena of localization as being rigidly controlled in some simple fashion; which has been more than once responsible for minor mistakes in matters of detail. An instance of this is the assumption that the lines which were imaged on the retinal horizons would always appear to be horizontal; which has not been confirmed. The same thing is true in regard to the assumption that the lines which were imaged in the two apparently vertical meridians (and which were therefore seen single) would appear to be perpendicular to the visual plane; and likewise in regard to the assumption that objects lying in the longitudinal horopter (*Langshoropter*) would be perceived in a plane normal to the direction of vision at the point of fixation. The phenomena have invariably proved to be more complex and liable to modification than was supposed at first. While these matters, it is true, are evidently connected with the general trend of nativism, at the same time they are not so much questions of principle and may be passed over.

as the factors involved in the rivalry between the two visual fields, the possibility of a regional formation of this phenomenon, and the direct discrimination between the two eyes which is sometimes possible. When we consider these connections in an unbiased fashion, their real nature will appear to be found in the anatomical relations, although we may not be able to say exactly what they are.

As has been shown in the course of the previous discussion, and as has been just brought out once more, an empirical conception of localization permits us to lay much more stress on innate predispositions than Helmholtz did. The question might therefore be asked whether the final conclusion that we have reached might not just as well be called a nativist view, of course not in the sense that was meant by the earlier exponents of intuition theories, but in a broader sense in which more significance was attached to experience. In a way this may be answered in the affirmative. It was stated at the outset (and the fact has been brought out still more clearly in the course of this discussion) that it would be a mistake to think of nativism and empiricism as two mutually antagonistic conceptions involving a choice one way or the other. Experience is placed in the foreground in the empirical theory, and innately determined relations in the intuition theory; and undoubtedly (speaking perfectly generally) both of these things have something to do with our perceptions of space. Here, if anywhere, it will be true that there is a certain amount of justification for each of the two originally conflicting opinions, according to the degree of importance that was attached to one or the other of these things. Whether a line should be drawn between the two conceptions, and if so, where to draw it, will always remain a matter of personal opinion, and is something that is hardly open to discussion. Accordingly, a certain measure of subjectivity must also be attached to the method of treatment which I have used here, although it seems the natural method to me and the most appropriate. I cannot refrain from mentioning it, because, while it was not of much consequence in enabling us to decide about the individual questions, it has determined the whole trend of the argument. The crucial question, it seems to me, will always be as to whether the spatial determinations of our sensory impressions can be differentiated from their other (qualitative and intensive) determinations as something that is fundamentally different —different partly as to their psychological nature, their inner relationships, etc., and partly as to their genesis, the nature of the mechanism by which they are produced, and especially their dependence on experience. To answer this question in the affirmative, to establish a fundamental difference of this kind, without disputing about all kinds of innate bases or any special kind—in my judgment this may be

taken as the criterion of an empirical point of view. Undoubtedly, this was the salient point for HELMHOLTZ also. Over and over again he says with special emphasis that we cannot attribute to sensation everything that underlies an experiential development and modification. On the other hand, the advocates of nativism have always insisted especially on the homogeneity between the space-determinations and the other sensory determinations, as is shown to some extent by the nomenclature that is employed. However, on the strength of the conception which we have finally reached here, there is undoubtedly the greatest justification for stressing this opposition; indeed, it is the really fruitful concept that has opened up this whole region. For surely the entire situation in regard to localization is one for which analogies can easily be found in the connection between our sensations proper and empirical conceptions, but nowhere in the conditions that determine the sensations themselves. We may be ignorant of the physiological processes involved in each of these different cases, but, by bringing the two together and comparing them, undoubtedly we do obtain a very useful insight into their different forms and modalities; whereas if they are all classified in one group under the head of sensation, the attention is simply diverted from the important characteristic distinctions.

From this point of view the position taken throughout this discussion might be said to be a fundamentally empirical conception, in spite of the fact that it does contemplate the possibility of innate bases. Anyone who will follow the argument as here presented will see that the principle which has guided us, and which remains still today the best way of obtaining an insight into those problems and is the basis of future investigation, has been the empiricism of HELMHOLTZ, even though it has had to be modified and amplified in many respects.

Note (by J. P. C. S.).—The following bibliography of more recent literature on the general subject of the perception of space, etc., may conveniently be inserted here:

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389–393, 439–443 and 470–473.—H. GEIPEL, Die Transformation des wirklichen Raumes in den Sehraum. *Physik. Zft.*, 21 (1920), 169–172.—F. SCHUMANN, Die Repräsentation des leeren Raumes in Bewusstein. Eine neue Empfindung. *Zft. f. Psychol. u. Physiol.* usw., 85 (1920), 224–244.—Idem, Untersuchungen über die psychologischen Grundproblems der Tiefenwahrnehmung. II. Die Dimensionen des Sehraumes. Ibid., 86 (1921), 253–277. M. v. FREY, Über die sogenannte Empfindung des leeren Raumes. *Zft. f. Biol.*, 73 (1921), 263–266.—K. KRÖNCKE, Zur Phänomenologie der Kernflache des Sehraumes. *Zft. f. Sinnesphysiol.*, 52 (1921), 217–228.—E. R. JAENSCH and F. REICH, Über den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter. II. Über die Lokalisation im Sehraum. *Zft. f. Psychol.*, 86 (1921), 278–367.—E. R. JAENSCH, Über den Nativismus in der Lehre von der Raumwahrnehmung. (Beilage zu der Arbeit von K. KRÖNCKE.) *Zft. f. Sinnesphysiol.*, 52 (1921), 229–234.—L. FOCHER, Physiology and psychology of WEBER's theory of space sense. *Zft. f. d. ges. Neurol. u. Psychiat.*, 87 (1923), 223–246.—G. M. GIESSLER, Space sense phenomena. *Arch. f. d. ges. Psychol.*, 45 (1923), 282–297.— P. MARIE and H. BOUTTIER, Eye movements and sense of space. *Schweiz. Arch. f. Neurol. u. Psychol.*, 13 (1923), 428–439.—K. HOROVITZ, Beitrage zur Theorie des Sehraums. *Sitz. d. Akad. der Wiss. in Wien, math.-naturw. Kl.*, Abt. II a, 130 (1921), 405–421.—Idem, Grössenwahrnehmung und Sehraumrelief. PFLÜGERS *Arch.*, 194 (1922), 629–646.—E. R. JAENSCH and W. SCHÖNHEINZ on visual perception. *Arch. f. d. ges. Psychol.*, 46 (1924), 3–60.

II. The Theory of Binocular Optical Instruments

Binocular optical instruments[1] have assumed so much importance recently that it seems desirable to discuss systematically certain things connected with the general theory of them. While the various topics to be considered have all been alluded to in the main text, the references are scattered about here and there, and so it may be worth while to summarize them in a somewhat clear fashion. Exception, too, has been taken to HELMHOLTZ's treatment of the subject in some ways; and where these objections seem to be valid, an attempt will be made to put the matter in the right light. On the other hand, a number of treatises on the subject have been written from the standpoint of physics, which, while they are unobjectionable and very complete in this respect (notably the work by CZAPSKI and v. ROHR[2]), at the same

[1] Here we must include not only those binocular instruments intended for the direct observation of objects of all sorts, but also (owing to the complete analogy of the problem that is involved) those contrivances for making pictures (photographs especially) that are to be afterwards viewed binocularly, as in the case of the stereoscope. In this latter case we speak of an *indirect* observation.

[2] CZAPSKI, *Grundzüge der Theorie der optischen Instrumente nach* ABBE. 2 Aufl. unter Mitwirkung des Verf. und mit Beiträgen von M. v. ROHR, herausgegeben von O. EPPENSTEIN. Leipzig 1904. (Reprint from WINKELMANN's *Handbuch der Physik*.)

¶See also the third edition of this valuable work published in 1924:—"bearbeitet von den wissenschaftlichen Mitarbeitern der ZEISSischen Werkstätte: H. BOEGEHOLD, O. EPPENSTEIN, H. ERFLE, A. KÖNIG, M. v. ROHR, herausegegeben von H. ERFLE und H. BOEGEHOLD."—Reference should be made here also to M. v. ROHR, *Die binokularen Instrumente*. 2 Aufl. Berlin, 1920.—Idem, Über perspcktivische Darstellungen und die Hülfsmittel

time do not altogether bring out the physiological relations in the way I should like to see the subject discussed for the purposes we have in mind here.

It is obvious immediately that the construction of binocular optical instruments opens up an exceedingly extensive field so far as the consequences for vision are concerned; for if the images to be exposed to each eye separately are produced independently, the geometrical relations between these images, and hence also the impression produced on the spectator, may be exceedingly varied. Thus it is easy to make optical contrivances that will cause the objects to be amazingly perverted. On the other hand, in designing and constructing instruments to be used for practical purposes, the problem generally is how to avoid illusions and distortions as much as possible. It will be useful for the following discussion if we proceed to consider at once what can properly be expected from optical instruments, in order to try to obtain certain points of view that will help to guide us in this matter. The first point to be mentioned is the question of creating a substitute for the object that will be as perfect a resemblance to it as possible. Naturally, this problem is concerned only with the methods of indirect observation (photographic stereograms). Here we can endeavour to obtain perfect congruence with the real objects themselves. In the second place, the problem may be to substitute a model for the object that is geometrically similar to it. The immediate purpose may be to replace the object by a *magnified* model (as in case of the microscope). But in other cases also the presentation of a model, and particularly one that is on a reduced scale, may be considered as a permissible and useful method of enabling us to get a correct notion of the object. Lastly, there is a third class of optical effects which we can keep in mind here, namely, where the result is equivalent to the observer's moving his position and, for example, coming nearer to the observed objects—an effect such as is usually obtained with a telescope. Consequently, the method we can adopt will be to examine, first, how the appearance of an object will be affected by changing its distance or varying its size, and then how this appearance will depend on the various optical contrivances that are combined in the instrument and capable to a certain extent of being freely adjusted with reference to each other.

zu ihrem Verständnis. *Zft. f. Instrumentenk.*, 1905. 25, 293-305, 329-339, 361-371. Idem, Die beim beidaugigen Sehen durch optische Instrumente möglichen Formen der Raumanschauung. *Münch. Sitz.-Ber.*, 1906. 36. 487-506.—Idem., Über Einrichtungen zur subjectiven Demonstration der verschiedenen Fälle der durch das beidäugige Sehen vermittelten Raumanschauung. *Zft. f. Sinnesphysiol.*, 1907. 41, 408-429.—C. PULFRICH, *Stereoskopisches Sehen und Messen.* Jena, 1911; and article on "Stereoscope" in eleventh edition (1911) of *Encyclopædia Britannica.*—(J.P.C.S.)

By treating the problem in this way we shall be able to see whether mere optical devices can possibly enable us to obtain some effect that is equivalent in its result to a change of the distance or size of the object, and if so, how it can be accomplished.

However, there is a difficulty about this way of looking at the problem presented by binocular instruments and about this plan of studying them, which must be mentioned at once. Whenever a binocular instrument is used to look at any objects whatever, the question may always arise to begin with, as to what the nature and configuration of real objects would have to be as seen by the naked eye in order for them to produce the same optical result, that is, give the same retinal images with the eyes adjusted in the same way, as is produced by the objects that are actually present when they are viewed through the optical instrument. The answer to this question is a problem solely of physics and geometry. An object such as this may be called the *space-image* produced by the optical image. Let us state at once that this space-image is always uniquely determined. If the positions of the two images of a given real point in the two optical systems are designated by P_l, P_r, and the places of the observer's two eyes by O_l, O_r, the corresponding place in the space-image will be given by the point of intersection of the straight lines O_lP_l and O_rP_r.[1] This question must be kept separate from the other question, as to what is the *visual impression* that will be produced under the given circumstances. There would be no difference between the two conceptions, provided it could be assumed, according to the view of the earlier form of the projection theory, that the apparent place of an object was determined by the point of intersection of the pair of lines of sight corresponding to the two eyes; but as this is not the case, the visual impression is much harder to define. It might as well be stated here at once that in general the visual impression is not determined uniquely by the conditions of the optical imagery; and hence the question as to the nature of this impression involves a series of considerations of a specifically physiological kind.

[1] Of course, the criterion of the "space-image" as here defined (that is, agreement between the retinal images obtained by viewing it with the unaided eyes and those obtained by looking at the real objects through the optical instrument) will not be absolutely realized to the full extent. Obviously, this criterion can be specially defined in various ways, and while there might be an agreement between the retinal images in the two cases in some respects, there might be departures in other respects. Thus, in the construction given above, the question would arise as to what was meant by the place O where the eye is supposed to be—whether it was at the *nodal point* or the *centre of the entrance-pupil* or the *centre of rotation*, etc. However, for our purposes these distinctions are not of any special importance; and so we can speak simply of the place where the eye is and ascribe to the space-image as thus defined the property above mentioned.

While obviously the question that will really be of most interest ultimately will be with reference to the *visual impressions* themselves and their relations to the real objects, still it will be better to keep in mind the much simpler matter at first and to consider the nature of the *space-image*. Before entering on a more general discussion, it will be well to speak of several special cases that are particularly simple. A *tautomorphous* space-image (as it is called by v. ROHR), that is, one which is congruent with the real object, will be obtained by taking two photographs with a base-line equal to the interpupillary distance and then viewing them from a distance which is equal to the distance that the plate was from the photographic lens, provided a third condition is fulfilled also. This last requirement relates to the lateral separation the two pictures ought to have, or, in other words, to the convergence of the eyes needed for viewing definite points. It can be formulated by saying that when the eyes are focused on a pair of corresponding points in the two pictures, each representing the same real point in the object, the convergence of the eyes must be the same as it would have been if the two eyes had been in the positions of the two lenses and focused on the real point itself. It is obvious at once that, so far as the retinal images and ocular adjustments are concerned, exactly the same conditions will be reproduced in this case as if the eyes occupied the places where the camera lenses were and perceived the real objects directly.

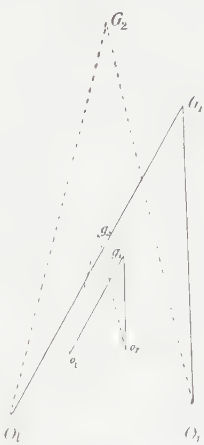

Fig. 77.

Moreover, when we consider the space-image that is obtained in the case of a simple *telestereoscope* [see Fig. 53], it is easy to see that it is absolutely similar to the real object itself. Here also we shall employ v. ROHR's term and speak of an image of this kind as being *homœomorphous*.[1] The simplest way of understanding how this happens in this case is to notice that any incident ray of light which is directed toward the virtual image of one eye in the pair of mirrors, after being reflected at each mirror, will proceed into the eye itself in the same direction as at first. Suppose therefore that o_l and o_r in Fig. 77 represent the places where the two eyes are, and that O_l and O_r indicate the positions of their virtual images in the two pairs of mirrors; then if G_1 designates the position of a real point, its space-image g_1 will be found by drawing a pair of lines from o_l and o_r parallel

[1] ¶From the Greek ὁμοιόμορφος, meaning similar in form. (J. P. C. S.)

to the lines O_lG_1 and O_rG_1, respectively, and determining their point of intersection. As the same construction applies to every other point G_2 and its corresponding space-image g_2, it is evident that the space-image will be a figure geometrically similar to that of the object, and that the virtual images of the two eyes will be in the same geometrical relation to the space-image as the two eyes themselves are to the real object. Hence, the ratio of similitude between the space-image and the object will be the same as the ratio between the inter-pupillary distance o_lo_r and the enlarged base-line O_lO_r which is the interval between the virtual images of the two eyes.

In order to treat the problem of the space-image more generally, let us simplify it by supposing that all the dimensions of the object are small in comparison with its distance from the observer. On this assumption, change of place can be considered as being simply change of distance occurring in the same ratio for all points of the object; and thus we can speak briefly of a change of distance between the observer and the object as a whole. Moreover, with this limitation, a model of the object enlarged or reduced in any ratio can be thought of as being placed at a distance which as compared with the actually given distance will be increased or diminished in a definite way, that is, scale and distance may be treated as two independent variables; whereas if the objects were of larger dimensions, obviously the condition, that the change of distance should be the same for all of its points, would be inconsistent with the requirement of geometrical similarity.

With the above limitation, we might proceed then to investigate the conditions for producing a space-image that would be in conformity with a similar model of the object placed at some arbitrary distance and executed on any desired scale. However, for reasons which will soon be evident, just at present it will be better to consider the nature of space-images (and afterwards of visual impressions also), especially in some definite respects that are important mainly because they are characteristic of the mode of appearance both of the object itself and of the space-image. If we can see how the space-image is modified by changes of distance and scale on the one hand, and by adjustments that can be made in the optical instrument on the other hand, it will enable us not only to comprehend the conditions for obtaining (orthomorphous[1]) space-images of the excellent type mentioned above, but also to see more clearly why there are unavoidable departures from this ideal form in other cases, and what they signify. Suppose we begin by considering the apparent (or angular) size of a given area lying parallel to the frontal plane; and in order to have a

[1] ¶Apparently, used here in the same sense as "homœomorphous"; in other words, the image is of the "correct" form when it is "similar" to the object. (J. P. C. S.)

precise notation, let us use the symbol φ to denote the angle subtended by the unit of length when it has this frontal extension. This angular magnitude will be referred to hereafter simply as the *frontal value.* It varies therefore inversely as the distance of this frontal unit of length (a fact which will be referred to again in the final discussion).

The next thing that has to be considered is the mode of appearance of intervals extending depthwise; and first we ought to see how this dimension is presented to view by one eye. When we reflect that in every instance the distance between object and observer necessarily constitutes a part of the variable conditions of vision to be considered here, and that, owing to changes of perspective, the entire appearance of solid bodies will be varied in many ways by change of distance, it is evident that the only way of specifying these relations by a definite value is simply to select a certain criterion more or less arbitrarily. However, indeed, such a criterion will be found to be quite sufficient for our purposes. Suppose we take this to be the angle subtended by a (small) area extending depthwise situated at a given distance not very far from the point of fixation. In Fig. 78 this distance is denoted by y, and the value of the interval of depth by z; then if the distance between object and observer is denoted by E, on the assumption that x is a small arc, we may write:

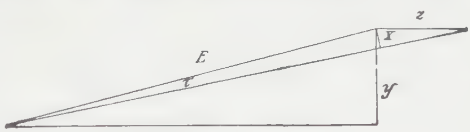

Fig. 78.

$$x = \frac{y\,z}{E},$$

and hence the value of the angle subtended by z will be

$$\tau = \frac{y\,z}{E^2}.$$

The angle subtended by an interval of depth of unit length ($z = 1$) situated at unit distance from the point of fixation ($y = 1$) will be called the *monocular depth-value* and denoted by τ. Evidently, it is inversely proportional to the square of the distance.

Lastly, we must take account of the binocular phenomena of the dimension of depth. The characteristic thing in their case is the relative parallax [page 373] of a point which is at a given distance from the point of fixation, either in front of it or beyond it. This value was found to be equal to $2ab/E^2$, where $2a$ denotes the interpupillary distance, b the depth of the given point from the point of fixation, and, as above, E the distance from the observer.

Putting b equal to the unit of length, we may define the *binocular depth-value* as the relative parallax of a point which is at a unit distance depthwise from the point of fixation. This function, which may be denoted by β, will be proportional therefore to $2a/E^2$.

Now in a certain way these three functions, φ, τ and β, are characteristic of the mode in which an object of a given kind will be seen under any conditions whatever; and the thing that has to be considered next is how these functions depend on the various conditions. The main condition that is involved is the distance between object and observer. Its effect can be seen at once from the formulae given above. Suppose, for example, that the distance is altered in the ratio of $1:\epsilon$, and that the corresponding values of the above functions become then φ_ϵ, τ_ϵ and β_ϵ; then we have $\varphi_\epsilon = \varphi/\epsilon$, but $\tau_\epsilon = \tau/\epsilon^2$ and $\beta_\epsilon = \beta/\epsilon^2$. Thus, *the frontal values are inversely proportional to the distance between object and observer; whereas the monocular and binocular depth-values are both inversely proportional to the square of this distance.*

The second case we should have to consider would be a geometrically similar enlargement or reduction of the same object, that is, the case where a model similar to the object was substituted for it. Let μ denote the ratio in which the dimensions of the object are altered, that is, let μ denote the *scale of the model*; then if, just as in the similar case above, the corresponding values of the new functions are denoted by the same symbols with the subscript μ, we find:

$$\varphi_\mu = \mu\varphi.$$

But since y and z in the formula for τ will both be changed in the ratio of $1:\mu$, the result in this case will be $\tau_\mu = \mu^2\tau$. And, since b is the only factor that is affected now in the formula for β, we must have $\beta_\mu = \mu\beta$. Thus,

$$\varphi_\mu = \mu\varphi, \quad \tau_\mu = \mu^2\tau, \quad \beta_\mu = \mu\beta;$$

and hence we can say that *when the object is replaced by a model at the same distance away, which is similar to it geometrically, the frontal values and the binocular depth-values will be altered in proportion to the scale of the model, but the monocular depth-values in proportion to the square of this scale.*

On the other hand, let us now consider the changes that are dependent on the optical device. We may begin by thinking first of the changes that are apt to be made principally in connection with the telescope. Let ν denote the angle between the optical axis of the instrument and the direction of the image of any object as seen by an

eye looking through the telescope; and let ν' denote the corresponding angle between the optical axis and the direction of the object-point as seen directly from some given anterior point; then by a well-known formula, $\tan\nu = a\tan\nu'$; and on the assumption that both angles are small, this formula may be written:

$$\nu = a\nu'.$$

The anterior point having this relation to the observer's eye will generally be somewhere in the object-glass of the telescope. It may be called the *entrance-point* (*Aufnahmepunkt*); and the coëfficient a, which shows how the angle is changed, and which signifies what is usually termed the magnifying power of the instrument, may be called here the *telescopic power*.—It may be observed here that, while a will generally be different from unity in the case of the *microscope*, the magnifying power of this instrument is not defined by this coëfficient. For instance, a microscope may not have any telescopic power at all (that is, the value of a may be unity), and yet its magnifying power may be different from unity, as this term is defined in the case of the microscope.[1] We shall have occasion to allude to this again. Moreover, in the case of the *stereoscope* the angles are magnified in the ratio of P to . . where P denotes the distance of the photographic plate from the lens of the camera, and B denotes the distance at which the image is viewed. And hence a telescopic power equal to P/B may be ascribed to the stereoscope.

If an optical instrument of telescopic power a is placed in front of each eye, the frontal values and the monocular and binocular depth-values will all be a times as large as they would be for the real objects viewed directly from the entrance-points; and so if these latter values are denoted by φ, τ and β, the corresponding functions as modified by the telescopic power will be:

$$\varphi_a = a\varphi, \quad \tau_a = a\tau, \quad \beta_a = a\beta.$$

Accordingly, *the effect of a "telescopic power" is to multiply the frontal values and the monocular and binocular depth-values all by the amount of the telescopic power of the optical instrument* (the term "telescopic power" being used here to include the corresponding function that is obtained when we make pictures and view them stereoscopically).

Moreover, a second effect that is obtained by using an optical instrument is what may be briefly described as a change of distance

[1] ¶See Vol. I, pp. 362-365. (J.P.C.S.)

of the entrance-point. In the simple telestereoscope the objects appear as they would look if they were viewed from the places where the virtual images of the observer's eyes are formed in the mirrors of the instrument. And so, in accordance with the previous terminology, these places will be called the entrance-points also. Following the usual practice, let us call the distance between the pair of entrance-points the *base-line*. The ratio between base-line and interpupillary distance will be denoted here by δ. Now it is perfectly obvious that the effect of taking two photographs from places separated from each other by an interval which is greater than the interpupillary distance and then presenting them to the eyes for stereoscopic fusion will be equivalent to the action of a telestereoscope: and so for this case also the ratio between the base-line and the interpupillary distance will be denoted by δ.

If, as before, the symbols φ, τ and β are used to denote the values of these functions for the unaided eyes placed at the same distance from the object as the entrance-points, and if φ_δ, τ_δ and β_δ denote the values of these same functions for the changed base-line, then

$$\varphi_\delta = \varphi, \quad \tau_\delta = \tau, \quad \beta_\delta = \delta\beta.$$

Thus, *when the base-line is changed in the ratio of* $1:\delta$, *the frontal values and monocular depth-values are not affected at all, but the binocular depth-values will be* δ *times as great as before.* (This statement applies not only to the telestereoscope but also to the stereoscopic viewing of photographs taken from different standpoints.)

By the aid of these rules the conditions can now easily be stated which must be satisfied in order that, with any base-line and any telescopic power, the eyes may be affected exactly as they would be in looking at a geometrically similar model at any arbitrary distance, without the aid of an optical instrument. All that is needed is that the functions φ, τ and β shall be modified in exactly the same way in one case as in the other; in other words, we must have

$$a\varphi = \frac{\mu}{\epsilon}\varphi, \quad a\tau = \frac{\mu^2}{\epsilon^2}\tau, \quad a\,\delta\,\beta = \frac{\mu}{\epsilon^2}\beta,$$

or

$$a = \frac{\mu}{\epsilon}, \quad a = \frac{\mu^2}{\epsilon^2}, \quad a\,\delta = \frac{\mu}{\epsilon^2}.$$

The optical relations here considered (namely, telescopic power and base-line) are not yet sufficient to determine the space-image uniquely. The nature of this image will depend on at least one other factor. The optical properties of the instrument as above specified

may be said to determine the retinal images, to express it briefly; but they do not define the degree of convergence needed by the eyes to focus a given point binocularly.

These relations can be varied, for instance, in the telestereoscope by changing the angle between the mirrors. Similarly, they can be modified in the ordinary form of stereoscope by increasing the interval between the two pictures that are viewed by the two eyes separately. These variations will not only affect the *distance* of the space-image but also its dimensions corresponding to definite frontal values, etc.

However, in spite of this circumstance, the preceding formulae enable us to state at once some important conclusions as to the nature of the space-images. For example, it is evident that the first two equations cannot be satisfied unless a is equal to unity. Therefore, *the optical instrument must not have any telescopic power (that is, a must be unity) in order to obtain a space-image in harmony with a model that is geometrically similar to the object.* If that is the case, then $\mu=\epsilon=1/\delta$. This means that with an instrument, which has no telescopic power and has a base-line δ times as great as the interpupillary distance, the space-image will have the same frontal values and monocular and binocular depth-values as a model of the real object would have if it were made on the scale of $1:\delta$ and viewed from a distance equal to this same fraction of the distance between the object and the entrance-points. Now since frontal values and monocular and binocular depth-valves determine uniquely the entire configuration of points at a given distance, the instrument will give a space-image which will be in conformity with such a model, that is, an homœomorphous image, provided the image is produced at exactly the right distance by an optical contrivance of the sort here specified. *An instrument, which has no telescopic power, and whose base-line is δ times as great as the interpupillary distance, will give, therefore, an homœomorphous space-image, reduced in the ratio of $\delta:1$, provided the distance at which the image is formed is in the same ratio to the distance between the real object and the entrance points as $1:\delta$.* The distance at which the space-image is formed will depend on the special design of the instrument, and consequently it will be better to regard this distance as an independent variable. Therefore a special name will be used to denote this distance which is necessary for the production of an homœomorphous or tautomorphous image, namely, the *orthomorphous* distance. It is equal to A/δ, where A denotes the distance between the real object and the entrance-points.[1]

[1] It may be recalled here that these conditions are actually realized in the ordinary telestereoscope (which has no telescopic power and in which the mirrors are parallel). It may be seen from the above that the latter requirement is of special importance for an

If the value of a is different from unity, it will be impossible to have an homœomorphous space-image; and so in such cases the image will be more or less unlike a model that resembles the object geometrically, that is, it will be distorted. The term used by v. ROHR to describe a space-image distorted in this way is *porrhallactic*.[1] Thus a porrhallactic space-image will always be obtained with an optical system of telescopic power different from unity; and, as follows from the above, this will be true no matter what happens to be the form of that other factor (referred to at the end of page 660) by which the distance of the space-image is determined. If the telescopic power is different from unity, it will be impossible to produce retinal images which can be made to give an homœomorphous space-image by any suitable adjustment in this latter respect.

But the fact that it may be impossible to obtain absolutely homœomorphous space-images does not imply that we cannot produce tolerably good images of this sort that may have a very special significance. Indeed, in my opinion this is the case, as may be shown by disregarding the monocular depth-values and considering simply the frontal values and the binocular depth-values. In the first place it is obvious at once that, no matter how the telescopic power and base-line are chosen, a space-image will be obtained under all circumstances which, so far as those two values are concerned, will agree with a model resembling the object geometrically and situated at a certain distance. Thus, the conditions for this are:

$$\mu = \frac{a}{\delta} \quad \text{and} \quad \epsilon = \frac{1}{\delta}\,.$$

No matter what may be the values of a and δ, these formulae will give perfectly definite corresponding values of the ratios μ and ϵ. Moreover, as soon as a space-image of this sort is formed at the same distance away as the model, it will agree with the latter in perfectly definite respects; for since at a given distance a definite frontal segment will correspond to the angular frontal value and, similarly, a definite interval of depth to the binocular parallax, it is obvious that a space-image of this character will conform to the model of the object both as to frontal dimensions and as to the relief everywhere.

Accordingly, such a space-image would be correct in the sense that it would be in agreement with a model of the object so far as the

homœomorphous image. On it depends the fact that the lines drawn from the eyes to a point in the space-image shall meet at the same angle as the lines drawn from the virtual images of the eyes to the corresponding point in the real object, or, in other words, that the space-image shall be produced at the orthomorphous distance.

[1] ¶That is, an image in which the relief is different from that of the original. (J.P.C.S.)

configuration of all points in a given frontal plane and the depths of all points were concerned. But it would exhibit differences from the model in regard to the monocular depth-values or, to express it more generally, in regard to the angular intervals between points belonging in two different layers. We shall return to this question again and discuss briefly the nature and significance of these discrepancies. Let us note here that, in the first place, if the space-image and a model resembling the object (and therefore the space-image and the object itself) are so in accord that the frontal and sagittal dimensions are in the same ratio, the ratio between the depth-values and the frontal dimensions will be correct. Since it has become customary to a certain extent to use the word "plastic" with special reference to depth-dimensions, a space-image of this kind may be said to be *orthoplastic*.

An orthoplastic image then would be one for which the ratio between depth-dimensions and frontal dimensions was correct; and, in contradistinction, the terms *hyperplastic* and *hypoplastic* might be used with reference to those space-images in which the depth-values were either too large or too small as compared with the frontal values.[1]

As already stated, the requirement for an orthoplastic image is also that this image shall be produced at a certain special distance, which may therefore be called the *orthoplastic distance*, in conformity with the previous terminology. Accordingly, the value obtained for it here is $S = A / \delta$, and at the same time we have also the relation $\mu = a / \delta$.[2]

Accordingly, *an instrument of telescopic power* a, *whose base-line enlargement is* δ, *will give an orthoplastic space-image, provided the image is projected at a distance* A / δ; *and the scale of reproduction then will be* a / δ. *In other words, we shall obtain that partial agreement between the space-image and a model of the object made on the scale of* $a : \delta$ *which we have agreed to call an orthoplastic effect.*

After these preliminary observations concerning the physical or optical relations, we may proceed now to discuss the *visual impression*. The main thing to bear in mind here is that this question has to do

[1] There is a certain objection to these terms, on account of the fact that it is quite common to speak also of an enhancement of the "plastic" effect, meaning thereby an increase in the ability to perceive depth-relations; which might be used in this sense with reference to an orthoplastic or even an homœomorphous space-image. Thus, for example, we could speak in this way of the simple telestereoscope as heightening the "plastic" effect although in the other sense the effect produced here is rather normal than "hyperplastic" let us say. Perhaps, for the sake of preventing confusion, it might be a good idea to speak of those instruments as "*auxoplastic*" where the "plastic" effect is enhanced in the latter sense.

[2] It will be well to recall here that this discussion is limited expressly to the case of objects of very small dimensions. It is only in this case that we have a right to apply the criteria of the orthoplastic effect without those of homœomorphism; for, evidently, with objects of larger dimensions, the requirements of orthoplastic distance will lead to conflicting conditions, unless the space-image is homœomorphous.

with the perception of space-relations, the subjective nature of which makes it practically impossible to measure them exactly and accurately; and hence any geometrical data can only be regarded as being approximate and valid to a limited extent. With this limitation, the visual impression can be estimated on the basis of the general laws that were developed in the theory of the perceptions of vision. Accordingly, a distinction must be made between the *direction* in which the thing is seen on the one hand, and its *distance* on the other hand. So far as the directions are concerned, the way they are arranged in the perception may be considered as being the same as that in which they are arranged in the space-image, so that the latter arrangement may be substituted in place of the former.[1]

The relations of distance, on the other hand, need to be very carefully considered. The main thing to be observed here is that the absolute distances at which the objects are perceived are dependent in an intricate way on a series of entirely different circumstances. These distances are not determined at once by the degree of convergence of the eyes. Thus the selfsame optical conditions (identical retinal images and ocular adjustments) may produce very different impressions of distance. On the other hand, the differences between the retinal images in the two eyes (binocular parallaxes or cross-disparities) and the concomitant variations of the convergence one way or the other required to fixate various points in succession do indeed positively give the relative depth-configuration of the points that are seen single; but the impressions of depth thus produced are also again dependent in value on the absolute distances that are determined and can be modified in various ways. No absolutely fixed or perfectly valid rule can be given offhand for the relationship between these things. Still, as was shown in a previous part of this volume (page 383), we can begin with the most likely assumption for the time being, namely, the assumption that if an object is perceived at the distance E, the nature of the visual impression, so far as depth-dimensions are concerned, will be approximately the same as that which would be

[1] This assumption, to be sure, is really not absolutely correct, especially inasmuch as it leaves out of consideration the differences in the angle-arrangements of the two eyes, the phenomena of diplopia, etc. However, on the assumptions made here, and for the present purpose, these details may be neglected. There will appear to be all the more justification for this when we consider that neither space-image nor visual impression is a perfectly rigid, uniquely defined conception, since the localizations do not agree exactly for different adjustments of the eyes. As to them we do not need to take simply their totality as being the visual impression, namely, what is seen over the entire field of view with a given adjustment of the eyes, but we can also take into account the direction in which each point appears when it is focused binocularly. With this way of looking at the matter, the assumption made here is still less open to objection.

produced by an object really situated at that distance, whose retinal images were of the same character, especially as to their differences in the two eyes.[1] In order for this to be so, we found that the observed depths were necessarily dependent on the apparent distance of the point of direct fixation and on the instantaneous parallaxes in a definite, although not very simple, way. This mode of binocular perception of depth (which, as stated, may be considered as probably approximately realized) was spoken of as a *proportionate* depth-perception.

Now it is plain from the previous discussion of the subject that this assumption is certainly not to be applied rigourously in a perfectly general way, and this fact should be distinctly emphasized here again. Still it is the only assumption which can be considered as approximately true to some extent at any rate, and which can be used for deriving general rules in regard to binocular instruments. It seems to me therefore that it is altogether out of the question to develop the theory of binocular instruments wholly on this basis at first. At least it will be desirable to consider once more the significance of the rules that are obtained in this way.

Starting with this assumption, we must infer that the *visual impression will correspond with any one of the space-images that can be obtained with the actually existing retinal images by varying the convergence of the eyes in any way we like.* So far as the visual impression is concerned, the convergence of the eyes (as it may be called here for brevity) which constitutes one variable in the determination of the space-image and makes it unique, disappears; and so, to begin with, there is the possibility that the visual impression may consist of the totality of space-images obtained by the variation of this factor. It would be impossible to say offhand which of the many impressions that might be produced in this way is the one actually realized; nor could it be stated generally anyhow, because it depends in an intricate way on the numerous details that go to determine the impressions of distance.

The relations between the various space-images can easily be seen in some respects at least. Thus a more distant object must not only be larger but relatively deeper than a nearer one in order to give retinal images of equal frontal dimensions and binocular parallax. Consequently, on the assumptions made here, even with definite retinal images, it will be possible to have a series of visual impressions

[1] Suppose that E_1, E_2 denote the real, and E_1', E_2' the apparent, distances of the two points; then, on the above assumption, the parallax between E_1' and E_2' would be equal to that between E_1 and E_2, and the same change of convergence would be necessary in changing the fixation from E_1' to E_2' as from E_1 to E_2.

increasing in size with increase of distance and becoming relatively deeper, so far as binocular perception of depth enters into these impressions. In case of retinal images which would produce an orthoplastic or homœomorphous visual impression at a given distance, the impression, therefore, will be *hypoplastic* when the apparent distance is less than that distance, and *hyperplastic* when it is greater. It is worth while to pause here to illustrate this relation by one of the special cases mentioned above, for example, in the case of the simple telestereoscope. We have explained that this instrument gives an homœomorphous space-image, reduced in the same ratio as that of the interpupillary distance to the interval between the virtual images of the eyes in the two pairs of mirrors. As might be anticipated from what has been said, the visual impression actually will correspond to a reduced model of this kind, provided the object actually is seen at the correspondingly reduced distance; but, perceived at other distances, it will appear changed in form also (the relief being exaggerated, for instance, when the distance is greater than it should be). But it is impossible to give any definite rule for the occurrence of one impression rather than the other. In fact this is confirmed by experience, and the nature of the impression produced by the telestereoscope will depend to a large extent on personal idiosyncrasies and incidentally on the nature of the observed objects also. Thus HELMHOLTZ states that he saw objects (figures of persons, for instance) in correct proportions but very much reduced in size. On the other hand, GRÜTZNER[1] reports that in his own case the objects did not appear so much reduced in size, but that the relief was apparently exaggerated.

It is impossible to say offhand what is responsible for this individual difference. Possibly it has some connection with the fact (to be considered later) that the amount of convergence of the eyes may not be an unimportant factor in determining the distance at which visual objects are perceived, and that this factor may have a different significance for different individuals. The truth is that it was probably an unusually important factor in HELMHOLTZ's case; and that may have been the real reason why, in looking through a telestereoscope with the mirrors exactly parallel (in which case the convergence will be diminished in the same proportion as the apparent interpupillary distance is increased), he perceived the object at the short distance and therefore in its correct form. To some extent this is borne out by what HELMHOLTZ says about how the impressions are modified by changing the adjustment of the mirrors; which apparently had a very positive effect on the visual impression in his case.—However, in the case of objects whose forms are familiar, it might also be possible that the impression of distance was determined directly by the relation between the frontal values and the binocular depth-values. This, too, is a detail in the mode of appearance that changes regularly as the distance is changed; and hence, under certain conditions, it might very easily be the decisive factor in the impression of

[1] PFLÜGERS *Archiv*, XC. 1902. p. 525.

distance, exactly in the same way as the visual angle is in the case of objects of known dimensions. If this were so, it would mean a visual impression corresponding to just that distance at which the object really should be to produce the existing relation between frontal values and binocular parallaxes. Here again it would not be surprising to find great differences among individuals in this respect, and, as v. ROHR intimates, it is quite possible that HELMHOLTZ's mode of vision, as gathered from what he tells us about it, was remarkable for a certain accuracy in the way of regarding particular space-relations, indicative of unusually good eyesight in some respects.

If we look at the subject now in a broader way, we see, to begin with, that, exactly as was the case with an orthomorphous space-image, the condition $\alpha = 1$ has to be fulfilled in order to get an orthomorphous visual impression; and, conversely, that such an impression will generally be out of the question if the telescopic power is different from unity. This is an immediate consequence of the fact previously noted, namely that, no matter what the distance of the space-image is, an homœomorphous visual impression cannot be obtained by retinal images produced with a telescopic power different from unity. And since, on the assumption made here, the visual impression must be in accordance with any one of these space-images, an orthomorphous visual impression will not be possible either. On the other hand, the same considerations show that *orthoplastic* visual impressions can be obtained with any telescopic power and with any change of base-line, the conditions therefor being given by the preceding discussion. The apparent distance of the visual impression must be equal to the so-called orthoplastic distance, whose value was found to be A/δ. But, while the space-image can be produced at any desired distance merely by adjusting the optical instrument in a particular way, this is not true with respect to the visual impressions, simply because, as already explained, the distance is determined in their case by a series of circumstances of various kinds which are so complicated that often there is no possible way of defining them precisely. The consequence is that, without considering the special conditions of the particular type of optical instrument, the nature of the observed objects, etc., it is utterly impossible to formulate any general rules from the condition that the visual impression shall be homœomorphous or orthoplastic. In fact, it is impossible even to say whether any such rules can be given at all. Accordingly, we have reached the point now where we must begin to be more specific and where we shall have to consider separately the principal types of binocular optical instruments.

Let us take first the case of the *binocular telescope*. It goes without saying that for the general purposes of this instrument a telescopic power is required different from unity; and consequently, according

to the foregoing, it will not be possible to obtain homœomorphous space-images or visual impressions. However, an *orthoplastic* effect can be produced, the condition being that the apparent distance (S) of the observed object shall be equal to A/δ. Accordingly, the ratio between the distance at which the object is perceived and its real distance must be equal to $1:\delta$. Its apparent dimensions will be then on the scale of $\mu = a/\delta$. In order to tell whether we can count on this being the case, and under what circumstances we can do so, we may look at the matter in a simple way at first. Ordinarily we are accustomed to seeing objects at very different distances. In particular, we have generally had ample opportunity of obtaining a *nearer* perception of the same objects as those seen in a telescope or of similar objects. To a certain extent it is even quite usual for us to see reduced reproductions of figures of this kind. On the other hand, reproductions on an enlarged scale are very rare. And so we can realize (as we know very well by experience) that the optical effect of a telescope does not consist in its *magnifying* the objects for us *absolutely*, but in its making them seem to be *nearer* to us. Consequently, if we may suppose here also that, instead of seeing the objects absolutely magnified, we see them in their true size at most, the result will be that, whenever the condition for producing the orthoplastic effect involves a value of μ greater than unity (that is, whenever $a > \delta$), the "plastic" effect will be always faulty, that is, it will be *hypoplastic* or too flat in appearance. Now it is well-known that this is the effect produced by an ordinary binocular telescope with no base-line enlargement. In this instrument $\delta = 1$, and of course $a > 1$; and the flattening effect obtained here is a well-known phenomenon.[1]

Moreover, proceeding on the assumption that the objects are seen in their *natural* size and at distances correspondingly reduced (which perhaps would be the most probable assumption in many cases), we can readily see how the instrument ought to be made in order to give orthoplastic impressions. Thus if the scale is to be unity ($\mu = 1$), then, since $\mu = a/\delta$ (according to the orthoplastic condition), *the enlargement of the base-line must be equal to the telescopic power*. Hence, it is evident that this special case, which HELMHOLTZ distinguished particularly and which has been the subject of much discussion recently, does in

[1] Perhaps, it might be added that the familiar flattening effect of the ordinary binocular telescope cannot be accounted for in a perfectly satisfactory way without considering the special physiological relations of the visual impression. Were we to see objects at their real distances and correspondingly magnified in absolute size (as would be thoroughly possible according to the scale of the physical relations), presumably they would give a correct "plastic" impression also. It is only on account of the special physiological conditions of vision that generally it is impossible to do this.

fact possess a peculiar importance. At any rate as to the frontal values and binocular parallaxes, the optical effects are the same as would be produced by bringing the object nearer the observer without changing its dimensions. It is possible therefore to get a visual impression of the object in its natural size and with correct "plastic" effect (in this limited sense). And since we may suppose that the objects actually are perceived to a great extent in their natural size and at the corresponding distance, we are justified in saying that the conditions which are comparatively most conducive for obtaining a correct "plastic" effect are also created by an optical system in which the telescopic power is equal to the base-line enlargement.

This result has been obtained by considering a case that is peculiar in some respects and it has been shown why the telescopic power should be the same as the base-line arrangement in this special case; and to that extent our method is similar to the arguments given by HELMHOLTZ in the text. But (as v. ROHR has very justly remarked[1]), it is not correct, or at least not strictly correct, to say, as HELMHOLTZ does, that an instrument whose base-line enlargement is 16, and whose magnifying power is also 16, will produce just the same effect as if the object were 16 times nearer to the observer than it really is seen by him, and viewed then with his unaided eyes.

This may perhaps be true in the limited sense explained above, but it is not true with respect to the monocular depth-values. Obviously, by actually bringing the object closer, we cannot obtain the total variations of perspective.

On the other hand, we cannot altogether agree with v. ROHR when he says that, so far as the orthomorphous effect is concerned, there is no advantage whatever in making the base-line enlargement equal to the telescopic power. Of course, it is true that the space-image will always be porrhallactic in this case, and consequently the visual impression will also be wrong in some ways. Still it may be that this is just the case when those faults are particularly trivial in amount or less important than usual, and in my opinion this is indeed the fact.—In this connection there is another matter which I believe calls for some explanation. v. ROHR says (*loc. cit.*, p. 87) that it is utterly incomprehensible why HELMHOLTZ in his work on physiological optics did not reproduce the exceedingly clear and correct exposition of the theory of the telestereoscope which he had published previously in 1857 in Volume CII of POGGENDORFF's *Annalen*. I cannot share this opinion. The truth is very likely that HELMHOLTZ's reason for giving a different explanation of this instrument was due to the fact that in the meantime he had modified and, as we may say, corrected his view on one particular point. Moreover, some other facts seem to indicate that in 1857 when HELMHOLTZ described the telestereoscope for the first time, he was still disposed to overestimate the importance of the convergence of the eyes for the visual determination of distance.

Indeed in the earlier description of this instrument, the assumption is made at the start that the distance as seen depends directly on the convergence of the eyes. The statement there is as follows (*loc. cit.*, p. 174):

"Although each telescope separately presents the object to the observer as it would look if it were n times nearer to him, yet the differences between the perspective views in the two eyes are not so large as they would be if the

[1] *Die binokularen Instrumente.* Berlin 1907. p. 87.

object were actually n times closer to the observer. This fault cannot be remedied by connecting a double telescope with a telestereoscope that has two pairs of parallel mirrors.

"All that will be accomplished in this way will be a further uniform reduction of all the apparent linear dimensions as they appeared in the double telescope itself. However, it is true that a correct relief may be obtained in the case of individual objects that happen to be at a particular distance, by letting the smaller mirror stay at 45° and simply adjusting the larger one so that the light will be reflected from it at an angle somewhat less than 45°. Thus, ... in the simple telestereoscope, with no magnifying glasses, it is possible to obtain an exaggerated relief, and therefore this device can be used to offset the opposite errors that are produced with the telescope-combination."

Now this description would be correct, provided the distance at which the object was *perceived* was determined only by the degree of convergence of the eyes.[1] In this case, just as the image produced by a telestereoscope without any magnifying power is geometrically similar to the thing seen by the unaided eye, only it is reduced, so likewise the effect of combining a telestereoscope with a binocular telescope will be to give a reduced copy of what would be seen by the ordinary binocular telescope alone (that is, by a binocular telescope without any base-line enlargement); and hence the flattening effect would persist unchanged. But the condition on which this statement depends is *not* fulfilled: the apparent distance is not determined positively at all by the amount of convergence of the eyes. Consequently, it is not necessary at all to add the telestereoscope in order to make the flattened image in the ordinary binocular telescope appear to be nearer (because, as a matter of fact, the relief would be just the same); but even if the image is perceived at the same distance as it was at first (in spite of the reduced convergence of the eyes), the fault of flattening will be corrected under these circumstances by the base-line enlargement.

Accordingly, the base-line enlargement is quite capable of abolishing, or minimizing at least, the flattening which is the characteristic fault of the ordinary binocular telescope; and *this* is what does it in this instance, and not any change in the adjustment of the mirrors or in the degree of convergence of the eyes.

I am therefore of the opinion that HELMHOLTZ's subsequent description, in which he considers that the flattening produced by the telescope can be abolished by the telestereoscope, and in which therefore he does not attach any particular importance to the mirrors not being exactly parallel, is more correct than his original article on this subject; and, undoubtedly, he was led to revise his description in consequence of the general change in his view of the matter that was previously mentioned.

However, there is the same mistake in the earlier article as to the monocular relations which we have criticized in his later discussion. The truth is that, when HELMHOLTZ says that each telescope presents the object just as it would look if it were n times nearer the observer, the statement is not absolutely correct as to the monocular appearance. When the objects are brought n times nearer, it involves a series of changes in their arrangement as seen by one eye that are not involved in the case of telescopic magnification.

It will not be amiss to say expressly that there is nothing in all this discussion that should be taken to mean that the rule given above must not be observed in the construction of optical instruments. In the first place,

[1] Or we may say that while the description is applicable to the space-image in the sense in which the term is used here, it does not apply to the visual impression at all.

it should be remembered that the assumption on which we have proceeded was a conditional one—namely, the assumption of a proportionate binocular perception of depth—and we shall have to return to that again presently; and in the second place, the realization of a correct "plastic" effect is not the only thing that has to be considered in designing optical instruments of this kind. There are technical reasons why the limitations of the base-line are much greater than those with respect to telescopic power. The objects can be made to appear considerably closer by increasing the telescopic power, and that enables us to make out the relative configuration of the details at least. Advantages of this sort may be worth obtaining at the expense of relative reduction of the relief or an apparent flattening.

The conditions in the case of the *binocular microscope* are different to begin with, because the absence of telescopic power (that is, the fulfillment of the requirement $\alpha=1$, according to our notation) is not inconsistent with the general purposes of the instrument. Suppose that a microscope is so designed that when the eye is at O_l (or O_r), as represented in Fig. 79, the angular configuration of all the points of the object is precisely the same as it would be with respect to another point o_l (or o_r) which was situated very close to the object; then the required condition ($\alpha=1$) would be fulfilled. The microscope would still have a certain magnifying power (according to the ordinary definition of that function for this instrument), its value being determined by the ratio between the so-called distance of distinct vision and the distance of the object from the point O. We might briefly describe the imagery in this case by saying that, instead of magnifying in the way a telescope does, the instrument would behave on the order of a magnifying glass.[1]

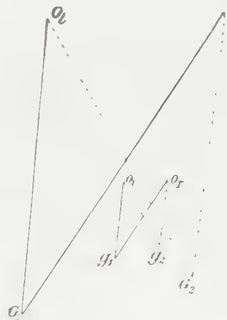

Fig. 79.

If, as is usually the case, the entrance-point is in the objective, the distance between it and the object will be approximately the same as the focal length (f) of the objective, and the magnifying power will be equal to $250/f$ (if f is expressed in mm).

Suppose that we have two of these microscopes adjusted with the centres of their exit-pupils at the same distance apart as the inter-pupillary distance ($2a$) between the two eyes, whereas the distance between the two entrance-points is equal to b, then, from the same considerations as in the case of the telestereoscope, it is evident that this binocular instrument will produce a space-image which will be

[1] The condition here formulated is equivalent to that which Czapski expresses by saying that the nodal points of the instrument must be at the centres of the entrance-pupil and exit-pupil. (*Zft. f. Mikroskopie.* XIV. 1897).

similar to the object geometrically and magnified as compared with the real object in the ratio of $b:2a$. The positions G_1, G_2 of the points in the space-image corresponding to two real points g_1, g_2 may be found by determining the points of intersection of the two pairs of lines drawn from O_l, O_r parallel to $o_l\, g_1$, $o_r\, g_1$ and $o_l\, g_2$, $o_r\, g_2$, respectively.

But the conditions in the case of the binocular microscope are entirely different from those in the telescope, being in fact simpler, because in the microscope we can usually assume that the objects are perceived approximately at a definite distance, namely, at the so-called distance of distinct vision (250 mm). On this assumption, we can state at once the condition for the production of an homœomorphous visual impression, by saying that the orthomorphous distance must be equal to the distance of distinct vision. If A denotes the distance between the object and the entrance-point, the orthomorphous distance will be equal to A/δ, and since in this case $A = f$ and $\delta = b/2a$, we obtain, therefore:

$$\frac{2\,a\,f}{b} = 250,$$

where the distances are all given in millimetres. Thus the base-line must be in the same ratio to the interpupillary distance as the focal length of the objective is to the distance of distinct vision. According to the suggestion of the American zoölogist GREENHOUGH, ZEISS has constructed a binocular microscope satisfying the above requirements.

Only one or two points need to be noted here in regard to the more specific technical conditions. For obvious reasons, it is desirable that the point lying about at the centre of the image should be on the axes of the two microscopes. At the same time the distance of this point must be approximately the same as the focal length of the objective; and hence the angle between the axes of the two objectives will be determined by the focal length and the interval between the two objectives, that is, by the magnitudes denoted by f and b. If the microscope consisted simply of a centered system of lenses traversed by the axial ray without deviation, the length of the tube would be determined by the same conditions also; that is, the latter would have to be so long that for the given angle between the two axes, the distance between the two exit-pupils would be equal to the interpupillary distance ($2a$). Then the space-image would coincide with the place where the real point was at the point of intersection of the two axes. However, it is not practicable to construct the instrument in this simple way; and it actually is made by producing a parallel displacement by means of the system of PORRO prisms used for making the image erect; this displacement being varied in a different way in the two tubes. This makes it possible to alter the distance between the

oculars within certain limits without changing the length of the tube, and so to adapt the binocular microscope to the observer's interpupillary distance; which is a practical matter of much importance.

As to the special optical construction, especially in regard to the erection of the image and the fulfillment of the requirement that the instrument shall not have any telescopic power, all we can do here is to refer to the descriptions of the instrument as published by its designers.[1]

In accordance with the rules given above, the magnifying power of the instrument will be $2a/b$ or $250/f$, where f is supposed to be expressed in mm. This magnifying power is necessarily rather low, because for practical reasons the separation of the two objectives, that is, the base-line, cannot be made less than a certain value. We ought to mention here, therefore, that it would be possible to obtain more highly magnified visual impressions by using oculars of higher power, thereby introducing a telescopic power; and while these impressions would not be homœomorphous, they would be orthoplastic. So far as the orthoplastic distance was concerned, it would not be necessary to change the conditions of construction; that is, this distance would still have to be made equal to the distance of distinct vision (250 mm), which, as we saw, was equal to $2af/b$.

Accordingly, in this case, also, $2af/b = 250$ mm; that is, the ratio between the focal length of each of the objectives and the conventional distance of 250 mm must be the same as the ratio between the base-line and the interpupillary distance; and, just as before, this condition imposes certain limiting values both for the base-line and the focal length. But a higher magnifying power could be obtained by introducing the telescopic power, because the former is equal here to $2a\alpha/b$. If the orthoplastic visual impression can be regarded as being a satisfactory substitute for the homœomorphous impression, it might be worth while to consider whether it really is better to sacrifice the higher magnification for the sake of obtaining an absolutely homœomorphous impression, or whether it may not be sometimes desirable to produce a highly magnified orthoplastic impression.

We shall take up next the question of the *production and observation of stereoscopic photographs*. In this case, as has been explained, the telescopic power is equal to P/B, where P denotes the distance of the plate from the camera lens and B denotes the distance at which the image is viewed. Suppose that the distance between the centres of the two camera lenses is δ times the interpupillary distance (that is, is $2a\delta$); then, according to the above, if A denotes the distance between the camera and an object, and S denotes the apparent distance of the visual impression, the condition that this impression shall be not simply orthoplastic, but homœomorphous, will be $S = A/\delta$. If this condition is fulfilled, the visual impression will be homœomorphous, and in case $P = B$, the scale of it will be $1:\delta$; but if P and B are different, the impression will be orthoplastic, and the scale of it will be $P/B\delta$.

[1] Czapski u. Gebhardt, *Zeitschr. für Mikroskopie.* XIV. 1897.

The question arises as to what rules we can derive from these results for taking stereoscopic photographs and then viewing them. We may say, to begin with, that here (as was the case with the binocular microscope, but not the case with the binocular telescope) it is generally possible to realize the aim of producing an impression that is not simply orthoplastic but homœomorphous (by making $P = B$, that is, by making the distance between the plate and the camera lens equal to the so-called viewing distance). And we shall suppose at first that this requirement is fulfilled; although afterwards we shall examine the reasons for departing from this rule in some ways, and the consequences that will be involved thereby. We may also state at once that the retinal images will not be altered by varying both plate-distance and viewing distance in the same way; and so, as far as the result is concerned, it does not make any particular difference and is not of any real practical importance whether these distances are large or small, provided the same mutual relation between them is maintained. Lastly, it should be noted that the circumstance that determines the distance A at which the photographs are to be taken is the question as to the special objects or parts of objects that we wish to combine in a unitary image. Generally, at any rate, these objects will be comprised within a certain angular region which, however, has to be approximately filled by them; and hence the *camera-distance* (or distance between the camera and the object) may be regarded as being determined by these conditions which cannot be specified exactly by any general rule. This being the case, the main question that remains to be considered, so far as taking the photographs is concerned, is as to the *camera base-line*. This is a matter which has been the subject of a great deal of discussion lately. The preceding analysis enables us to formulate a perfectly fixed rule for it, provided we can assume a definite value for the apparent distance (S) of the visual impression as being given once for all. That being the case, we have $\delta = A/S$.

On the assumption that the apparent distance of the scene in the stereoscope is definite and fixed, the camera base-line and the interpupillary distance must be in the same ratio as the camera-distance and this fixed distance of vision; in other words, *the camera base-line and the camera-distance must be varied in the same proportion.*

This rule will probably be found to apply very well to a certain extent with some particular kinds of objects, especially *microscopical* preparations. Since we are in the habit of examining such objects at the so-called distance of distinct vision, it might be natural to suppose that we would localize their reproductions in the stereoscope about at this same distance also. Assuming that this is so, we can write:

$$\delta = \frac{A}{250},$$

where the distance A is expressed in millimetres.

If the camera-distance (A) is supposed to be equal (approximately) to the focal length (f) of the microphotographic objective, the ratio between the base-line (b) connecting the centres of the two objectives and the interpupillary distance ($2a$) will be the same as the ratio between the focal length (f) and the distance of distinct vision; that is,

$$b = \frac{2\,af}{250},$$

where the distances are all measured in millimetres.

Of course, we must not lose sight of the fact that there is already some question here as to the assumption on which these results depend, the assumption, namely, that the object is perceived at the distance of 250 mm; for in looking through a stereoscope the conditions and the habit of perception may be quite different from those in the case of microscopy; and therefore it is very possible that the observer may see the reproduction or model in the stereoscope at a distance which is not equal to 250 mm, but which may be greater than that. If that is the case, and if the conditions have been correctly determined for the distance of 250 mm, the result will be a false "plastic" effect and exaggerated relief. The apparent distance of the figure seen in the stereoscope is also often estimated to be much more than 250 mm, sometimes as much as 350 mm.

But with respect to most of the objects generally portrayed in stereoscopic pictures, such as landscapes, architecture, sculpture, human beings, animals, etc., the conditions are altogether different from the special case considered above; because here experience more than anything else tells us that the figures represented in the stereoscopic view may be at very different apparent distances. If we pause to consider how this is and on what the distances depend, we may suppose, as intimated above, that the mere fact of *knowing the forms* of some objects contributes to determine the impression of distance and enables us to obtain an idea that is about correct, being neither hyperplastic nor hypoplastic. Indeed when the camera base-line exceeds the interpupillary distance considerably, we do see reduced models that are fairly correct in form. And so in a case where the objects are things with which we happen to be familiar, more or less latitude will be allowed for the camera base-line by the condition of orthomorphism, owing to the fact that the apparent distance of the observed figure will be adjusted of itself, so to speak, to produce this

appearance. Therefore, according to this assumption, the condition that should be satisfied will be *to make the camera base-line and the interpupillary distance equal, if it is desired to get an impression of the object in its natural size; but to use a larger base-line, in order to get the impression of a reduced model. In the latter case, the scale of the reduced model will be $\delta:1$, when the base-line is δ times the interpupillary distance.* Which of these two impressions it will be better to obtain, is a question which must be decided on other grounds than that of orthomorphism. We shall allude to it again.

On the other hand, at the opposite extreme, so to speak, in *the case when the form of the objects is entirely unknown*, it will be quite impossible to be at all certain about the apparent distance of the visual impression, and so, no matter how the camera base-line happens to be chosen, the production of an orthomorphous effect will be altogether problematical. We find that this is true, for instance, with reference to stereoscopic views of landscapes in which there are objects such as rocks, mountains, etc. By combining stereoscopically pictures which are taken from two stations that are very far apart, the impression is produced in a very beautiful and striking manner of a small model at a short distance away, as was stated in the text. But *how* far off or *how* big it appears to be, it will be impossible to say; and hence there is always the chance that it may not be seen at the orthoplastic distance and therefore in a false relief, the depth-values being either too great or too small.

Some further observations may be added here concerning the conditions mentioned first where a certain latitude was allowable for orthomorphism. It should be noted in the first place that this scope is necessarily limited by the existence of a certain range of distance within which a visual impression must lie. Thus, to begin with, the so-called distance of distinct vision may be considered as being the lower limit in this case. One of the results of the general habits of vision we have formed is that we seldom ever view objects at distances less than 250 mm; and if camera-distance and base-line happen to be such that the orthomorphous distance turns out to be less than this value, it is natural to expect that instead of seeing the objects at this distance, we shall see them farther away and in exaggerated relief. This is the explanation of the remarkable hyperplastic distortions that are produced by looking at near objects through a telestereoscope. A point which may be of even greater practical importance in this case is the fact that quite often at any rate there is an upper limit also for the apparent distance of the thing viewed in the stereoscope. In fact we must remember that the conditions are peculiar in the case of the stereoscope and altogether different from those in the

binocular telescope, for instance; because, as the spectator is well aware, no real objects are actually present, but the things under observation are artificial pictures. Besides, even when the pictures are excellent and absolutely faithful in the portrayal of the forms represented, the absence of colours, the conditions of illumination, frequently the lack of motion, etc., tend to prevent us from getting the impression of viewing real things. And so it is quite seldom that the effect produced on the spectator can be said to be a real illusion, whereas he does perhaps get the impression, at least very often, that there is a copy before him on the order of a model. Now whenever this is so, it is impossible for the impression to arise of a very great distance; and so if some of the objects in the picture are things that ought to be very far away and whose apparent distance even in a reduced model would have to be very great in order to obtain the correct relief, these objects will not be perceived at the proper distance, but at too small a distance, the relief therefore being too low. The truth is that the stereoscopic view of a landscape often gives the notion of a shallow background as if it were painted on a screen.

Finally, let us return again to the question as to how the base-line should be chosen, when the condition of orthomorphism allows more or less latitude in this respect. In regard to this, there is a very prevalent idea concerning the province of stereoscopy, which in my judgment is certainly too narrow; the idea, namely, that under all circumstances the endeavour should be made to obtain an appearance that is "true to nature." This requirement would mean that the objects ought to appear in their natural size; that is, according to our notation, we ought to have $\mu = 1$, and hence also $\delta = 1$. Consequently the camera base-line and the interpupillary distance should be equal.

Obvious as this requirement may seem at first sight, still when we come to test it more carefully, we find that it is correct only to a limited extent at any rate. It may be considered as applying in the case where the one thing to be obtained is as life-like an illusion as possible. This may be desirable very often, but it is not always so; and consequently I believe we ought not to underestimate the importance of those portrayals which give the impression of reduced models.

Representations of this sort have long been recognized as being exceedingly useful for scientific and educational purposes, and therefore no special explanation is needed here. But it seems to me that, besides their value from a purely intellectual point of view, they are of importance from an aesthetic standpoint also. If the reduced model enables us to perceive and to comprehend the entire space-form of larger figures in a manner which would be utterly impossible by looking at the object itself, and especially if it enables us to survey a large

portion of it and at the same time obtain a correct perception of the relief, there is no reason why we might not suppose that in many instances a model of this sort would be more attractive and more impressive than a representation of the object in its real size.

What I have especially in mind here is architecture (both interior and exterior) and sculpture as well. Such considerations will be of all the more weight if there are also other reasons for not wanting to produce a real illusion; and often this is the case, as has been said. Accordingly, on purely physical or physiological grounds, it is just as hard to say when and how much the base-line should be enlarged as it would be to judge the value of a portrait that was more than life-size. The final decision of a question of this nature demands an artistic sense formed and developed by large experience.

So far we have examined only those stereoscopic systems without telescopic power that permitted us to obtain absolutely homœomorphous visual impressions. As was hinted above, we have still to inquire whether it may not be better, under some circumstances, to be content with getting orthoplastic impressions, and if so, how the rules which have been found for taking and viewing the pictures would be modified in consequence.

By choosing a viewing distance that is less than the plate-distance, a telescopic power a will be introduced; and then (on the assumptions always made in this discussion) the result would be that, instead of seeing a similar model on the scale of $1:\delta$, we should see an orthoplastic figure whose frontal values and depth-dimensions would be in the ratio of $a:\delta$. Undoubtedly this view would possess one advantage from the fact that there would be an increase of apparent size at any rate, so that finer details could be discerned and the resolving power would thereby be increased, etc. Still in order to form a proper estimate of the value of this method, we should remember that in general there is nothing to prevent us from producing a perfectly homœomorphous model on the scale of $a:\delta$. We simply have to reduce the camera-distance and the base-line together in the ratio of $a:1$, and then the orthomorphous distance will be just the same as before, and the scale of the visual impression will be increased in the ratio of $1:a$.

Inasmuch, therefore, as we are at perfect liberty to do just as we like, we may just as well keep to the conditions for obtaining the orthomorphous impression.

However, there is still another thing that has to be taken into account. We have seen that we cannot be absolutely sure that the stereoscopic figure will have the correct appearance. On the contrary, it was expressly stated that in case of a large number of objects it was

important to know something about them in advance in order to see them at the proper distance and so to obtain a correct notion of their form also. We may perhaps conjecture that such circumstances would weigh more and be more reliable for impressions that were absolutely true to nature than in the case of those that had to be merely orthoplastic, and that therefore orthomorphous impressions could be obtained with more certainty than orthoplastic ones.

However, we must recall here first of all the limiting condition on which we deduced the possibility and meaning of an orthoplastic effect. The assumption we made was that the dimensions of the objects were small as compared with their distance. Now this assumption may be considered as admissible in the case of objects viewed through binocular telescopes and microscopes; but some of the angular dimensions in stereoscopic pictures are apt to be very considerable and the objects depicted are often also at very different distances; and the whole scene, therefore, may comprise a very great extent of depth. Now it is evident at once that the two conditions deduced above, namely, the condition that the ratio between the apparent orthoplastic distance and the true distance shall be $1:\delta$, and the condition that the depth-dimensions shall be seen on the scale of $\alpha:\delta$, are, in general, incompatible with each other. For objects of very slight depths, they are approximately fulfilled; and then the depth-extension may be seen on the scale of $\alpha:\delta$, and at the same time the apparent distance of the object as a whole may be approximately equal to $1/\delta$ of its real distance. But with objects that are deeper, this will not be the case. If here a point were seen at the orthoplastic distance ($1/\delta$ of its real distance), and if the distance of another point from it were seen on the scale of $\alpha:\delta$, the latter point would not be seen at the orthoplastic distance, but at a distance appreciably different from it. In other words, when the viewing distance is not the same as the plate-distance, we cannot possibly have a perception in which the relations between frontal values and depth-values will be everywhere correct; that is, on the assumption of a proportionate depth-perception, as has been invariably supposed throughout this discussion. We might naturally expect, therefore, that that relation will be only partially realized, whereas in other parts there will be variations from it; or it may be that the perception will vary, so that, for example, a point on being fixated will seem to be at a different distance from that at which it seemed to be before (when it was seen indirectly on looking at another point), etc.[1]

[1] While these disturbances might be expected on theoretical grounds, it would certainly be difficult to say how they are actually *noticed*, or to what extent. Doubtless, this will depend in large measure on the peculiar nature of the object and also on the idiosyncrasy of the observer. We shall refer to this again.

It is to be understood hereafter that, whenever there is supposed to be a systematic connection between taking the photographs and viewing them stereoscopically, the conditions of orthomorphism are satisfied; that is, viewing distance is equal to the plate-distance. The only case where we might have to consider exceptions to this rule would be when, for some reason or other, the choice of the distance between camera and object is limited, as, for example, when the objects are very far away. Under such circumstances, the only way by which the objects as seen in the stereoscope can be made to subtend visual angles of relatively large size will be by using photographic lenses of longer focus; in which case, therefore, the plate-distance will exceed the viewing distance. And it may be worth while to sacrifice perfect homœomorphism for the sake of the advantage to be gained in this way.

If certain pictures are given to begin with, and the question is simply how they had better be viewed stereoscopically, that will be a different matter. Obviously we can get a great variety of effects by selecting different viewing distances. For example, we can make the objects appear relatively larger by viewing the pictures at a nearer distance. Of course, the disadvantage about this is that we cannot count on getting homœomorphous impressions, but only orthoplastic ones at most. Whether this is serious enough to warrant us in not using this method at all, or at any rate to make it seem inadvisable, is a question we shall not undertake to decide.

The problem of making photographs for stereoscopic views has been the subject of much discussion recently. From the standpoint of physics especially, much prominence has been given to the general condition for orthomorphism (namely, the condition of equality between viewing distance and plate-distance). However, the whole subject has been obscured in a certain way by viewing it differently according to the effect to be obtained. On the one hand, the purpose kept in mind was how to obtain a visual impression that was *congruent* with the real objects; whereas, on the other hand, all that was proposed was to get an impression that was *similar* to the objects in a geometrical way. Much confusion has been caused by the use of the term *orthostereoscopy* in both of these senses.

I shall mention Stolze[1] first as the leading representative of a view that is very common and that is certainly very natural in some ways, namely, that the duty of the stereoscope is simply and solely to create a substitute for viewing the object itself with the naked eye; and that all that is necessary in order to accomplish this purpose is to obtain the same retinal images as would be produced by looking at the real objects with the unaided eyes (which usually implies that the base-line must be equal to the interpupillary distance). Now, in the first place, this is a rather one-sided way of considering the question, because, as we have shown, it does not attach sufficient importance to the uses and value of having a simple reduced model which can be observed. But another objection is that it appears to imply that all

[1] *Die Stereoskopie und das Stereoskop.* 2. Aufl. Halle 1908.

the complex physiological conditions involved in the visual impression can be left out of account. STOLZE's conception is that, provided the retinal images are in complete accord with those produced by the real objects themselves, the visual impression will undoubtedly be the same in both cases. The answer to that is that perfect agreement as to colour, luminosity, etc., obviously can hardly ever be attained. But the physiological effect is not determined solely by the production of a picture that harmonizes as nearly as possible with the object in its space-relations. It depends rather on very various conditions, affecting the impression of distance in the first place and at the same time indirectly affecting also the different mode of appearance of the thing that is seen in the stereoscope. It is just such important characteristics of this sort that *cannot* be imitated or reproduced by photographs of real objects. When these conditions are taken into account, it will be obvious that we have no right to disregard entirely the physiological conditions of the impression of distance; and this is the reason therefore why no perfectly simple general rule can be given for the choice of the camera base-line.

A different plan has been followed by many other writers by trying to determine what the conditions are in the case of the stereoscope for obtaining the impression of a model geometrically like the original, neither hyperplastic nor hypoplastic. Starting from a point of view quite like that which we have adopted here, HEINE[1] has rightly insisted on the fact that the impression of relief is connected in a positive manner with the apparent absolute distance of the object as viewed in the stereoscope. Moreover, he has stated the rule quite correctly according to which (on the assumption of a given stereoscopic distance) the camera base-line and the interpupillary distance must be in the same ratio as the distance of the object from the camera is to that apparent distance. And he was able to demonstrate that when these conditions were fulfilled, a row of objects that were very different as to size and distance would be seen in correct relief. It is true that the conditions in this case were chosen so that they were particularly favourable for making the models appear to be at the given distance (350 mm). And while HEINE's description perhaps does not make this point as clear as it might be, it ought to be noted that we never can be absolutely sure that the apparent distance of the object seen in the stereoscope does have this definite value of 350 mm; and the mere fact that the angle of convergence was made equal to 11° when the pictures were being viewed is no guarantee at all that the models really were at that given distance. HEINE has also proved, in a thoroughly correct way, in my opinion, that the apparent distance of the stereoscopic visual impression in the case of landscapes and views of that kind may vary exceedingly, and that it is therefore impossible to give any definite rules here for the choice of the base-line. On the other hand, KOTHE's criticism[2] of HEINE's rule is due to the misunderstanding previously mentioned. KOTHE says: "If this procedure were right, the two half-images would have to be equal to the inversions of the retinal images, as the latter would be produced . . . . by looking at the object; that is, the stereoscopic photograph would have to give the same retinal image as the real object itself." However, this requirement, which KOTHE calls "the most important law of orthostereoscopy," is not justified unless our aim is to produce an impression that is *congruent* with the original. But what HEINE had in mind was to give the impression of a geometrically similar model. As a matter of fact, under the conditions which he gives, the retinal images made by the photographs are the same as would be made by a model of this kind when it was viewed at a certain distance.

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[1] *Archiv f. Ophth.* LIII. 1902. p. 306.

[2] *Zeitschrift für wissensch. Photographie.* I. 1903. p. 319.

In an earlier part of this volume, where some reference was made to the illusions noticed by ELSCHNIG[1] in which spherical objects appeared to be "over-plastic," the explanation was found to be in the special relations of the apparent contours of round bodies; and so it will not be necessary to consider ELSCHNIG's own theory of this phenomenon or the other explanations that have been offered.

There are several matters that still need to be discussed in connection with this whole subject, some of which were left open at the time for special consideration later. In the first place, the reasons ought to be stated why we have a right to consider the so-called orthoplastic impressions as the next best substitute for orthomorphous impressions, although of course they are not equivalent to each other; and why we should aim therefore to produce the former when it is impossible to have the latter. It will be recalled that the orthoplastic figure agrees with the orthomorphous one so far as the ratio between the frontal values and the binocular depth-values are concerned, but that the two are different with respect to the monocular depth-values. We are able now to appreciate in a certain way the significance of such a discrepancy. Under these circumstances, as above noted, the apparent configuration of the points inside a layer parallel to the frontal plane would be approximately as it should be, and the apparent depths of all points would be nearly correct also, but the apparent angular distance between two points at different depths would be incorrect by a finite amount. Accordingly, the effect of this would be to modify the perspective arrangement of points at different depths and particularly the directions of lines extending depthwise. Thus lines that were really perfectly sagittal would not appear to be quite so, etc. Except perhaps in the case of regular geometrical forms (such as parallelopipeds), I am disposed to think that such changes of form are of comparatively slight consequence, certainly less so than the effect of the variation of the ratio between the frontal dimensions and those of depth known as flattening or too great "plastic" effect. Generally, we are hardly aware of such variations, as is shown, for instance, by the fact that we are in the habit of viewing pictorial representations at totally different distances without specially considering that the image cannot really give a faithful rendition of the way the object would look unless it were viewed at a certain particular distance. Moreover, objects seen by looking through a telescope with one eye seem to be nearer generally without causing any annoyance because the perspective is not the same as it would be if the object really were closer. Thus, although it is impossible to say definitely what these disturbances really do amount to, in most cases I believe we can

[1] *Archiv f. Ophth.* LII. 1901. p. 294.

assume that they are of comparatively little importance, and especially that they will never be so conspicuous as the faults resulting from incorrect value of the ratio between frontal values and depth-values, which are so particularly in evidence in exaggerated reliefs and effects of shallowness. Naturally, their importance will depend on the peculiar character of the observed objects and probably on the observer's idiosyncrasies also.

Another matter, which was left out of consideration entirely at first, and which requires some discussion, is the question of the convergence of the eyes in viewing objects in binocular instruments.

It is certain that the degree of convergence of the eyes alone is not sufficient to determine the apparent distance of a point focused binocularly; and to that extent we were justified in not taking this factor into consideration. However, this should not be taken to imply that the degree of convergence is of no consequence at all, because, as a matter of fact, that is not true. On the contrary, there is reason to believe that it does have a certain importance for the perception of distance under some circumstances although it would be hard to estimate it exactly, especially as it varies greatly with different individuals. Consequently, while not exactly required, it would always be advisable to design an optical instrument in such manner that when the objects are viewed at just the right apparent distance to give the proper "plastic" effect, the vision of the two eyes will be reinforced by a corresponding degree of convergence. For example, in a binocular telescope the objects are intended to be made to look nearer, but still they are always far away; and so such instruments are evidently designed so that, instead of the eyes being very convergent, they are practically parallel. It is the same way in the case of a stereoscopic instrument intended for viewing close models very much reduced; the instrument ought to be adapted then to make the eyes have the necessary convergence. In this particular case, it is true, it might be more doubtful as to what would be best, because, on account of some other considerations (especially the matter of fatiguing the ocular muscles), it might be advisable to make the axes of the eyes practically parallel here too.

The last and most important point that still has to be considered concerns the assumption which we made in regard to the subjective depth-values corresponding to binocular parallaxes of any kind.

We proceeded on the supposition that when we got the impression from certain retinal images that an object was situated at the absolute distance E, this impression would be consistent likewise with the other characteristics that an object would have to have in order to produce the given retinal images. This idea was based primarily on the ab-

solutely similar assumption that had to be made concerning the binocular perception of depth in the case of ordinary vision with the naked eyes. We found that there was no good reason for supposing that there were any regular and definite exceptions to this general principle. And we may add here that neither is there any reason to think that in viewing artificial images the situation is different from what it would be in looking at the objects directly in the ordinary way. On this ground, it seems to me we are justified in starting with this principle at any rate. However, it is only fair to say that the experimental examination of this whole subject is at present still very incomplete; and so, strictly speaking, a discussion based on this principle cannot claim to be anything more than a preliminary endeavour to give a satisfactory explanation. Another thing, too, that should be expressly stated is that the values of the perceived depths as expressed in terms of the hypothetical principle cannot be regarded as being absolutely or mathematically exact. Besides, as is necessarily true of all the conditions of perceptions of depth, in all probability the values required here may be modified by very complex factors of various kinds. There is especially one possibility that certainly ought to be mentioned at least; and that is that binocular perception of depth may be something that cannot be measured in a quantitative sense, since it is more or less subject to empirical conditions.

If the latter is the case, we may conjecture that pictorial representations deviating more or less from the above rules will not be noticed at once as being out of the way. Now we certainly can venture to say that in such cases objects are presented to us (through the instrumentality of optical processes) in a form in which they never can be seen in reality; and that when we fail to be disturbed by this and are not conscious of any disadvantage in it, it simply implies lack of attention and observation, in short, lack of training in vision. However, I think it worth pointing out that there is also another way of looking at these facts. The objects depicted in a painting of any kind are also exhibited in a manner in which they never can be made to appear by looking directly at the things themselves with the eyes perfectly free. And yet by constantly looking at such pictures, undoubtedly we do get in the habit of viewing them in a way that corresponds to the objects portrayed, and this involves a certain development of the imagination. But the more it is developed, the more we shall come to realize finally that the view before us is one that never could be produced by real objects under any circumstances whatever. Thus the very thing which at first we are disposed to regard as a failure in seeing correctly, considered from another standpoint, may be regarded also as a useful training that enables us to understand certain pictorial

renditions. Anyhow, the question may well be asked whether the conditions are not similar in looking at stereoscopic views or in using a binocular telescope.

The matters involved in these last considerations are not concerned so much with rules that can be established for binocular processes as with the significance of such rules. The above considerations would seem to indicate that this significance is always a conditional one more or less; and, this being the case, it is easy to see how the effect of unavoidable departures from those rules (such as we have in the binocular telescope, for instance) may not apparently be a serious evil. However, at the same time there would seem to be some question as to whether generally those rules were actually strictly observed wherever it was possible to do so.

A few special comments should be appended in regard to more recent constructions of binocular instruments. We need not go into the mechanical details, which are interesting simply from the standpoint of optical technique and their connections with physics. Moreover, we may speak chiefly of the types of instrument made by the Zeiss firm, partly because the mechanical perfection of these models entitles them to a certain pre-eminence, but partly also because, being made to comply very closely with the theoretical requirements, they are, therefore, likely to be of special interest for us here. The instrument which may be mentioned first is the *binocular telescope* with enlarged base-line; which at present is being made by Zeiss in two forms, namely, "*Feldstechers*" (or field glasses) and relief telescopes ("*Teleplaste*").[1] In these instruments the enlargement of the apparent interpupillary distance (denoted by δ) is called its specific "plastic" effect. In the prism binocular field glasses it amounts to between 1.7 and 2.0; and in the relief telescopes to between 3 and 7. The total "plastic" effect is the product of the magnifying power of the instrument and the specific "plastic" effect (equal to $a\delta$, according to our notation). What is meant by it is that, if the binocular acuity of depth-perception is such that at the distance G the naked eye is just able to perceive the interval of depth X, the smallest interval of depth that can be perceived with an instrument of this kind will be $X/a\delta$.

One of the various useful applications of the principle of the binocular telescope that may be specially mentioned is in connection with instruments for the measurement of distance. In the stereoscopic

[1] As to the optical design of these instruments, see especially: Czapski, *Über neue Arten von Fernrohren.* Berlin, 1895.

range-finder (*"Telemeter"*) a scale etched on glass is insreted in the tube of each telescope approximately in the focal plane of the objective where the real images of external objects are projected. These scales are so adjusted with reference to each other that the binocular parallaxes of corresponding divisions are the same as those which objects would have at certain specified distances. Accordingly, when the scales are fused stereoscopically, the divisions appear like objects arranged in a definite sequence and in a definite manner with respect to the actually existing external objects, so that they form a *scale of distances* with which the distances of the various observed objects can be immediately compared. In viewing a terrestrial scene, the upper part of the field of view is generally empty, and so the scale-divisions are made to appear in this free space; the result being that the scale will be seen like a tape-line, so to speak, suspended in midair, and the mark, hanging apparently directly over an object, will give the distance of that object from the observer.

The binocular microscope made by Zeiss at Greenhough's suggestion (which has already been alluded to) is the main instrument of this type that needs to be mentioned here. Lately it has been designed and executed in several different forms intended specially for studying the skin and the cornea; being provided at the same time with suitable illumination-devices for this purpose.

The Palmos camera has been made for taking stereoscopic photographs with a base-line equal to the interpupillary distance.

For *viewing* such pictures Zeiss makes a stereoscope in which the oculars can be adjusted very conveniently with respect to the pictures and also with respect to their distance apart. Incidentally, this instrument can be used both for ordinary photographs and for transparencies.

We may mention here also the *"Verant" stereoscope.* If, for the purpose of getting visual impressions corresponding to the natural size of the objects, it is desirable that the distance between the camera lenses shall not exceed 65 mm, the stereoscopic pictures themselves must not be wider than this, that is, they should not be more than about 60 mm. If, therefore, the latter are not to correspond to a field of too small angular dimensions, it will be necessary to use photographic lenses of quite short focus; and then for the orthomorphous effect the images will have to be viewed at a correspondingly short distance, which involves using strong magnifying glasses. Ordinary glasses, however, do not give good images on account of the wide lateral extent of the field that has to be depicted in this case. In this respect the *"Verant" lenses* are made to satisfy the requirement of giving a good image when the eye is placed at a definite distance of 25 mm from the glass and turns through a considerable angle. The use of such lenses

is especially advantageous for viewing stereoscopic pictures like those that have just been mentioned.

The *stereo-comparator* described by PULFRICH[1] is mainly a stereoscope in which, just as in the stereoscopic range-finder, marks have been inserted in the optical systems used for making the observations. On one side the mark is fixed, whereas on the other side it can be moved to the right or left by means of a micrometer screw. Viewed stereoscopically, this mark appears like an object whose apparent distance can be varied by the contrivance above mentioned (principle of the travelling mark, as it is called). If photographic stereograms are viewed with such an apparatus, and the mark adjusted so that it appears to coincide with different parts of the figure produced by the stereoscopic combination, the distance of any such part can be calculated from it with great precision. HEINE[2] has tried to adapt this method to microscopic objects.

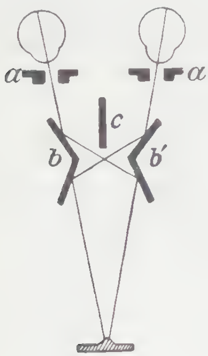

Fig. 80.

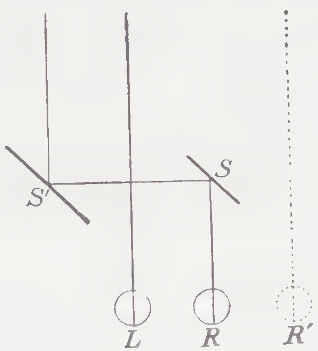

Fig. 81.

As these binocular methods of measuring distance are of exceedingly great practical importance in many ways, and since, on the other hand, it is found that their usefulness depends very largely on the personality of the observer, it is important to ascertain with certainty a person's capability in this respect. PULFRICH[3] has constructed a *test-chart* for this purpose. The chart is made on the order of a stereogram and shows a large number of objects, whose positions with respect to each other in the two pictures are more or less different. When they are

[1] *Zeitschr. f. Instrumentenkunde.* XXII. 1902; XXIII. 1903.—*Neue stereoskopische Methoden.* Berlin 1903.

[2] *Arch. f. Ophthalm.* LV. 1903. p. 285.

[3] *Zft. f. Instrumentenk.* XXI. Lately ZEISS has brought out another test-chart, depending however on the same principle, being different merely in detail.

correctly combined stereoscopically, they show up in a series of parallaxes of different amount, and it is possible to tell for what values of the parallax the observer can still perceive difference of depth with certainty.

Of the large number of instruments made for scientific or experimental purposes, two recent forms of *pseudoscopes* may be referred to here. In the one designed by EWALD[1] there are two pairs of mirrors b and b' (Fig. 80) by which the views of the object as seen by the two eyes are interchanged right and left, as is evident from the diagram. STRATTON's instrument[2] consists of a pair of parallel mirrors S and S' (Fig. 81). If the two eyes are at L and R, the right eye will evidently get a view of the object as if it were to the left of L, and so the instrument acts as a pseudoscope. If the left eye is transferred to R and the right eye to R', the impressions in the left eye will be changed in the same way, and then the instrument may be used as a telestereoscope. Since the virtual image of the eye situated at R not only lies further to the left but at the same time further back, the instrument can only be considered as correct for viewing very distant objects.[3]

[1] PFLÜGERS *Archiv.* CXV. 1906. p. 514.

[2] Psychological Review. 5. 1898. p. 632.

[3] ¶Some recent literature on the subject of binocular instruments is as follows:

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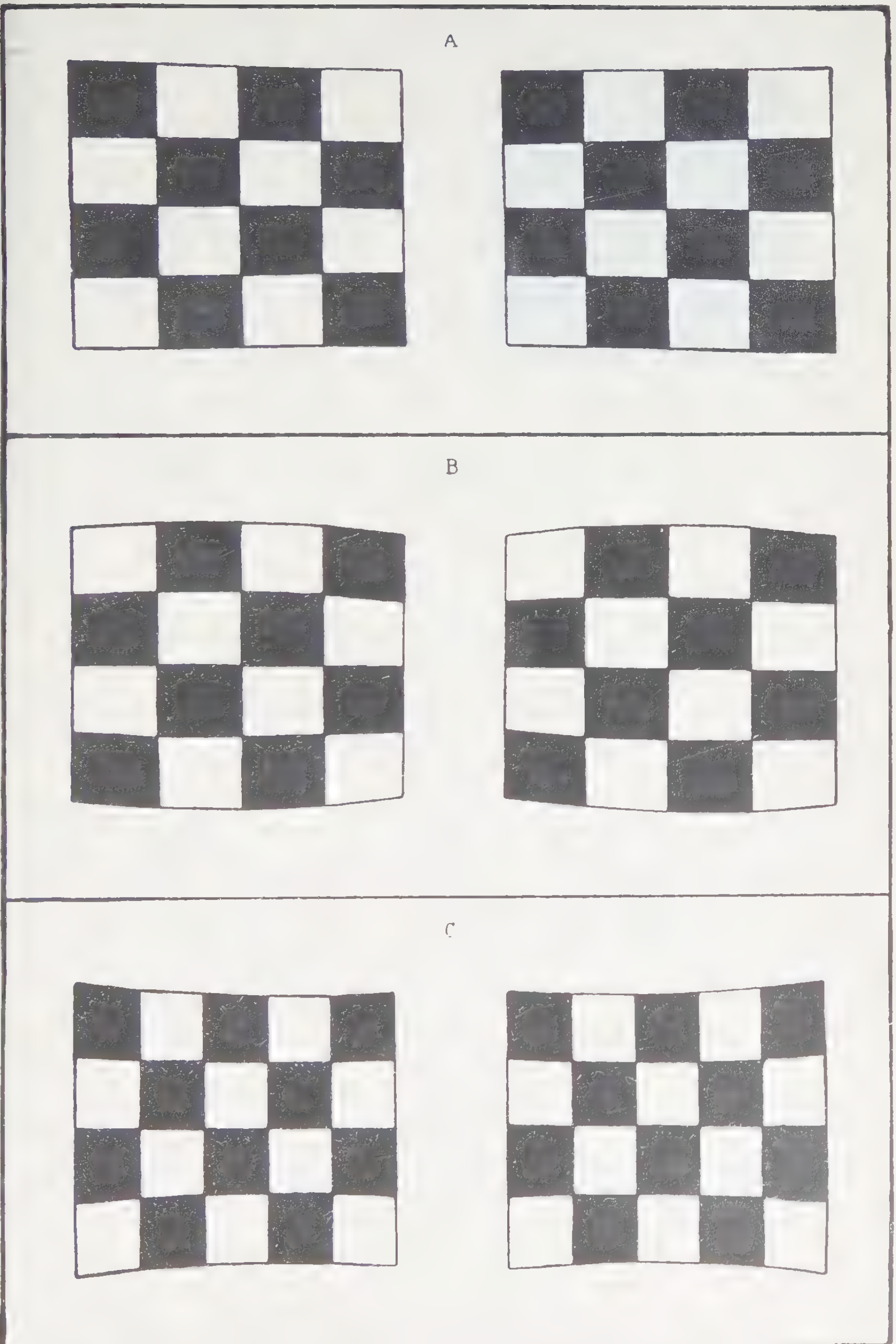
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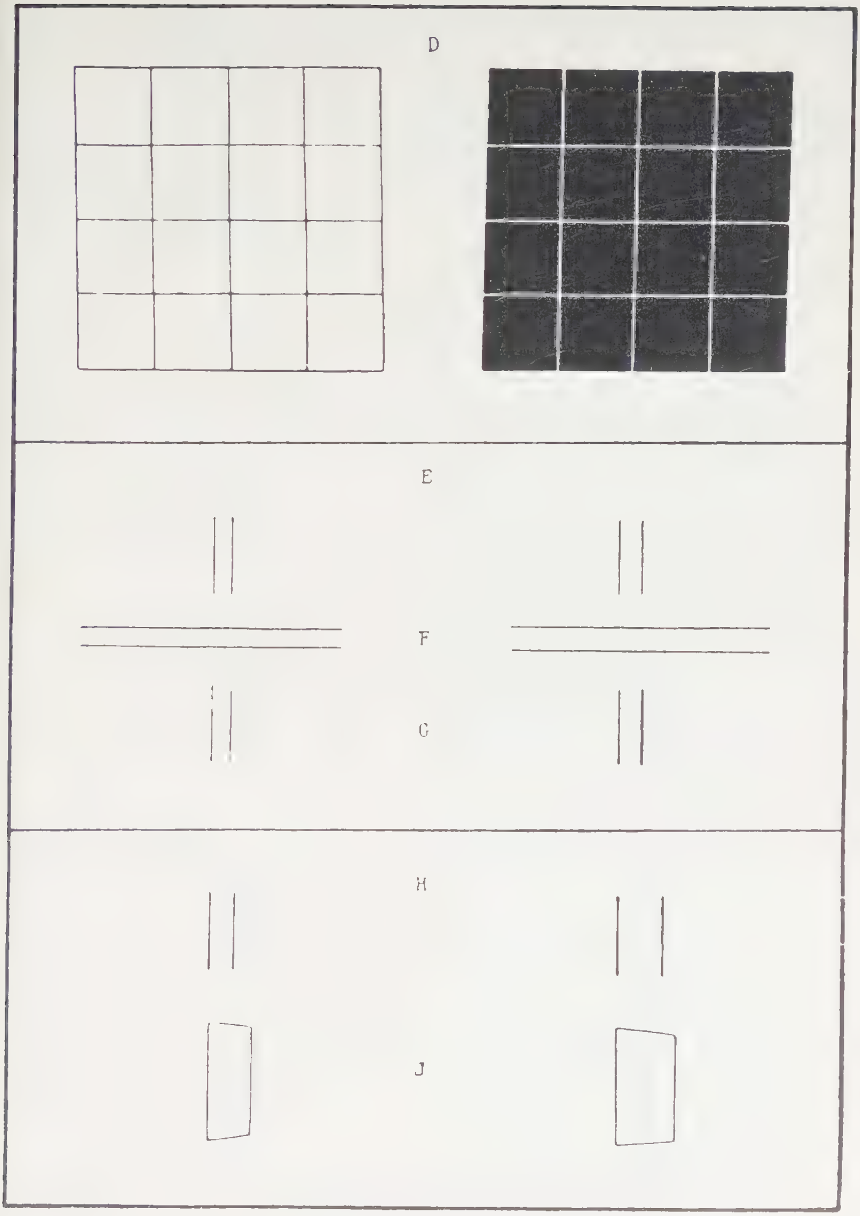

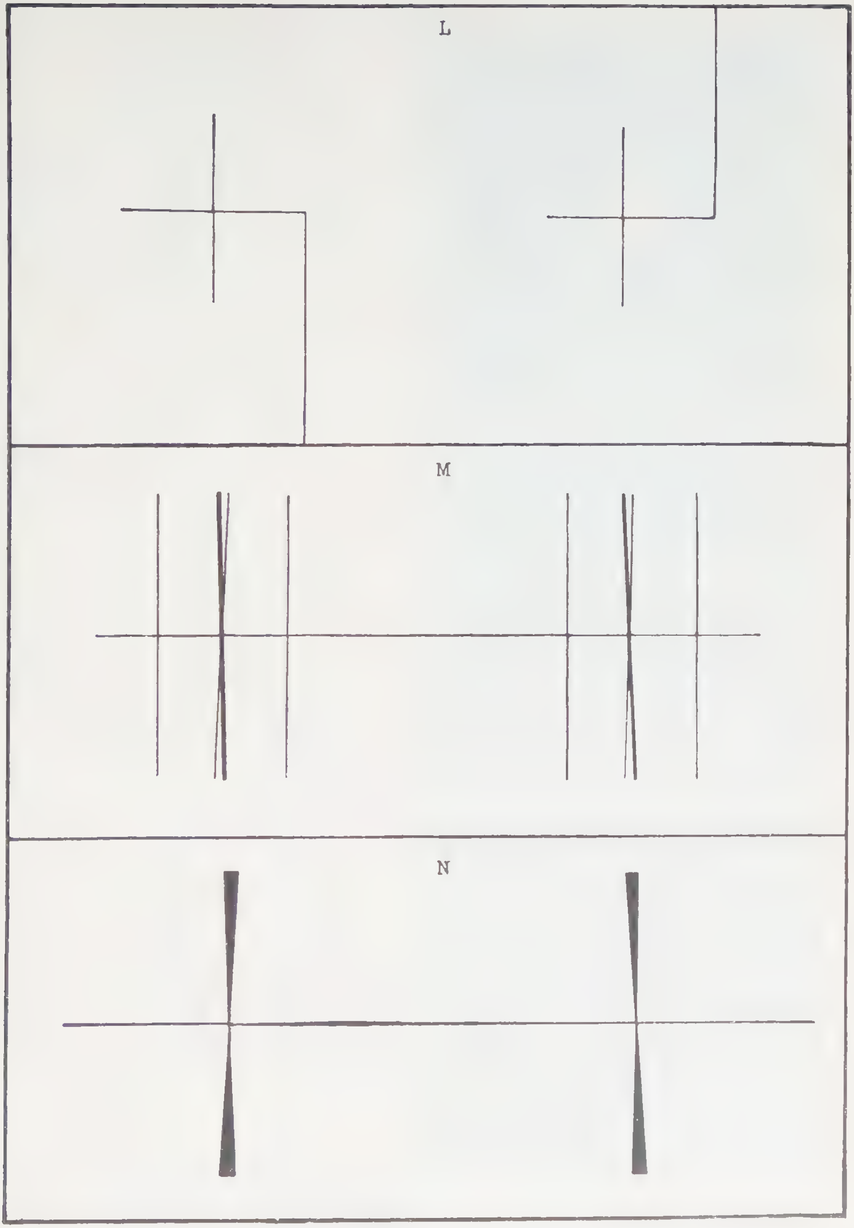
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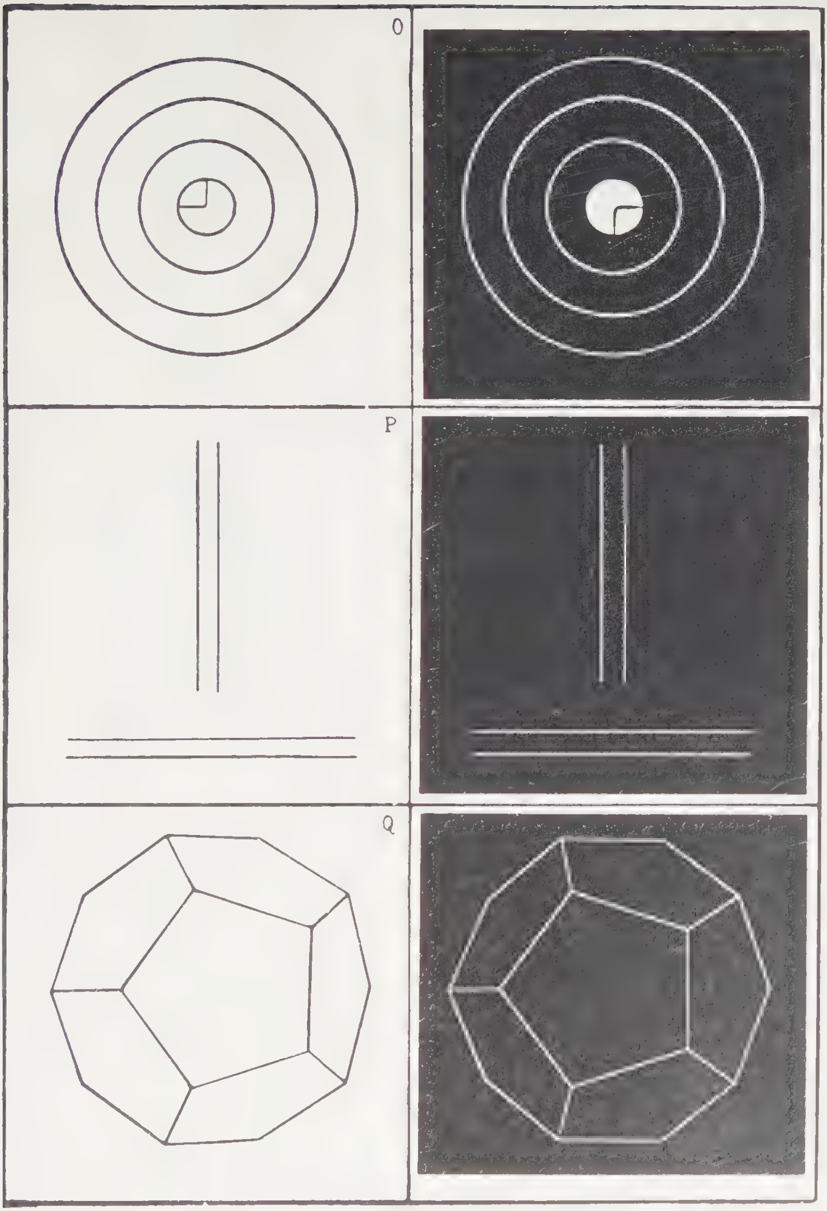

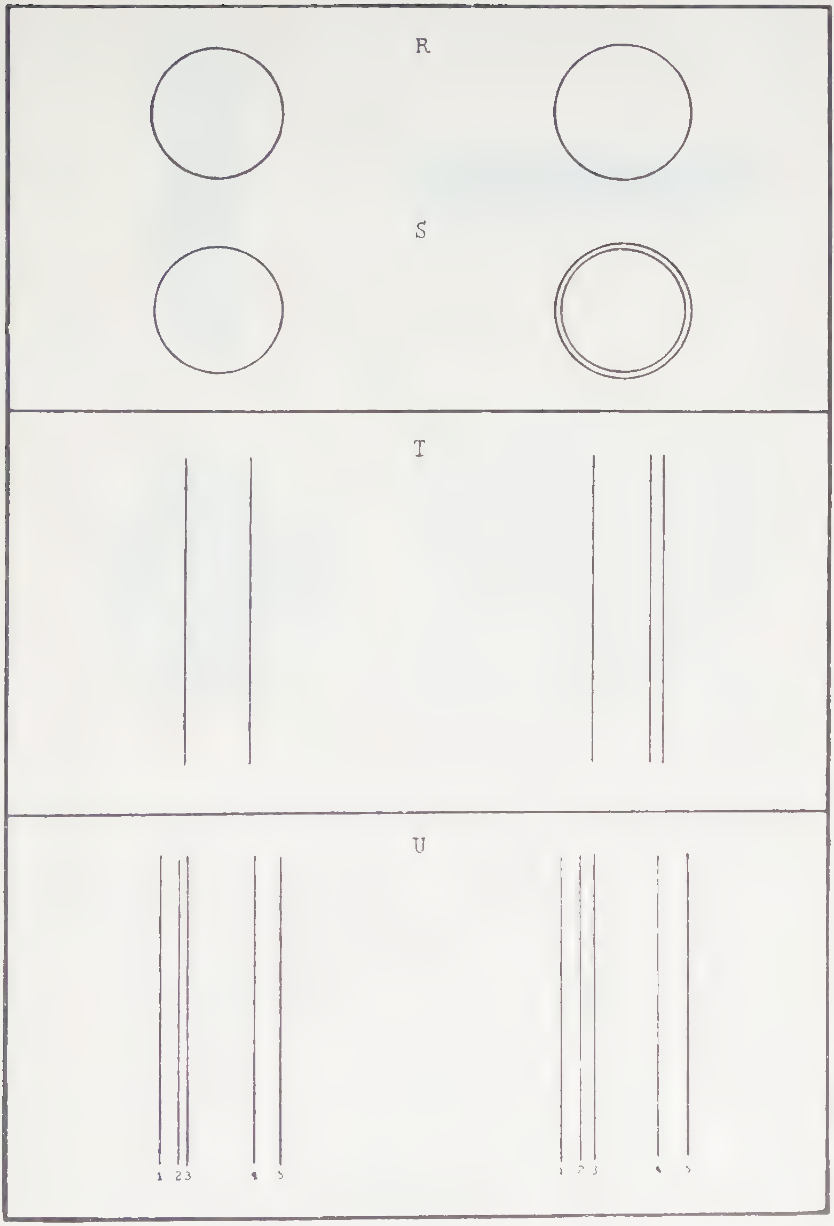
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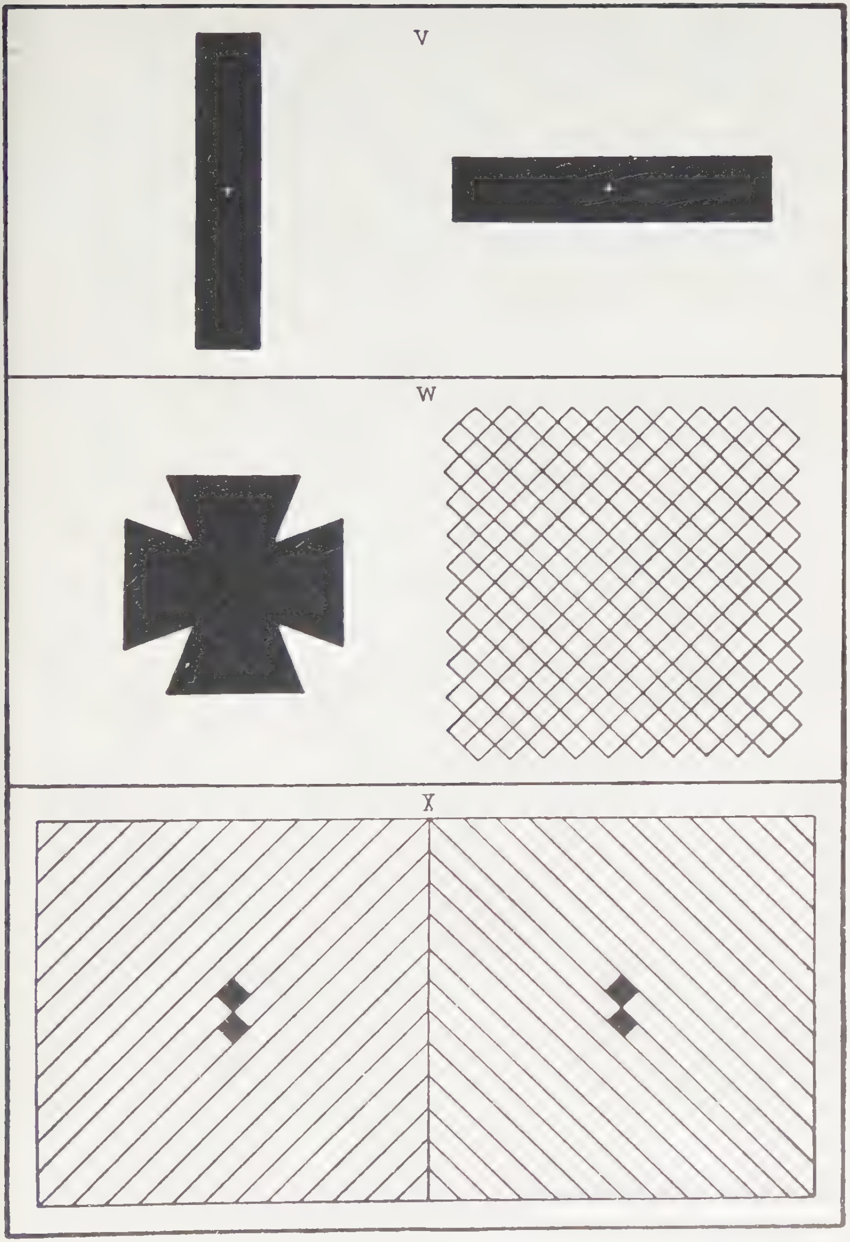
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